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GREAT BRITAIN.
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ON THE FORMATION OF AN ARTIFICIAL COMPOUND DISPERSIVE POWER IN A COMPOUND LENS, FOR CORRECTING THE FRONT LENS OF A REFRACTING TELESCOPE.

BY PETER BARLOW, F.R.S.

THE general principle of correcting the front lens of an object-glass for achromatism is very well understood; it consists only in making the focal lengths of the two lenses proportional to their dispersive powers. When this proportion obtains, and the lenses are placed in contact, the spectrum vanishes, and the object-glass is achromatic; but when it does not, a new spectrum is formed; and by considering the compound lens as a simple one, a new dispersive power is created, and this lens thus formed may be employed to correct another front lens. Also, as the artificial dispersive power thus created may be made either greater or less, in almost any proportion, than that belonging to either of the media by which the compound lens is formed, the correction may be made at almost any distance behind the front lens, and consequently the correcting compound lens may be made proportionally less than the front lens it is intended to correct.

When the compound correcting lens is used in contact with the front lens, it becomes the triple object-glass of Peter
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Dollond. When it is made a parallel glass for the mean rays, it agrees with the construction proposed by Mr. Rogers; and in all its intermediate states, it is the form which I have proposed for the naturally high dispersive power of the sulphuret of carbon.

It is certainly a little remarkable that the original idea of the triple object-glass had never led to this view of the subject; for it is obvious that if, instead of comparing the combined focus of the two crown lenses with that of the flint, we compare the combined focus of the two back lenses with that of the front, or the two front with that of the back lens, we arrive at a new artificial dispersive ratio of the kind above alluded to, but the author of this ingenious arrangement, having had in view only the reduction of the spherical aberration, seems to have lost sight of the other advantages he might have derived from the same principle.

In my endeavours to supply the place of flint glass by the sulphuret of carbon, I was in some measure forced upon that step which Mr. Dollond had failed to take, by finding it necessary to open the lenses; and this construction immediately recalled to Mr. Rogers an idea which he had formed some time before of that principle, which he has since published. But neither this gentleman nor myself, any more than the inventor of the triple object-glass, seems to have had any idea that we had each penetrated by different roads upon the borders only of an extensive field of uncultivated practical optics, which, if duly explored by well directed experimental researches, cannot fail of producing many valuable results.

While we confine ourselves to crown and flint glass, the range is certainly rather limited: if we employ the high refractive glass of Mr. Faraday's manufactory, it is considerably increased; but with the sulphuret of carbon it can be so far extended as to make the entire correction for colour, in an object-glass of any diameter, in the eye-piece of the telescope only. Mr. Coddington has, indeed, remarked, in his highly valuable 'Treatise on Optics,' that my correcting lens must be considered as a part of the eye-piece of the telescope; but this has reference only to its theory: what I allude to above is to be understood also in a practical sense; as it is possible to pro-
duce the entire achromatic correction in a tube one-eighth only of the length of the telescope, and of one eighth of its aperture.

In order to illustrate the above view of the subject, let

\[ f + \delta f = \text{the focal length of the red ray.} \]
\[ f = \text{ditto mean ray.} \]
\[ f - df = \text{ditto violet ray.} \]

In any positive lens, let also

\[ f' + \delta f', f', f' - df', \]
represent the same quantities in another lens, having a different index and dispersive power, where \( \delta, \gamma' \) denote the dispersions of the red ray in the two media, and \( d \) and \( d' \) those of the violet ray. In most cases we consider \( \delta = d \), and \( \gamma' = d' \); it is not so, however, in all cases, and I propose therefore at present to give this generality to the notation.

Confining now our investigation to one side only of the spectrum, as for example the red, and considering the second lens as negative, we have, when the two are in contact,

\[ \frac{1}{f + \delta f} - \frac{1}{f'} = \frac{f'(1 + \delta') - f(1 + \delta)}{ff'(1 + \delta + \gamma' + \delta \gamma')} = \text{reciprocal focus of the red ray, and} \]

\[ \frac{1}{f} - \frac{1}{f'} = \frac{f' - f}{ff'} = \text{reciprocal focus of the mean ray.} \]

Consequently

\[ \frac{ff' - (1 + \delta + \gamma + \delta \gamma')}{f'(1 + \delta') - f(1 + \delta)} = \frac{ff'}{ff'-f} = \text{length of the coloured focus; and this therefore, being divided by the mean focus, gives} \]

\[ \frac{(1 + \delta + \gamma + \delta \gamma')(f' - f)}{f'(1 + \delta') - f(1 + \delta)} = \frac{ff'}{ff'-f} = \text{dispersion of the compound lens, which after reduction, and denoting this dispersion by } \Delta, \text{ becomes} \]

\[ \frac{f' \delta (1 + \delta') - f \gamma' (1 + \delta)}{f'(1 + \delta') - f(1 + \delta)} = \Delta \ldots \ldots \ldots (1) \]

And here it is obvious,

1. If \( f' : f' : \delta + \delta \gamma' : \gamma' + \delta \gamma' \)
we get \( \Delta = 0 \), and it becomes the common double achromatic object-glass; in which \( \delta \gamma' \) being very small, the proportion is generally given as
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\[ f : f' :: \delta : \delta' \]
as stated in the first page of this paper.

2. If \( f = f' \)
we have \( \Delta = -1 \), which is Mr. Rogers' correcting lens.

3. If \( f : f' :: 1 + \delta : 1 + \delta' \)
the denominator vanishes, and \( \Delta \) is infinite.

And between these limits lies an immense range of dispersive powers, almost entirely unexplored; and out of which a well directed course of experiments could not fail of eliciting many valuable practical results.

If the lenses are both positive, the expression becomes

\[ \frac{f'd(1+\delta') + f'd'(1+\delta)}{f'(1+\delta') + f(1+\delta)} = \Delta. \quad (2) \]

In this case \( \Delta \) can never become zero, but it must necessarily fall between the two values of \( \delta \) and \( \delta' \), and will consequently be less than the greater of the two.

The above deductions, it will be observed, relate only to the red ray, but by everywhere changing \( \delta \) into \(-\delta\), and \( \delta' \) into \(-\delta'\), they apply equally to the violet ray. The expression (1) therefore, for the violet ray, with a positive and negative lens, is

\[ \frac{fd'(1-\delta)-fd(1-\delta)}{f(1-\delta)-f'(1-\delta')} = \Delta; \quad (3) \]

and with two positive lenses it is

\[ \frac{fd'(1-\delta)+fd(1-\delta)}{f(1-\delta)+f'(1-\delta')} = \Delta. \quad (4) \]

The former, of course, becomes zero when

\[ f : f' :: d - dd' : d' - dd', \]
which, rejecting as before \( dd' \), as inconsiderable, gives as in the red ray

\[ f : f' :: d : d' :: \delta : \delta'. \]

It follows from these formulæ and equations, that although in two media we should have

\[ \delta' = d', \quad and \quad \delta = d, \]
yet, as the combination of the red ray with the mean, would require the proportion

\[ f : f' :: (1+\delta') \delta : (1+\delta) \delta' \]
and the combination of violet ray with the mean, the proportion

\[ f : f' :: (1-\delta') \delta : (1-\delta) \delta' \]
we see at once, in this change of sign from + to −, the origin
of that imperfection which is generally designated the secondary spectrum; that is, the same proportion, which combines the red and mean rays, will not combine the violet and mean rays, so that there is necessarily a certain quantity of uncorrected colour when the lenses are in contact, and the dispersions equal on each side the mean ray, and which, with crown or plate and flint glass, is generally rendered worse by the inequality of the values of \( d' \) and \( d' \) in the latter; the inequality lying on that side which increases the evil: but I have shown in a paper in the 'Philosophical Transactions' for 1828, that by opening the lenses we have a certain command over the artificial dispersion which offers at a least chance of complete correction.

If, in the above expression, we make \( f' = pf \), and \( \delta' = m \delta \), it becomes

\[
\frac{p (1 + m \delta) - m (1 + \delta) \times \delta = \Delta.}
\]

In which \( \Delta \) denotes, as before, the dispersion of the red ray in the compound lens, \( \delta \) that of the red ray in the plate or crown lens, and \( m \delta \) that of the other medium, whether it be flint glass, Faraday's glass, or sulphuret of carbon: \( m \) and \( \delta \) are therefore given quantities, while \( p \) is assumable at pleasure within all practicable limits.

At present we have considered the lenses, whose compound dispersion has been ascertained as being in contact; let us now inquire into the circumstances of the compound dispersion due to two lenses placed at a distance from each other.

Let the focus of the first lens be \( nf \), and let the cone of rays from this lens be intercepted by a second lens at the distance \( f \) from the focus of the first; then \( n \delta f \) will be the coloured focus of this first lens, that is to say, there will be all the colour due to the focus \( nf \), but reckoning only from the place of interception the focus being \( f \), we shall have \( \frac{n \delta f}{f} = n \delta \) the dispersion of this ray, estimated from the second lens: we have, therefore, only to substitute \( n \delta \) instead of \( \delta \) in our former expression, and we shall thus obtain the value of the dispersion of the compound open lens, \textit{viz.}: in this case we shall have
for the red ray with a positive and negative lens,
\[
\frac{f' n \delta (1 + \delta') - f' (1 + n \delta)}{f' (1 + \delta') - f (1 + n \delta)} = \Delta; \ldots \ldots (5)
\]
for the violet ray,
\[
\frac{-f' n \delta (1 - \delta') + f' (1 - n \delta)}{-f' (1 - \delta') + f (1 - n \delta)} = \Delta; \ldots \ldots (6)
\]
for the red ray, both lenses being positive, we shall have
\[
\frac{f'' n \delta (1 + \delta') + f' (1 + n \delta)}{f' (1 + \delta') + f (1 + n \delta)} = \Delta; \ldots \ldots (7)
\]
for the violet ray
\[
\frac{f' n \delta (1 - \delta') + f' (1 - n \delta)}{f' (1 - \delta') - f (1 - n \delta)} = \Delta; \ldots \ldots (8)
\]
where it is assumed that the dispersion is equal on each side the mean ray, as is the case in crown or plate glass, which is the medium to which this principle is more particularly applicable; and here again, as we cannot completely answer both conditions, so as to combine the red and violet with the mean ray, we must, as in the case of the common double or triple object-glass, take a mean value of \(\Delta\) by rejecting \(n \delta\) and \(\delta'\) as inconsiderably; this reduces the expressions to
\[
\frac{f' n \delta \pm f' \delta'}{f' \pm f} = \Delta;
\]
the upper signs applying to the case of two positive lenses, and the lower to a positive and negative lens.

Taking this view of the subject, we must, in the combination of two lenses to correct the front lens of an object-glass, consider it rather as a combination of the two distant crown lenses, to be corrected by the simple flint lens, at least this will be the most convenient form for computation. For example, the focus of the compound lens will be
\[
f'' = \frac{ff'}{f + f'}
\]
its dispersion \(= \Delta\); and calling the dispersion of the flint \(\delta''\), we have only to compute its focus \(f''\) by the common analogy adopted in the usual form of telescope, \(viz\).
\[
\Delta : \delta'' :: f'' : f'''
\]
that is
\[
f''' = \frac{ff' \delta}{f' n \delta + f \delta'}.
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It would extend this article to too great a length to enter into the various cases that arise by giving different values to \( n \) and \( f' \) in this expression. I shall therefore, simply for illustration, take the particular case proposed by Mr. Rogers, in which it is assumed that \( \delta = \delta' \), the first and second lens being both crown, and \( f = f'' \), the foci of the second and third lens being equal: this gives

\[
f' = \frac{f f'' \delta''}{f' n \delta + f \delta};
\]

or

\[
f' = \frac{\delta'' - \delta}{n \delta} \times f.
\]

As an example, suppose the correcting lens to be placed at half the focal length of the front lens; and the focus of the front lens \( nf = 84 \) inches, and that the correcting lens is composed of crown and English flint glass, of which the relative dispersions are as 2 to 3, that is \( \delta = 2, \delta'' = 3 \) and \( n = 2 \), consequently, \( f = 42 \), we should have

\[
f' = \frac{3 - 2}{4} \times 42 = 10.25 \text{ inches},
\]

that is, the focus of each of the lenses forming the compound correcting lens must be 10.4 inches, the whole length of the telescope being 84 inches.

As another example, let the correcting lens be composed of crown and Faraday’s glass, of which the relative dispersions are as 10 to 19. Then we should have

\[
f' = 19 - 10 \times \frac{f}{2 \times 10} = 18.9 \text{ inches},
\]

that is, the focus of each of the correcting lenses must be 18.9 inches.

Lastly, let the correcting lens be crown glass and sulphuret of carbon, the relative dispersions being as 3 to 10. Here we should have

\[
f' = \frac{10 - 3}{2 \times 3} \times 49 = 49 \text{ inches}.
\]

We see thus the great advantage of a high dispersive power in this form of construction: for, in consequence of the depths of the curves required in the first example, it would be practically impossible to take \( n \) greater, or at least very little greater,
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than 2; with Faraday's glass, the focus being nearly double and the refractive power very great, the curves will be of much greater radii, and the principle would admit of some considerable extension; but with sulphuret of carbon it may, as has been stated in a former part of this article, be pushed so far as to enable us to make the whole correction for colour in the eye-tube only, that is, in a tube of 7th or 8th the focal length of the telescope.

We have seen, for example,—still taking the whole focus, 7 feet or 84 inches,—that when

\[
\begin{align*}
    n = 2, f &= 42 \quad \text{and} \quad f' = \frac{7}{6} f = 49 \text{ inches.} \\
    \text{If } n = 3, f &= 28 \quad \text{and} \quad f' = \frac{7}{5} f = 21\frac{7}{10} \text{ inches.} \\
    \text{If } n = 4, f &= 21 \quad \text{and} \quad f' = \frac{7}{4} f = 11\frac{1}{2} \text{ inches.} \\
    \text{If } n = 5, f &= 16.8 \quad \text{and} \quad f' = \frac{7}{5} f = 7\frac{3}{4} \text{ inches.} \\
    \text{If } n = 6, f &= 14 \quad \text{and} \quad f' = \frac{7}{4} f = 4\frac{3}{4} \text{ inches.} \\
    \text{If } n = 7, f &= 12 \quad \text{and} \quad f' = \frac{7}{3} f = 4 \text{ inches.} \\
    \text{If } n = 8, f &= 10\frac{1}{2} \quad \text{and} \quad f' = \frac{7}{3} f = 3\frac{1}{5} \text{ inches,}
\end{align*}
\]

which latter case even (having regard to the reduced aperture) is still as practicable as the flint and crown corrector at half the focal distance.

With Faraday's glass it has been shown, that with a focus of 84, and with \( n = 2 \), we have—

\[
\begin{align*}
    n = 2, f &= 42 \quad \text{and} \quad f' = \frac{3}{5} f = 18.9 \text{ inches.} \\
    \text{If } n = 3, f &= 28 \quad \text{and} \quad f' = \frac{3}{5} f = 8.4 \text{ inches.} \\
    \text{If } n = 4, f &= 21 \quad \text{and} \quad f' = \frac{3}{4} f = 4.7 \text{ inches.} \\
    \text{If } n = 5, f &= 16.8 \quad \text{and} \quad f' = \frac{3}{4} f = 3.02 \text{ inches,}
\end{align*}
\]

the case \( n = 4 \) being perfectly practicable. Thus it appears the greatest extension that can be given to this principle with flint glass is \( n = 2 \); with Faraday's glass, \( n = 4 \), and with sulphuret of carbon, \( n = 8 \): that is, in the first case, a plate lens of any aperture can be corrected with a flint lens of half that aperture; with Faraday's glass, by a lens of one-fourth the aperture; and with sulphuret of carbon, with a lens of one-eighth the aperture: and it will be observed that, between the values of \( n = 1 \) and \( n = 2 \), each of these mediums admit of unrestricted application—the case \( n = 1 \) in each being the triple object-glass, under that particular arrangement in which the focus of the first lens is equal to the focal length of the telescope.
All the above variations may be considered as belonging to one class, distinguished by the condition of $f'$ being equal to $f''$. Another class belongs to the case of $f''' = 0$, which is the form I have adopted in my fluid telescope. In this the range is more limited: we cannot here with any advantage make $n$ greater than unity, while we employ flint-glass. With Faraday's glass, we can make $n$ any number between 1 and $\frac{1}{3}$; and with sulphuret of carbon, any number between 1 and $\frac{1}{2}$. In the former class the focal power remains throughout the same as the length of the telescope; but in the latter it may be increased with the sulphuret of carbon to nearly double the length, and with Faraday's glass to $1\frac{1}{2}$ times the length. It is not, however, necessary to confine ourselves to either of these classes: we may make $f''' = qf'$, and take $q$, any number within practicable limits, greater or less than unity; and while $q$ is less than unity, the focal power of the telescope will be increased, and with $q$ greater than unity, it will be diminished.

Hence it appears that the refracting telescope, which has for about eighty years been limited to one particular form, will admit of an immense variety of untried forms, some of which seem to offer important advantages.

In all these cases, for example, the size of the concave correcting lens, which has hitherto set a limit to the dimensions of refracting telescopes, may be considerably diminished, leaving the aperture and light the same; or, which is equivalent with any given correcting lens, we may employ a much greater front lens.

In one class, also, we can, by increasing the focal power, diminish the length of the telescope; and in another, by diminishing the focal power, increase the light.

We have, also,—as I have shown in the 'Philosophical Transactions' for 1828,—at all events a control over the amount of the secondary spectrum, if not the power of destroying it altogether; and, lastly, the error arising from spherical aberration may be diminished almost without limit.

On the latter subject it may be asked, how (as we destroy or counteract the spherical aberration entirely in the common telescope) can it be farther diminished? To this I can only reply, that however completely we destroy it in our formulae,
it is still practically existing in the telescope; that is, we can only destroy the aberration for one index, whereas the perfection of the instrument requires it to be destroyed for three; an error, therefore, must remain, and it is for this reason that, with a given aperture, opticians are compelled to employ a certain length of tube, or rather, not less than a certain length; and, unfortunately, that length increases in a higher ratio than the aperture. The only remedy, therefore, is a reduction of the curvatures; and in some cases included in the above classes, these curvatures may be reduced to one-eighth of those of the common telescope of the same focal power; but to what extent we may in consequence reduce the length of the telescope, can only be satisfactorily determined by experiment.

Unfortunately, such experiments are attended with great expense; and that expense is rendered greater than is necessary, because many of our observers, however competent they may be to judge of the performance of a telescope, have too little inclination to examine theoretically and judge of principles; and therefore to gain their assent to any new form of construction, the instrument must be perfect, which of course requires the most perfect glass and the best workmanship, and necessarily creates great expense: whereas, if they were able or content first to examine principles only, the charge of such experiments would be much diminished; and when the best principle had been selected, then, and not till then, I would incur the expense of perfection.

It is exceedingly difficult, with such a field of inquiry before one, to say which of all the different cases should be selected as an individual test. I have, however, suggested a form, and the Royal Society have ordered the instrument to be constructed, which I have every reason to hope will be found highly advantageous. In this, the amount of spherical error,—or, which I consider equivalent, the amount of curvature,—will be less than one-eighth of what would belong to the common telescope of the same focus. The focal power will exceed the focal length, and the sum of all the refractions in the passage of the rays through the lenses, as well as at each individually, will be reduced to a minimum. These appear to
me to be important advantages; but there may be others equally important which have escaped me, and over which, had they occurred to my mind, I might probably have been able to exercise some control.

This impossibility of foreseeing all the bearings of an inquiry so completely untried as that which forms the subject of this paper, proves the necessity of an extensive series of experiments to elicit the most useful results; but such a series of experiments, particularly under the disadvantage to which I have alluded above, involves too great an expense to be undertaken at the charge of a private individual. At the same time, I must think that an extensive field of practical optics is here opened, which is highly deserving of cultivation.

ON THOSE BIRDS WHICH EXHIBIT THE TYPICAL PERFECTION OF THE FAMILY OF ANATIDÆ.

By WILLIAM SWAINSON, Esq., F.R.S., L.S., &c., &c.

ALTHOUGH natural history, from having been formerly pursued with an exclusive reference to specific differences, long merited its popular definition of being a science of observation, the attention now bestowed upon the generalization of facts, gives the hope that this science will soon become one of demonstration. It is to be feared, however, that in this eager desire to develope general laws, we sometimes overlook those means by which such results are to be obtained,—that we theorize, as it has been well observed, where we should analyze*, and decide upon the properties of a group, thought to be natural, before we have thoroughly investigated the group itself. The present generation of naturalists, in fact, are as much prone to fall into error from their over-anxiety to generalize, as were those of the Linnaean school, from an exclusive devotion to specific differences, and a total neglect of all the higher objects of the science. This passion for theory, among our own countrymen, dates its origin from the period when naturalists began to study the celebrated 'Hornæ Entomolo-

* 'It is the prevalent error of the day, among the naturalists, to attempt to generalize where they ought to analyze.'—Bicheno, Linn. Tr. xv. p. 489.
gicae; and when they perceived that the variation of animal structure could only be explained upon the principles there promulgated. But regardless of the warning which the talented author of that work has so often given to his disciples, that nothing is more easy than to form circles upon paper, 'provided we do not consider it necessary to prove them,' the fascination of his theory has been such, that some of his followers—forgetful of the discriminating caution of their master, and overlooking those tests which he has himself applied to the only genera he has perfectly analyzed—have ventured to pronounce certain groups to be natural and circular, which, upon closer investigation, prove eminently artificial. Certain forms are fixed upon as types of structure, before the principles which regulate such types have been either explained or discovered. An arbitrary standard of perfection is thus planted, around which are assembled such other species as more or less approximate to this fancied point of excellence; as this, however, is founded upon no one fixed principle of natural arrangement, every systematist thinks his own type better than that of his predecessor.

It appears to me, therefore, essentially necessary to the stability of any system of zoology professing to be natural, that the doctrine of types should be more deeply investigated: since, if such forms do actually exist in nature, we are justified in believing they must be regulated by certain general laws, which, when developed, will be conspicuous in all natural groups of animals. At all events, no arrangement can be demonstrative which is in any degree founded upon an assumption, like that of *types*, as they at present stand. It cannot be too often repeated, that all true knowledge of zoology rests exclusively upon analysis—upon a perfect acquaintance not only with the organization of a species, but with its habits, instincts, and natural history, properly so called: to fix, *à priori*, upon the type of a genus, or upon the divisions of a family, before the first has been demonstrated, or the latter analyzed, is manifestly erroneous: it is clearly beginning at the wrong end; and we do no more than tread in the footsteps of our predecessors, who first established certain rules of their own for making genera, and then referred every new
object to these arbitrary divisions. If this error led to so much confusion and to such forced combinations before any pretence was made to discover the natural system, it is fraught with peculiar evil in these days, when a glimpse of the harmonious plan of the Great Creator is the ultimate object of every zoologist. By fixing à priori upon a type, and arranging all the subordinate groups in their several supposed relations to that type, we not only incur the risk of commencing in error, but of continuing it through the whole contents of a very large division, and an entire order of animals will thus be inevitably thrown into confusion.

It must indeed be admitted that, with our present imperfect knowledge of the laws of nature, we must, in many instances, be content with theoretical deductions: but we should ever bear in mind, that these theories must be relinquished when opposed to a better knowledge of facts, and the right interpretation of such facts. It has been well observed, that every great discovery has originated from some preconceived theory which has struck open a new path of inquiry, enlarging and expanding the mind at every step. No one will, therefore, deny the importance, much less the legitimate use of theory, as an instrument necessary to develop the greatest truths: the objections are not to its use, but to its abuse. Every naturalist, in fact, who does not confine his attention to the sole study of species must have a theory or a system: it may be borrowed, or it may be new, true or false; still, so soon as he attempts combinations, he becomes essentially a theorist. He sorts and separates his species into parcels or groups, under some definite notions as to the characters they possess. If, after having done this, he proceeds to verify his first impressions by critically examining their correctness, or, in other words, by analyzing his groups, he makes that just and legitimate use of theory which is allowable: but, on the other hand, if he contents himself with investigating the details of one of his groups, and, having discovered certain properties belonging to it, proceeds to apply these results to the remainder, taking it for granted that further analysis is unnecessary, he then invests theory with an importance to which it is not entitled. So
long, indeed, as the world of science is distinctly informed that
the arrangement of the one is the result of analysis, and that
of the others is theoretical, no mischief is done: on the con-
trary, the doubts which would be expressed upon the latter,
particularly by an able and candid writer, will frequently lead
to important discoveries. At all events, we should know how
to apportion our confidence; which would certainly be greater
upon that group which had been analyzed, than upon those
which rested upon mere theory. The inventor of an artificial
system has no occasion to lay his reasons before the public;
but those who propose natural arrangements are imperatively
bound to candour upon these points. Since no name, how-
ever great, no style, however persuasive, or no theory, how-
ever captivating, can be of the least weight when unsupported
by analysis.

The consequences which have resulted from this anxious
desire to generalize, are no where more conspicuous than in
the existing arrangements of the Anatidæ; a family of birds
to which our attention has been recently directed, as form-
ing a considerable portion of the ornithological collections made
by Dr. Richardson, and described in the second volume of
the 'Northern Zoology,' now almost ready for publication.
This family, whose geographic range is chiefly in the temperate
and arctic latitudes, has long excited the attention of European
ornithologists. The species are numerous, and the modifica-
tions of form so many, that no one group of ornithology of
equal extent, except, perhaps, the Falconidæ, has been so
much divided into genera and sub-genera. Upon the value of
these minor divisions, there is, of course, much diversity of
sentiment, the inevitable consequence of an opinion almost
universal among naturalists, that nature knows no other defi-
nite distinctions than those which separate species. The truth
of this, however, has already been questioned*; and it will be
my object, upon this occasion, to prove that the views of Mr.
Macleay, in regard to his definition of the term genus, (as
exemplified by him in those of Scarabeæus and Phaneæus,) are

substantially correct, and, that a genus, so interpreted, is a definite group.*

The most superficial observer, on looking to the family of the

ANATIDÆ, or Ducks,

under which he will include the geese and swans, must be struck by the remarkable shape and structure of the bill, totally different from that of all other birds. This, in fact, is the only group in the aquatic order wherein the bill is very considerably dilated in its breadth, and of a texture unusually soft. In addition to these, a third, and a very important character is discerned: the cutting margins of the bill are provided with numerous transverse lamellar plaits, so much developed in some species, as to project beyond the bill; thus assuming an analogy to the teeth of quadrupeds. This analogy, however, is more imaginary than real, since these appendages are destined for a very different purpose. The feet, although in general short, are adapted to more than one purpose, since they are not only used for swimming and diving, but for walking. The adoption of this structure is in admirable unison with their natural habits, and with the station that Almighty Wisdom has ordained them to fill in the great empire of Nature. The Gulls feed indiscriminately upon marine animals, whether living or dead: they are the purifiers of the waters, as the Vultures are of the land. The Pelicans and the Penguins derive their support from those large fish which the more feeble Gulls can neither capture nor swallow, while the Terns skim the ocean in search of small fish which rise to the surface. But the inconceivable multitudes of minute animals which swarm, as voyagers assert, in the northern seas, and the equally numerous profusion inhabiting the sides of rivers and fresh waters, would be without any effectual check upon their increase, but for a family of birds destined more particularly for that purpose. In the structure, accordingly,

* Strange as it may appear, not one of Mr. Macleay's disciples have adopted the views of their master on this highly important question. The definition of a genus, given in the Zoological Journal, vol. iii. p. 97, &c., is diametrically opposed to that of the Hora Entomologicae. The one is founded upon abstract reasoning, the other upon demonstration.
of the Ducks, we see all these qualifications in the utmost perfection. By means of their broad bill, as they feed upon very small and soft substances, they capture at one effort considerable numbers. Strength of substance in this member is unnecessary: the bill is therefore comparatively weak, but great breadth is obviously essential to the nature of their food. As these small insects, also, which constitute the chief food of the Anatidae, live principally beneath the surface of the mud, it is clear that the bill should be so formed that the bird should have the power of separating its nourishment from that which would be detrimental to the stomach. The use of the laminæ thus becomes apparent: the offensive matter is ejected between their interstices, which, however, are not sufficiently wide to admit the passage of the insect food at the same time. The mouthful of stuff brought from the bottom is, as it were, sifted most effectually by this curiously-shaped bill; the refuse is expelled, but the food is retained. It is probable, also, that the tongue is materially employed in this process; for, unlike that of all other birds, it is remarkably large, thick, and fleshy. From being so highly developed it must be endowed with an unusual degree of sensation; and, indeed, a very exquisite sense of taste must belong to any animal which has to separate its food from extraneous substances, without deriving any assistance in the process from its powers of sight: against this deficiency Nature has wisely provided, by heightening and increasing the senses of taste and touch. I am acquainted with no family of birds where this organ is similarly formed, excepting that of the Psittacidae. The tongue of the Parrot, as every body knows, is so thick and fleshy as to resemble that of man. It is, however, much shorter and less delicate than that of the Duck; and, although endowed, in all probability, with sensations somewhat similar, the more immediate purpose of its structure is very different. Those who are familiar with the manners of Parrots, in their native regions, well know that they feed not only upon soft fruits, but upon others of the hardest texture. The seeds, for instance, of the numerous palms of Brazil are the most favourite food of the Macaws; and the thousands of Parraqueets swarming in tropical America always prefer nuts to fruits. This, in fact, is clearly evinced by the
great power of the bill. A thick and strong tongue thus becomes necessary, not so much for taste as to assist the parrot in turning round the nut that is to be cracked; it aids the efforts of the bill, supports the fruit in a steady position, and assists in turning it to a more vulnerable part.

It is, therefore, to the formation of the bill, as more particularly connected with the peculiar habits assigned by Providence to this family, that we must look for its typical distinctions; no other of its characters, I apprehend, can be selected, since the rest are common with several other aquatic families. There is nothing peculiar in the diving of Ducks, since the dab-chicks do the same; or in their frequenting both land and water, for so do the Gull family. If, therefore, this line of reasoning be just, it follows that we must esteem that form to be pre-eminently typical of the whole family, which exhibits these peculiarities of the bill in the highest state of development. Every ornithologist will consequently point to the Shoveller Duck as a fit representative of the

**GENUS ANAS**;

and as a form which differs from all others found in Europe by the uncommon breadth of its bill, and by the delicacy and great development of the projecting laminae. We have had frequent occasion to remark*, and to demonstrate the truth of the observation †, that such birds as are typical of a very compre-

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* Encyclopaedia of Geography, now in the press.
† See 'Northern Zoology,' vol. ii.

Vol. II. Aug. 1831.
hensive division, enjoy an extent of geographic range far above all others. The Shoveller Duck of Europe is not only found from the northern regions to the table-land of Mexico *, but is stated to inhabit the Coromandel coast, and other parts of India †; while another species, precisely of the same form, is recorded as a native of Australia ‡. The geographic distribution, then, of the true Shovellers may be termed universal. But among these broad-billed Ducks of the southern hemisphere, we find a very remarkable modification of form; the breadth of the bill and the length of the laminae are nearly the same, but the edge of the upper mandible, instead of being smooth, as in the European species, is furnished with a thin membranaceous skin, which considerably projects, and hangs down somewhat like a wattle on each side. For this form, hitherto uncharacterised, I now propose the name of

MALACORHYNCHUS,

and I shall view it, for reasons hereafter stated, as a sub-genus. The bill of the European Shoveller is flexible; but in this group it is much more so. One species, described by authors under the name of the soft-billed Shoveller, can scarcely exhibit this debility more remarkably than another which is now before me: it came from the same country,

* Specimens communicated to me by John Taylor, Esq., F. R. S., &c., from this locality, differed not from those brought home by Dr. Richardson.
‡ New Holland Shoveller, l. x. p. 313.
and seems to be undescribed. Guided by the same views, we next inquire what other ducks present us with the projecting laminae of the Shovellers; or where we shall look for the gradual diminution of a structure so important to these birds. This diminution we find in the sub-genus, **CHAULIODUS**, of the 'Northern Zoology,' founded upon the well-known Gadwall duck, a bird so repeatedly described, that it is surprising how any part of its structure should have escaped observation. It is, however, certain that this bird makes as near an approach to the Shovellers as any other yet known. The form of the bill, indeed, is no longer spatulate, or perceptibly broader towards the end; but the laminae of the upper mandible are still very fine, distinct, and more numerous than those of any other form subsequently mentioned, for they project a full tenth of an inch beyond the margin. The tail now begins to be lengthened, and, in a new species from Africa (*C. Capensis*), which I have recently received, is so much attenuated, as to evince an evident affinity to the pin-tail duck, forming the sub-genus **DAFILA**, of Dr. Leach. Nature has now so far receded from the typical form, that one of the chief peculiarities of that structure is nearly lost, and another considerably modified. The laminae of the upper mandible, which, in the *Chauliodus strepera* (Sw.), are so much shorter than those of the true shovellers, and are still more abbreviated in *C. Capensis*, become almost concealed by the margin of the bill in the bird now before us. The most
striking characteristic, therefore, of the genus we are now considering has nearly disappeared, precisely in that form which is furthest removed from the type. But the shape of the bill, although essentially modified, has not undergone a total alteration: its breadth towards the tip is not only as great as at the base, but is even more dilated; so that, in this respect, it resembles the Shovellers more than the Gadwalls, while it differs from both in being higher at its base, considerably more lengthened in proportion, and much more convex throughout.

It assumes, in short, a semi-cylindrical form, the end being particularly obtuse and slightly dilated. The precise point of junction between the Pin-tails and that group which was known to the ancients by the name of

BOSCHAS,

has not yet been explained. Under this subgenus we comprehend all those ducks usually denominated Teals, together with the Mallard, long domesticated in our poultry yards. As this is by far the most numerous group, so it exhibits a greater diversity of form among the species. They are all, however, characterised by a bill longer than the head, whose breadth is equal throughout; it is sometimes, indeed, a little dilated, but never contracted at its tip, while the laminae of the upper mandible are entirely concealed by the margin of the bill. The neck and the tail, which in Dafila are both considerably lengthened, are much shorter in this group, which is further distin-
guished by the brightness and beauty of plumage observed in nearly all the species.

On comparing the bill of the common Teal with that of the Pintail, we see a close affinity between the two forms. But as the tail of the first is so much developed, in comparison to that of the Teal, it becomes essential to discover, if these sub-genera actually followed each other in nature, what species united them more closely. By the uniform liberality of the zoologists attached to the British Museum, and more particularly J. E. Gray, Esq., I am now enabled to do this. The beautiful *Anas (Boschas) formosa*, Sw., or Baikal Teal of methodists, is precisely a bird which intervenes between these two sub-genera. Essentially a Teal, it differs from all others I have yet seen in the superior length of its tail, the feathers of which are a full inch longer than the under covers*; while the convexity of the bill, from being greater than in the common Teal, establishes its close approximation to *Dafila*.

Proceeding thus by analysis, we find several foreign species which may be either called Teal or Ducks. The *Boschas Javensis*, Sw., is more especially a bird of this description. It is closer allied to the mallard than to any other of the group: this is indicated by the more depressed form of the bill, and the white collar round the neck; the nape also is very conspicuously crested, a peculiarity found in no other group of the

* In *Anas (Boschas) crecca*, the tail is so short, that the under covers reach almost to the tip of the middle tail feathers.
Mr. Swainson on the Typical Perfection

genus. To this and to the curled tail of the tame duck, we shall presently advert.

Having now reached what appears to be the typical form of *Boschas*, we see that nature, as usual, again departs from it. The bill of the Mallard is throughout more depressed than that of the common Teal. This depression, in fact, from being greater than that of the Gadwall, or of the Pintail, obviously assimilates more to the Shoveller. The affinity, however, appears remote, since the laminae of the Mallard are concealed, while those of the Shovellers are conspicuously projecting. If, therefore, the affinity was immediate, it could only be demonstrated by a species having the bill of the common Duck but with the laminae projecting. Now such a species is actually the blue winged Teal of North America, in which these laminae project nearly as much as in the Gadwall, while the upper mandible exhibits that peculiar sinuosity towards the base which is seen in no other ducks besides the Shovellers. If this affinity required any further support, it is placed beyond doubt by the fact mentioned in the *General History of Birds,* that the plumage of the New Holland Shoveller, excepting the white facial crescent, is precisely the same as that of the blue winged Teal,—the very bird which thus unites the subgenus *Boschas* to that of *Anas,* and completes the circle of the whole group.

Zoological circles, however, if founded in nature, rest upon much better testimony than mere opinion. I have attempted to prove, in the forthcoming volume of *Northern Zoology,* that the variation in animal structure is regulated by certain
fixed laws, to the test of which every group thought to be natural must be brought. I shall therefore now proceed to demonstrate the accuracy of the foregoing observations, condensing them into the following tabular form.

GENUS ANAS.

Bill longer than the head, depressed nearly its whole length. The base not enlarged, the tip very obtuse; the laminae of the upper mandible generally projecting. Hinder toe not dilated, short; claws short, thick. *Fig. 2, a.*

1. Typical Group.
   - Bill spatulate, simple; laminae considerably projecting. *Subgenera.*
   - ANAS. Lin.

2. Sub Typical Group.
   - Bill spatulate, furnished with a lobed membrane; laminae considerably projecting. *Malacorhynchus, Sw.*

3. Aberrant Group.
   - Bill of equal breadth, projecting laminae short, slender, acute, crowded. *Chauliodus, Sw.*
   - Bill more cylindrical, lengthened; tail long. *Dapila, Leach.*
   - Bill depressed, of equal breadth; laminae distant, obtuse, and generally concealed; tail short. *Boschas, Antiq.*

On proceeding to trace the ANALOGIES of this genus, I am aware that it would be more satisfactory to compare it with the other groups of the same order and family. But this could not be done without assuming the correctness of the groups themselves, since the natural divisions of the Natatores, and the sub-families of the Anatidae, rest, at present, upon mere opinion;—they have been predicated, but not proved. I must, therefore, content myself with tracing how far the foregoing series partakes of those analogies which belong to the different groups of perching birds; whose circular affinities I have elsewhere attempted to demonstrate. This comparison, indeed, will be much more difficult than the former, since the strength of the analogies between two given groups is always in proportion to the proximity of these groups in the great scheme of nature. It would scarcely be necessary to advert to this obvious truth, did I not apprehend that some writers, who have as yet given but a superficial consideration to the subject, will pronounce these comparisons altogether fanciful. This, however, is not precisely the question. No one
who lays claim to the title of naturalist, practical or scientific, in these days, can deny the difference between analogy and affinity; still less would he be bold enough to argue, that the whole of creation is not a book of emblems, in the construction of which there must be a plan, a harmony, and a design, perfect and consistent in all its parts. If, then, analogies exist, as distinct from affinities, they must be universal. They may, indeed, be so clear and immediate, as to strike every one with a conviction of their truth: or they may be so disguised or remote, as to lead shallow reasoners to pronounce them fanciful; more particularly as of late much reason has been given, by the speculations of certain continental writers, for such imputations. That, however, which is fanciful, can never be twisted into regularity; for the moment a theory is found to explain facts which have never been explained by any other, it ceases to be fanciful.

Having stated thus much, I shall proceed at once to compare the Shovellers, the pre-eminent type of the genus, and of the whole family of Anatidae, with the Conirostres, that being the typical division of the order Insessores, or perching birds. Both agree in being the types of their respective circles, and in their wide, if not universal, geographic distribution. For what particular office the singular lobed membrane of Malacorhynchus is intended, must remain at present unexplained; but every principle of just reasoning authorises us to conclude, from analogy, that it is intimately connected either with the nature of the food or the manner of capturing it, peculiar to these birds; it is remarkable also, that this membrane is situated precisely at that part of the bill which in dentirostral birds is occupied by the tooth; both, in fact, are appendages; and the triangular membrane of these ducks is shaped much in the same way as the tooth of the Shrike. The colour of a species now before me strengthens this analogy, since it is banded all over with those black stripes which so particularly distinguish the American Thamnophilinae, or bush-shrikes. We may then fairly liken the sub-genus Malacorhynchus to the Dentirostres.

In the Gadwalls we have a very peculiar plumage: it is generally grey, and always dull. The British species is dis-
of the Family of Anatidae.

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tinguished from all other ducks of this division by its flying
with great rapidity. This has been observed by Wilson (Am.
Orn., viii. p. 121), who adds the important fact, that 'it is a
very quick diver.' Bewick also remarks, that 'these birds shew
themselves expert in diving as well as in swimming, for the
instant they see the flash of the pan, they disappear, and dive
to a distant secure retreat.' Now as this power of diving is
not one of the characteristics of the typical Ducks, and is much
more conspicuous in the Gadwall than in any other, the fact
can receive no other explanation than by supposing this sub-
genus to represent the order Natatorse, and consequently the
Fissirostres. The former are the dullest in their plumage,
and the most aquatic in their habits; while the latter, which
includes the Swallows, are the swiftest fliers in the whole circle
of ornithology. Precisely as is the Gadwall in its own group.
The pintail Duck, independent of its pointed tail (a charac-
ter which it shares with the Widgeons, and the sub-genus
Harelde, Leach) is remarkable for its long neck, and for its
narrow lengthened bill. Now every ornithologist is aware that
all such birds as have these parts greatly developed, belong
either to the order of Waders or to the tribe of Tenuirostres.
The long necks of the herons, the curlews, the cranes, in short
of nearly all the Grallatcres, incontestably prove this; and if
we look to the Humming and other suctorial birds of the
tenuirostral type, we see that both groups are more especially
characterised by these marks*.

There now remains but one division of the genus to be
tested, and this must be compared both with the Scansores
and the Rasores. Let us look then, to those peculiarities
which especially distinguish the common duck, the Anas (Bos-
chas) domestica of Linnaeus. First, its bill, from having the
laminae so short as not to project beyond the margin, is more
entire than any other. Secondly, it appears to be that pecu-
liar species which has been endowed by Almighty Wisdom
with a disposition favourable to domestication. In every cli-

* Montague, whose writings are replete with original and important facts, was
the first to record that the male Pintail undergoes an annual change of plumage.
Now this is one of the strongest peculiarities of the order Grallatcres and of the
Tenuirostres. In these groups, and these only, is this annual change almost
universal,
mate and in every country, where man assembles around him the mute companions which live and propagate under his care, the Mallard, almost exclusively, is the only Duck which is seen in this state. How many attempts have been made to domesticate others, and how completely, for all practical purposes, have they failed? Thirdly, there is a very elegant and peculiar development of tail in this species, and several others of the same group have lengthened crests, at the hinder part of the head. Further, this bird is not only more terrestrial in its habits than any we have here noticed, but differs from all these in frequently constructing its nest at 'some distance from the water,' and even in high trees and towers*. In several species the under plumage is regularly and beautifully spotted; and in others the tertiary feathers are richly ornamented and curved in an unusual manner. Although the Teals are generally considered the smallest race of ducks, yet they are not the typical examples of this particular group; while the Mallard and the other flatter-billed species are, unquestionably, the largest in the whole circle. I have thus brought together, under one view, every peculiarity, whether of external form or of habit, that can possibly be selected as in any way peculiar to the Mallard, when viewed in reference to that particular group with which it is here associated; we shall now see that every one of these facts illustrates, in the most complete manner, the analogy of this sub-genus to the rasorial and the scansorial groups.

The order Rasores is remarkable for birds having an entire bill: it comprehends the peacock, the turkey, the pheasant, and the fowl: all those land birds, in short, which seem set apart for domestication by man. In this assemblage, also, we find the most beautiful and singular development of the tail, the most elegant crests, and the most decided partiality for living on the ground. In this order, likewise, we have the most striking examples of a spotted plumage, witness the pea-

* 'Many instances are recorded of the common duck depositing her eggs at a considerable height from the ground: one, mentioned by Mr. Tunstall, was found sitting upon nine eggs on an oak, twenty-five feet from the ground.—The author of the "Rural Sports" also records an instance of a duck taking possession of the deserted nest of a hawk, in a large oak.'—Montague, Orni. Dict. Suppl. Many other instances are mentioned in the popular compilations.
cocks, the guinea-fowl, and the whole family of pheasants; while the plumage of many are lineated in a manner precisely similar to that of the different species of Teal. It is among rasorial birds, that we perceive that extraordinary beauty and elongation of the tertial feathers, which is so conspicuous in this particular group of ducks, and both comprise the largest birds yet discovered in their respective circles. This analogy, in short, which can hardly be rendered more complete, explains, also, the striking and apparently anomalous habit of the wild duck building so often in the hollows of trees, and in similar situations, since this habit more particularly belongs to the whole tribe of Scansores, which corresponds to, and represents the order of Rasores.

Having now illustrated the whole of these inferior divisions, I proceed to offer the following table as the result.

**GENUS ANAS. ANALOGIES.**

<table>
<thead>
<tr>
<th>Orders of Birds</th>
<th>Tribes of Perchers</th>
<th>Typical characters</th>
<th>Subgenera</th>
</tr>
</thead>
<tbody>
<tr>
<td>INSESSORES</td>
<td>CONIROSTRES</td>
<td>Typical in their respective circles</td>
<td><strong>ANAS</strong></td>
</tr>
<tr>
<td>RAPTORBS</td>
<td>DENTIROSTRES</td>
<td>Bill dilated into a lobe or tooth</td>
<td><strong>MALACORHYNCHUS</strong></td>
</tr>
<tr>
<td>NATATOES</td>
<td>FISSIROSTRES</td>
<td>Pre-eminently aquatic, or flying with great rapidity</td>
<td><strong>CHAULIODUS</strong></td>
</tr>
<tr>
<td>GRALLATOES</td>
<td>TENUIROSTRES</td>
<td>Bill and neck lengthened, nostrils long</td>
<td><strong>DAPILA</strong></td>
</tr>
<tr>
<td>RASORES</td>
<td>SCANSORES</td>
<td>Head crested, margin of the bill entire</td>
<td><strong>BOSCHILAS</strong></td>
</tr>
</tbody>
</table>

Each of these three columns, it will be observed, forms a circular group, of which the two first have long been before the public; and although their analogy with the types of form in the genus *Anas*, are of necessity remote, they are nevertheless unquestionable. When we consider, in fact, the great dissimilarity between the groups here compared, we can only feel astonishment that they possessed any one character in common. It would, indeed, as I before stated, have been better had the subgenera of *Anas* been compared with the groups belonging to its own family, but this would have far exceeded the proper limits of my paper. On a future occasion I may possibly take up another portion of the subject. External structure and natural habits are thus proved to be in perfect unison with each other, and both combine to furnish another proof that the system
of Nature is essentially a system of types and symbols. Seeing
then that this system can never be developed without the aid of
those matter-of-fact naturalists,—those true students of nature,
who throw aside ponderous systems and observe facts, how
much it is to be regretted that the natural history of birds has
been so greatly neglected! But for the writings of Le Vaillant
and D'Azara, among foreigners, and those of White, Wilson,
Montague, and Selby, in our own country, the true naturalist
who sought to apply isolated facts to general truths, would
receive but little help from all that has been written on the sub-
ject*. The most trivial circumstance in the habits and eco-


the highest pinnacle of fame. The study of nature is as
diversified as it is vast, and requires to be pursued by different
modes, and by different capacities. Without the aid of the
systematist, or of the 'closet' naturalist, the whole book of the
creation would exhibit but a ponderous collection of isolated
facts, interesting indeed in themselves, but crude, uninviting,
and trivial to the philosophic inquirer. If natural history can
teach nothing more than that one bird built on the ground, and
that another constructed its nest upon a tree, it may be a
rational recreation, but it can never become a science.

In regard to the tabular disposition of the five sub-genera, or
types of form, given in the preceding pages, it will be expected
that I should say a few words, since it is at variance with the
mode of exhibiting circular affinities, adopted by that distin-
guished writer who first detected this arrangement. On this

* Anatomical facts, of course, are equally important with those of habit, and
in Mr. Yarrell we can now boast of an ornithologist whose labours in this depart-
ment are peculiarly valuable and important.
of the Family of Anatidae.

point, I must refer the reader to the ornithological volume of the 'Northern Zoology,' now about to appear, where he will find our peculiar views explained and illustrated. I have, indeed, chosen to enumerate, in both instances, the subordinate divisions of the aberrant group, but they are always viewed by me as forming a distinct circle of their own, the primary divisions of every natural group being considered as three, and not five. In the present instance, the three sub-genera of Chauliodus, Dafila, and Boschas, possess one common character, in not having the bill conspicuously dilated at its extremity; while their circular succession can hardly be questioned, when we find that the greatest modern reformers* leave the Gadwall and the Mallard in the same group;—these writers having overlooked the modification of the laminae, and passed over the difference in the habits of these birds, as not bearing upon the question.

The theory, that the Mallard is the typical representation of this family, has now, I trust, been thoroughly investigated, and demonstrated to be erroneous; nor can I consider the two circular arrangements† that have been made of the whole family, each apparently perfect, but essentially different, in any other light. They appear to me to be the result of abstract theory, and of a theory completely misapplied. On the other hand, I deem it but justice to the great merits of another ornithologist of our own country, to acknowledge the assistance I have derived from his highly valuable paper, on the trachaea of birds‡, and at the same time to declare that if there is any truth in his own inferences, drawn from internal structure, or in mine, resulting from attention to external form and habits, he has himself marked out the true circle of the Anatidae, so far as the British species are concerned, totally unconscious of having done so. There is, and there cannot be, but one plan of creation. In our efforts to develope this plan, we must, as Mr. Yarrell justly observes, 'combine ascertained habits, external characters, and anatomical structure:' and in proportion as we can do this, so may we assume that our arrangement is NATURAL.

* Dr. Leach, Dr. Fleming, Stevens, Vigors.
ON THE RELATION BETWEEN THE POLYHEDRAL AND SPHEROIDAL THEORIES OF CRYSTALLIZATION; AND THE CONNEXION OF THE LATTER WITH THE EXPERIMENTS OF PROFESSOR MITSCHERLICH.

By J. F. DANIELL, F.R.S.,
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THE Molecular Philosophy, which is the subject of the following paper, is situated on the very confines of human knowledge—on that ill-defined and misty line where the most exalted intellect must be conscious that its powers begin to fail. Towards this debateable region it is, in general, sound discretion in the practical philosopher not to advance too far. Within its confines an ignis fatuus has too often been mistaken for the light of truth, and high energies have been wasted which might have brought forth substantial fruit on firmer ground.

It is for this reason that the professors of the Royal Institution have ever acted most wisely in excluding from their lessons of chemistry the Atomic Doctrine, and have confined themselves to the exposition of the theory of Definite Proportions. Had they followed certain illustrious examples, which have not been wanting, of dogmatizing upon the number and weight of ultimate atoms, instead of developing the beautiful relations of chemical equivalents, chemistry, in the place of that captivating simplicity which its aspect now presents in our systems, would have exhibited that kind of pseudo-mathematical confusion, which, from this and other analogous causes, too often obscures the doctrines of other justly celebrated schools.

But although this recondite and mental philosophy should not be mingled up with practical science, there is no reason why it may not be studied apart: on the contrary; a more noble exercise for the faculties cannot be conceived than the speculations to which it leads, when bounded by a proper discretion. As science slowly advances we gradually raise our point of sight, and we may reasonably expect that our horizon will consequently extend; and objects, which before were but dimly discerned, will become more distinct, and our judgments
corrected of things in the farthest distance. A more extended view of this description of the ultimate structure of crystalline bodies appears to me to be afforded by the experiments of Professor Mitscherlich upon the expansion of certain bodies by heat; as an introduction to which, a connected retrospect of the two theories of crystallization, although presenting but little new, will, I trust, not be considered as misplaced.

The observation upon which M. Haüy founded his beautiful theory of the structure of crystals is well known. He took a six-sided prism of calcareous spar, and, in attempting to split it, he found that of the six edges of the superior base, three alternate edges only yielded to the blow, and that the division there took place at a certain determinate angle. The three intermediate edges resisted this division; but, in applying the same force to the inferior base of the crystal, the intermediate edges alone yielded. By following up this cleavage in the natural directions thus pointed out, the new-formed planes met together, and he at length obtained an obtuse rhombohedron of definite angles; which was further divisible in the direction of faces into, apparently, an infinite number of similar smaller rhombohedrons.

To this invariable solid he gave the title of the Primitive Form of calcareous spar, and he supposed it to be the form of its ultimate molecules; from aggregations of which, externally modified, according to geometric laws, he conceived all secondary forms of the same substance to be produced.

This conclusion seemed to derive much force from the observation, that any crystal of calcareous spar, of whatever form, carefully broken, may always be resolved into an infinitude of small rhombohedrons, and that this form persists to the utmost limit to which we can carry mechanical division.

The same observation is applicable to other substances; but the primitive form is, in many cases, peculiar to the substance examined: thus, for instance, a crystal of sulphuret of lead is resolvable by mechanical force into a number of small cubes, in the same manner that calcareous spar is resolvable into small rhombohedrons.

M. Haüy calculated the secondary forms of crystals, by decrements of particles taking place on different edges and
angles of the primitive forms. There is no difficulty in conceiving a compound cube made up of a large number of small cubes; but if we place, upon each of the six faces of the cube so formed, layers of cubic particles, decreasing each by a row of particles parallel to the edges, till a pyramid is constructed upon each, terminating in a single particle, the figure becomes converted into a dodecahedron with twelve equal rhombic sides. If the decrement take place upon the angles, instead of the edges, of the original cube, the figure is converted into an octohedron.

By decrements of more than one row of particles, and by intermediate and mixed decrements, it may be shown that an almost infinite variety of secondary forms may be constructed; any or all of which may be assumed by the substance to which the primitive form belongs. Figures of these various decrements are now so common in elementary works of science, that there is no occasion to present them here. So far our observations upon the mechanical properties of such bodies will go hand in hand with the hypothesis. Parallelopipeds, or six-sided figures, of the nature of the rhombohedron and cube, might attract one another by their similar sides, would form stable combinations, fill all the spaces which they occupy, and would yield to mechanical division only in the direction of their joints.

But there is a class of substances, which, affording the same series of secondary crystals as sulphuret of lead, yields to mechanical division in very different directions; and affords primitive forms of a very different character. This class of crystals is well represented by fluor spar.

If we apply the edge of a knife with a little dexterity to a cube of fluor spar, we shall find that its eight solid corners may be removed, and that the new-formed planes will coincide with those of a regular octohedron. We may go on separating slices from any of these faces, all of which may be split into acute rhombohedrons. Had observation rested here, no difficulty would have occurred in applying the hypothesis,—the acute rhombohedron would have been the primitive form—and the cube, the octohedron, and all the modifications of this series might readily have been produced by decrements
upon its edges and angles. But this rhombohedron, unlike that of calcareous spar, is not only divisible in directions parallel to its six faces, but may be split into two tetrahedrons and one octohedron,—the four solid angles again, if the two tetrahedrons may be split off, and two octohedrons will remain; and the octohedron may be divided into six smaller octohedrons and eight tetrahedrons. Thus the whole mass may be resolved into tetrahedra and octohedra; no one of which can we conceive so small as not to be again divisible in a similar manner.

Which, then, of these two solids is entitled to be considered the primitive form of the crystal? Neither of them can fill space without leaving vacuities; and we can scarcely conceive either of them forming an arrangement sufficiently stable to constitute the basis of a permanent crystal.

They may both be symmetrically arranged, so as to afford to the eye the external forms of the series of secondary crystals, which may be geometrically calculated from their various decrements; but they must be conceived to attract one another by their edges only; and the tetrahedral arrangements will be regularly interspersed with octohedral, and the octohedral with tetrahedral cavities. The following figures, which I never yet saw represented in works upon crystallography, exhibit the construction of the tetrahedron, the octohedron, and the cube upon each of these hypotheses.
The tetrahedral arrangement, in fact, represents the cavities in the octahedral construction; and the octahedral arrangement would exactly fill the interstices of the tetrahedral.

This appeal to the eye cannot be without its effect in producing a conviction that such arrangements, although symmetrical, must be unstable, and that they are contrary to all our ideas of the common powers of attraction in matter. It is so obviously impossible, according to all our experience, that solids of this kind should attract one another by their edges, and not by their sides, that we are compelled to adopt the unphilosophical expedient of recurring to an uncommon and unknown power; our ignorance of which will be but ill concealed, by conferring upon it the name of polarity, or some such indefinite, convenient term.

Another observation here occurs, which I never remember to have seen advanced, but which appears to me to be fatal to this hypothesis. M. Haiiy, in this ambiguous choice of a primitive form for fluor spar, and a vast variety of other crys-
talline substances, chose the tetrahedron with octohedral va-
cuities, rather than the octohedron with tetrahedral spaces, for
reasons which he has assigned. Now, if we refer to the figure
of the cube, constructed upon this principle, it will be observed
that, if ever there had been a power which could have thus
grouped together these particles, mechanical force would have
split the solid in directions parallel to the faces of the cube,
and not parallel to the faces of an octohedron; for in such an
arrangement each particle is in contact with one other par-
ticle, while in the second each is engaged by three; so that
the force of attraction must be greater in the latter than in
the former. We thus, in fact, demolish the foundation upon
which the whole superstructure is founded.

Dr. Wollaston * proposed to obviate the difficulty of the in-
tersticial vacuities in these tetrahedral and octohedral arrange-
ments, in a most ingenious manner. He suggested that the
whole difficulty would vanish, by considering the elementary
particles to be perfect spheres, and to assume that arrangement
which would bring them as near to each other as possible.

The relative position of any number of equal balls in the
same plane when gently pressed together, forming equilateral
triangles with each other, is familiar to every one; and it is
evident that if balls so placed were cemented together, and
the stratum thus formed were afterwards broken, the straight
lines in which they would be disposed to separate would form
angles of 60° with each other.

If a single ball were placed at rest upon such stratum, it
would be in contact with three of the lower balls; and the lines
joining the centres of four balls, so in contact, or the planes
touching their surfaces, would include a regular tetrahedron
having all its sides equilateral triangles.

The construction of the acute rhombohedron and octohe-
dron, on the same principle, is as simple as that of the tetrahe-
dron; and the following figures will illustrate the simplicity and
stability of the arrangement, and its perfect harmony with
the known laws of attraction, both in its construction and the
directions in which it would be disposed to yield to mechanical
force.

* Phil. Trans. 1813.
Dr. Wollaston next proceeded to inquire what forms would probably occur from the union of other solids most nearly allied to the sphere; and he showed that, by supposing the elementary particles to be spheroidal, many solids might be constructed which are well known to crystallographers.

By imagining the axis of the *elementary spheroid* to be its shortest dimension, a numerous class of well-known solids originate. By grouping together oblate spheroids, in the same manner as the spheres in the formation of the acute rhombohedron, the resulting figure will still be a rhombohedron; but the measure of its angles will be different, and will be more or less obtuse according to the degree of oblateness of the original spheroid. If the proportion of the axis be as 1 to 2.87, the rhombohedron will be that of *calcareous spar*.

If the degree of oblateness were as 1 to 2, a right-angled rhombohedron or cube would result. These solids would obviously be split, by mechanical force, in directions parallel to their faces.

If the elementary spheroid on the contrary were *oblong* instead of oblate, it is evident that by mutual attraction their centres would approach nearest to each other when their axes are
parallel, and their shortest diameters in the same plane. The manifest consequence of this structure would be, that a solid so formed would be liable to split into plates at right angles to its axis, and the plates would divide into prisms of three or six sides, with all their angles equal; as occurs in phosphate of lime, beryl, &c.

It is, however, a very singular circumstance, that the construction of the cube with spheres, upon the same principle as the octahedron and the other solids of that series, escaped the ingenious author of this hypothesis—a failure which, had it been essential, instead of accidental, would have rendered it as untenable as that whose defects it was intended to supply.

Dr. Wollaston was perfectly aware that the hypothesis must have appeared defective, if it had not included some view of the manner in which so simple a form might originate; the only mode which occurred to him of supplying this desideratum was, to imagine a mass of matter to consist of spherical particles, all of the same size, but of two different kinds, in equal numbers, represented by black and white balls: these, he suggested, might be arranged four and four above each other, as in the following figure, alternately black and white throughout: the distances of the centres of the black balls, being every way a superficial diagonal of the cube, are equidistant, and their configuration represents a regular tetrahedron; and the same is the relative position of the four white balls. Every black ball is thus equally distant from all surrounding white balls, and all adjacent balls of the same denomination are also equidistant from each other, and the whole might be conceived to be in equilibrio.

The experimental part of the investigation had hitherto been confined to the action of mechanical force; by the application
of which it was found that crystalline bodies may be cloven in certain determinate and constant directions, from which the planes of least resistance in the solid are easily determined; but this is not the only force by which their structure may be dissected. The lower degrees of chemical affinity may be applied, as I have formerly shown, (Journal of the Royal Institution, vol. i. p. 24,) more delicately to the same purpose.

An irregular mass of alum, which, although to the eye it exhibits no traces of crystalline arrangement, may easily be shown to possess as regular a structure as the best defined crystal of the same substance. Mechanical force will not avail us for this purpose, as regular cleavages cannot be detected in it; but if we expose it to the solvent power of water, at first the fluid acts upon the salt with so much energy as to overcome the cohesion of the solid in every direction alike; but, as the water becomes saturated, its power diminishes, and it is nearly balanced by that delicate modification of cohesion upon which crystalline structure depends. The consequence is, that the solid now yields to the solvent only in the points of least resistance, and the mass will present the form of octohedrons and sections of octohedrons, as it were, carved or stamped upon its surface.

The numerous forms which are thus dissected from the mass are arranged in a definite order, with regard to each other and the different faces of the mass; and the series which occur upon one face, and those which correspond with it, are never intermingled upon dissimilar faces. Thus in one direction the light will be reflected from the faces of octohedrons and sections of octohedrons all upon the same plane; and by turning the mass upon its axis, the same will be repeated at every quadrant of a circle. By gently inclining the mass, the reflection will next arise from right-angled parallelograms of every dimension, which are similarly repeated upon turning the mass upon its axis.

Now, by supposing the process of solution continued till the several planes intersected each other, it is clear that various modifications of the octohedron and cube would result, all necessarily referable to the same structure of particles in the original mass: and it is obvious that each of the almost infinite
external variety of solids which would be produced by the meeting together of the various facets thus presented to the eye must be derivable from one principle of internal arrangement. The octahedron therefore and the cube could not depend, in this instance, upon such opposite constructions as those represented in Dr. Wollaston's hypothesis. But we are not on this account driven to abandon the spherical atoms, for, however singular it may appear that this eminent philosopher should have overlooked the fact, the cube may easily be shown to be derivable from a structure precisely analogous to the octahedron, the tetrahedron, and the acute rhombohedron.

It has been already shown that by placing a sphere upon two opposite faces of an octahedron, we convert the solid into the latter figure: by placing one upon each face, it is as simply converted into a cube as in the following figure. Now,

![Diagram of a cube and a sphere.](image)

such a cube would obviously be divisible by mechanical force in directions parallel to the faces of the octahedron; because in those directions each particle is held by the attraction of three others only; while in the transverse direction each is engaged by four others; and this is the direction in which fluor spar yields. But now another difficulty occurs—this may be supposed to be a satisfactory account of the construction of the cube with octahedral cleavages, but how shall we explain the octahedron of sulphur of lead, which splits into cubic fragments? This is one of a numerous class of substances exhibiting the same phenomenon of the cubic fracture with the same series of crystals as fluor spar and others of the octahedral class. The theory will be worth little if it should be found applicable only to the latter.

If we suppose ten spheres endued with equal powers of attraction simultaneously exerting their powers upon each other, their forces would be most equally balanced in the cubic form, producing the compact and stable arrangements
Mr. J. F. Daniell on Crystallization.

represented in the last figure. If, from some predisposition of affinity, the particles of any solution should continue to combine in this definite proportion, a number of cubes would be formed; which, again attracting one another, would unite together side by side, according to the general laws which we have observed. A compound cube would be thus naturally constructed, and it is evident that mechanical force would resolve such a solid into a number of smaller cubes; for upon the planes of junction the spheres of one cube are only held to the spheres of another cube by the binary attraction of two particles for each other, while in every other direction each ball is in contact with three others at least.

Now, the first simple cube may be resolved into two similar, but irregular tetrahedrons; and if we suppose an octohedron

formed by a decrement upon the angles of the cube analogous to that in M. Haüy’s cubic system, these tetrahedrons would exactly fill the interstitial spaces; and an octohedral arrangement would be formed with precisely the same angles as the constructions which we have previously considered, but which would exhibit the phenomena of the cubic instead of the octohedral cleavage.
It would be easy to show how all the regular solids of the octahedral series, and their modifications, might be produced by either of these principles of arrangement; but enough has been premised to introduce the additional argument in favour of the spheroidal hypothesis of crystallization, which may, I think, be unexpectedly derived from the experiments of Professor Mitscherlich.

We have hitherto considered the arrangement of our atoms as due solely to their mutual powers of attraction—let us now contemplate it as the result of a balance of the attractive power of the atoms, and of the repulsive power of an elastic atmosphere, with which we may conceive each to be surrounded, and which will represent the repulsive power of heat. The atoms we suppose attractive of each other and of the particles of the hypothetical atmosphere, but the latter highly repulsive of each other.

Upon these postulates, each spherical atom would be surrounded by a stratum of equal depth in all its parts, uniformly distributed over its surface; which, preventing the actual contact of the particles, would nevertheless allow them to arrange themselves according to the laws of the predominant attraction.

We may suppose the figure $a$ to represent a section of the tetrahedral arrangement of spheres in simple contact; and the figure $b$ of the same spheres with their atmospheres; an arrangement essentially the same with regard to structure and external figure.

Any addition or diminution of the repulsive aura would cause the atoms to recede from or approach towards each other equally; and if we were to heat a solid so constructed, it would expand equally in all directions.

But what would be the case with the structure of oblate spheroids, instead of spheres?

In the first place, as the force of their attraction must, from
the nature of their form, be exerted with greater force in the
direction of their shorter axis than in that of their longer,—
taking for granted the two fundamental laws of attraction,
first, that all the particles of matter attract one another directly
as their masses, and inversely as the squares of their distances: secondly, that a body of any shape will attract a particle of
matter anywhere with the same force, and in the same direc-
tion, as if all the matter of the body were collected in its
centre of gravity,—it is clear that their repulsive atmospheres
will not be distributed in equal layers over their surfaces; but
will collect in greater depth above the shorter axis than the
longer; and the atom, with its atmosphere, will assume more
of the spheric form.

A solid crystal, therefore, thus constructed, must change the
measure of its angles with every change of temperature. This
is precisely what Professor Mitscherlich has ascertained to
happen with crystals of carbonate of lime and other substances
not crystallizing in the octohedral series.

He found that a rhombohedron of calcareous spar changed
the inclination of its planes to the amount of 8°.5 in the inter-
val between 32 and 212. As the temperature augmented, the
obtuse angles diminished; that is, the smaller axis of the
rhombohedron dilated more than its other diagonals, so as to
cause an approach to the cubic form.

In substances crystallizing in the octohedral series, he found
that the expansion was equal in all directions.

The mere inspection of the rhombohedron, made to represent
the primitive form of carbonate of lime, upon the spheroidical
hypothesis, is almost enough to produce a conviction that it
must expand and contract differently in the directions of its two
axes; and the theory might certainly have had the advantage
of anticipating an observation which tends so powerfully to its
support.

A new and interesting field of research has thus been opened
by this third method of disturbing crystalline cohesion, which
cannot fail of proving fertile of important consequences to the corpuscular philosophy.

There is a great and natural reluctance in the mind to give up an hypothesis ingeniously constructed, like that of M. Hauy, so as not only to present in a general point of view a great number of particular facts, but to enable us to reason from the known to the unknown, and actually to predict facts before trial; and it is from this cause, I think, that the spheroidal view of crystallization has not hitherto received all that consideration which it appears to me to deserve.

It is, however, by no means uncommon in physics to find two theories maintained as to the origin of natural phenomena, both of which cannot of course be the real laws of nature, but each of which will enable us to generalize and group the facts together with accuracy. I need only refer to the two theories of electrical phenomena and the two theories of heat.

When two hypotheses thus run parallel to each other, and each explains a great many facts in common with the other, such unforeseen evidence in favour of one, as I have been endeavouring to explain, is of the utmost consequence. But it is of scarcely less importance to show, if possible, the connexion of one with the other, which enables them to apply to so many circumstances in common.

Now it appears to me that this is not difficult with regard to the two theories of crystallization.

Those who have attentively studied the great work of M. Hauy must be well aware that he could not have been insensible to the difficulties which attended his system, when viewed as an account of the real process of nature; but, whilst in doubt with regard to this ultimate object of inquiry, he most philosophically adopted a temporary substitute for truth, which was capable of leading him by legitimate reasonings to conclusions in exact accordance with observations so numerous as fairly to embrace the whole range of phenomena which the theory was intended to account for.

The fact is, that, so far from insisting on the real existence of his primitive forms and integrant molecules, as many of his followers injudiciously have done, he very early in his reasoning points out the difficulty of the ambiguous choice, and of the vacant interstices; and shows that the whole purpose of
his calculations is answered by imagining the primitive form in all instances the paralleloipedons (or six-sided figures), which would result from the addition of the vacant spaces to the molecules.

To such imaginary forms he gives the name of *substractive molecules*, and he observes that 'the theory would not fail of attaining its principal object, if it were to stop at the paralleloipedons which the mechanical division of crystals first affords; and the species of anatomy which these paralleloipedons undergo, when we attempt to ascend to the true form of the integrant molecule, is an ulterior step, without which observation, rather than theory, would leave something to be desired. The paralleloipedon here represents the unity to which all the results of the theory may be referred; and it matters little whether or not, beyond this unity, there may be fractions formed of its subdivisions*.'

With regard to the crystals which derive their origin from the octohedron or tetrahedron, he remarks, 'We may consider the decrements which give rise to these forms as taking place by one or more rows of small rhombohedrons, with angles of 120° and 60°. Whether the solid parts of these rhombohedrons be octohedrons, which leave between each other vacuities of the form of tetrahedrons, or whether the contrary be the case, is perfectly indifferent to the theory, which considers here *nothing but the rhomboidal spaces, abstracted from the small bodies which occupy those spaces†.*

Now, the spheroidal hypothesis shows how these *abstract geometrical spaces* may be filled up more completely, and with a greater regard to the known laws of attraction, than by the method which suggested itself to the mind of the immortal author of the 'Treatise on Mineralogy,' the primary object of whose system is not affected by the change. The calculations founded upon his imaginary *substractive particles* will still furnish the key to the different series of secondary forms; and we establish, rather than upset, them by showing that they may be referred to a system of attractions which, when tested by the antagonist powers of heat, chemical affinity, and mechanical force, is found consistent with the observed phenomena in every particular.

* Haüy, Traité de Minéralogie, tome i., p. 97. † Ibid., p. 473.
ON THE ILLUMINATION OF THEATRES.

By ALFRED AINGER, Esq.

The important rank which dramatic representations have ever held among the amusements of all civilized people gives an interest to everything connected with the improvement of even their most subordinate auxiliaries. I have imagined, therefore, that an investigation of the existing methods of illuminating theatres, and a proposition for remedying some of their defects, may not be thought unworthy of a place in a scientific journal.

The first, and this is perhaps the most trifling objection to the present system, is, that it is obviously artificial. Although, ninety-nine times out of a hundred, the scene is supposed to be exhibited by natural light,—that of the sun or moon,—the greater part of the light which reaches the spectator is visibly derived from lamps or candles, which are so conspicuously and variously situated, as of necessity to come more or less within his angle of vision. By this means, not only is the illusion of the scene very much diminished, but its force and vividness, considered as a mere picture, are greatly impaired by the superior intensity of numerous radiating points, which overpower the light merely reflected from the actors and the scenery. A similar injury is occasioned by the unpleasant flickering of the foot-lamps above their screens, and by the excessive light they give to the highly-decorated proscenium, which thus becomes a brilliant and sparkling frame to a comparatively dull picture. The effect is something like what might be produced by hanging variegated lamps round the edges of a painting; and, if avoidable, is scarcely more judicious. The amazing force of the dioramic pictures is mainly owing to the adoption of an opposite course—that of subduing, as much as possible, everything extraneous to the ostensible objects. The central chandeliers, which have been introduced within a few years, produce, to a certain extent, the same sort of mischief, by the powerful light thrown on the ceilings, to which they are of necessity very near, that they may not interfere with the view of the upper spectators. This proximity of the ceiling to the chandelier, and its consequent lightness, are further objectionable, because the first causes the decorations
to be quickly covered with soot, and the second renders it visible; so that this part of the theatre becomes disgustingly dirty before the remainder is sensibly soiled. Nor can this be guarded against by the use of glass chimneys to improve the combustion of the gas, because they would be too inaccessible to be cleaned, and they would moreover be productive of danger to the frequenters of the pit.

In addition to the smoking and flickering of the foot-lamps, immediately between the spectator and the scene, they injure the sight by the currents of unequally heated air which they interpose, and which, by irregularly refracting the light, give a wavy and disagreeable appearance to whatever is seen through them. The effect of these lights on the performers is rendered evident by the obviously constrained aversion of their eyes, while the expression of the features is almost destroyed by the reversal of the shadows under which the face is usually and best seen. The figure suffers as much as the face from this inversion; and it becomes peculiarly inappropriate when viewed in conjunction with a scene where the shadows are evidently derived from a superior light.

A further objection to all the lights is, that being in sight of the audience, they cannot conveniently receive those attentions which it would occasionally be useful to bestow on them, and also that their combustion is rendered imperfect by the impurity and agitation of the atmosphere about them. Their immense consumption of air adds materially to the draughts, which too frequently prove prejudicial to the health of delicate visiters to the theatre; and the velocity which they lend to the general upward current through the roof, together with the rarefaction and impurity they impart to it, must interfere considerably with the transmission of sounds from the stage to that part of the audience which is below or but little above its level.

The mode of illumination which I am about to suggest will, if practicable at all, remove the whole of the objections I have enumerated. It will be sensible to the audience only through its indirect and intended effect, and it will give appropriate shadows to the performers. The light may be obtained under the best circumstances for securing good combustion; the
flames may be supplied with a steady current of pure air; they
will therefore burn with little smoke, and neither air nor smoke
will ever in any way become obnoxious to the audience. The
house may thus be preserved well ventilated and clean, while
the lights, by their perfect combustion, will be rendered eco-
nomical, and still more so by the opportunity of using appro-
priate reflectors to direct the rays towards the points at which
they are required. It is not an exaggeration to say that four-
fifths of the light at present employed are wasted in conse-
quence of radiation in worse than useless directions, or of
absorption (to use the common term) by imperfectly reflecting
surfaces.

I propose to remove the foot-lamps and the central chande-
lier, together with all the smaller lights round the circumference
of the house, and to substitute an illuminated dome, as shown
(in the plate) in section, fig. 1, and in plan, fig. 2. This dome
would, at its lower part, be formed into circular or octagonal
panels, the frames of which might be enriched and gilt, but the
panels themselves would be occupied by glass, behind which
would be lights of greater or less power, and to each a reflector
equal in diameter to the panel, and of greater or less perfect-
tion in regard both to form and materials.

In the drawing I have described three tiers of circular
panels, each containing thirty-six, or in the whole one hundred
and eight. To the quadrant immediately opposite the stage,
or from C to C in figure 2, containing twenty-seven panels, I
should apply the most powerful gas-lights* I could obtain,
and the most perfect parabolic reflectors. Their axes would
be in radii of the dome, which would be made such a segment
of a sphere, that those radii would point precisely to those
parts of the stage where the light was required. It may be
thought that light so directed would be too concentrated, or,
in the language of artists, spotty; but as the reflectors would
not average more than eighteen inches in diameter, and the
flames would have considerable magnitude, so much of the
source of light would be ex-focal, that each parabola would
supply a cone of rays sufficiently obtuse to mingle with those

* If it be desirable to employ the light of lime burning in oxy-hydrogen gas,
such an arrangement as is here proposed seems peculiarly adapted to the purpose.
of the adjacent reflectors, and blend the whole into one undistinguishable mass. If, for example, an eighteen-inch parabola be generated, having the distance between the vertex and focus three inches, and if the diameter of the flame in that focus be upwards of an inch, the cone of rays issuing from such a reflector will have at its apex an angle of more than twenty degrees. The light thus thrown on the performers would, I think, be greater than could be obtained from twenty-seven equal flames in the situation of the foot-lamps; for, if lines be drawn from the place of the foot-lights to the average position of the actors, it will be evident how extremely small a sector of the sphere of rays proceeds in an available direction; while, in the proposed arrangement, nearly every ray, with only so much loss as is occasioned by the imperfection of the reflecting surface, would be directed exactly to the required points.

The panels in the remaining three quadrants might be supplied with inferior lights and reflectors; and the glass would be ground, to diffuse the light over the house, and to improve the appearance of the dome when seen by the audience. The change from the ground to the unground glass would be visible only to a small part of the house; and though, as far as it goes, it would be a defect, it is not, I think, to be put in competition with its numerous and important advantages. It is not, indeed, impossible that the light would be found sufficiently strong to allow of the glass from C to C being slightly ground, by which every objection would be obviated. If this be not practicable, the defect would be seen only when expressly looked for, and would be merely a slight exaggeration of the effect which would be produced upon a similar dome exposed to the sun's rays, which might be nearly vertical on the one side, and wholly excluded from the other. The variations of light required upon the stage would be managed with much greater beauty and effect, than when a rising or setting sun is seen to be accompanied by the rising or sinking and smoking of a hundred artificial flames.

It will, perhaps, be doubted whether a light, such as I have described, would extend over a sufficient portion of the stage: such a doubt will exist only with those who are unaware how
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small a part of the light on the stage is derived from the foot-lamps. Whenever the performer is behind the line A A on the plan, he is lighted principally from the lamps which are numerous and powerful immediately behind the proscenium, at the points A A; from these also, and from similar lights concealed behind the side-scenes, the scenery obtains nearly all its light, receiving so little assistance from the foot-lamps, that I have the authority of one of the most eminent scene-painters for saying, that if sufficient light can be obtained for the performers, there would be no difficulty in regard to the scenery. I have given all the reasons which occur to me for thinking that it would be sufficient in quantity; and its average direction is indicated by the sword in the hand of the figure drawn on the section.

I have, therefore, no doubt that there would be very little difficulty in realizing the whole of the plan; and that, if realized, the result would be to add much to the comfort, convenience, and splendour of theatres—to give infinitely greater effect to the scene—and to do justice to the features and expression of the performers.

[See Note at the end of the Miscellanea.]

ON THE RED SOLUTIONS OF MANGANESE,

BY THOMAS J. PEARSALL,
Chemical Assistant in the Royal Institution.

THE crimson solutions obtained by the action of certain acids upon oxides of manganese, possess some remarkable properties, which have received only partial and unsatisfactory explanations. It has been hitherto supposed that an oxide of manganese existed, which was capable of dissolving in acids, to produce pink or deep red coloured solutions, but the precise state of oxidizement has not been agreed upon. The red oxide, deutoxide, and peroxide have each been selected as the one present*.

In consequence of experiments which I have made to dis-


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cover the cause of the colour, and the other properties of the red solutions of manganese, I am led to believe that they are not due to the presence of an oxide, but to manganic acid, which has not hitherto been suspected *.

These solutions are always strongly acid; they do not crystallize or form definite salts; the colours and tints are rapidly destroyed by deoxidizing agents, and the concentrated solutions, especially if obtained by means of sulphuric acid, are readily decomposed by mere dilution with water. The solutions have a peculiar odour, and exert strong bleaching powers, which effects have been supposed due to the presence of foreign bodies, rather than to the state of the manganese existing in the solutions. It was necessary, therefore, at first, to ascertain whether these are constant properties of acid red solutions of manganese, or whether they are owing, as supposed, to the action of chlorine derived from accidental sources.

Black oxide of manganese was repeatedly washed with hot distilled water, which gave no trace of muriatic acid when tested by nitrate of silver; then oil of vitriol, free from muriatic acid, was diluted with its weight of water and poured upon the oxide. In twenty-four hours the fluid had assumed a crimson colour, and after several weeks a very deep crimson solution was obtained, which, when diluted, produced no turbidness with nitrate of silver. A dilute solution of sulphate of indigo was powerfully bleached by this red fluid, and a stronger solution of indigo had its colour instantly destroyed, leaving only an amber tint. These bleached portions of fluid gave not the least indication of chlorine when tested; but a solution of indigo bleached by chlorine gave a precipitate with nitrate of silver.

Part of this red solution was introduced into a retort, the neck of which was dipped into a solution of sulphate of indigo, and the retort heated by a spirit-lamp; the indigo was not bleached, neither was there any trace of the odour of chlorine; the operation was continued until part of the contents of the retort had distilled over, the temperature having been raised to about 400°.

The retort was then heated with the neck immersed in a solution of nitrate of silver, in which no cloudiness occurred,

and the fluid in the retort still strongly bleached indigo; but when a small portion of solution of chlorine was added to the same crimson fluid, or to colourless protosulphate of manganese, and then heated in the same manner, chlorine was driven over, and rapidly bleached the blue liquor. If chlorine, therefore, had been present in the previous experiments, it would have been rendered evident by this arrangement.

In other experiments the crimson sulphuric acid solution was decomposed by being much diluted with water, the oxide separated by filtration, and the clear liquor distilled; half the fluid, however, passed over into the indigo without affecting any bleaching change, nor did the liquor in the retort bleach: if chlorine had been present in the solution, it would have remained after dilution.

Then pure hydrated protoxide of manganese, which had become brown by exposure to the air, was mixed with sulphuric acid, and a red fluid was obtained which possessed bleaching properties, but which, when diluted with water or heated with alcohol, lost all colour and all bleaching power: hence there is no evidence that chlorine is the bleaching agent; on the contrary, the bleaching power of the sulphuric solutions accompanies the coloured state of manganese, and it appears that these two properties are present or absent together.

As the red sulphuric solution appears to have been the only one referred to with regard to this power, I proceeded to examine whether other red solutions of manganese possessed similar properties.

In the process of triturating together binoxalate of potassa and peroxide of manganese, pointed out by Van Mons*, a crimson fluid is produced of great depth of colour; it is acid, and becomes colourless after some time, depositing crystals. I found that while red it bleached indigo very strongly, the action being accelerated by the addition of sulphuric acid.

This crimson solution lost its colour when heated in a retort; carbonic acid gas was evolved, which bubbled through the solution of indigo, without altering it, and the fluid in the retort, now clear and colourless, had lost its bleaching power.

* Quarterly Journal of Science, ix. 409.
When nitrate of silver was added to this crimson solution, an abundant white precipitate of oxalate of silver was produced, which was readily soluble in diluted nitric acid, and thus easily distinguished from the chloride of silver, for which it might be mistaken from its similar appearance; more especially as the oxalate, like the chloride of silver, is soluble in pure ammonia.

Hence it appears that these red solutions possessed bleaching powers in consequence of the peculiar state of manganese, and independent of chlorine*.

When pink or crimson solutions of manganese, supposed to contain red, deut, or peroxide, were compared with solutions known to contain manganesic acid, their similarity in some properties was so striking, that I was induced to suspect that manganesic acid alone ought to be regarded as the cause of the peculiar effects.

1. The varieties of scarlet, crimson, or purple colours belonging to manganesic acid, in different circumstances, may be imitated by those from sulphuric acid and oxides of manganese. 2. The red solutions of oxides are always very acid; manganesic acid is soluble and compatible with acids. 3. The red solutions by oxides and sulphuric acid bleach; manganesic and sulphuric acids, mixed, also bleach very strongly. 4. The crimson solution by the action of binoxalates upon oxides of manganese bleaches indigo; manganesate of potassa with binoxalates also does the same. 5. Both kinds of solutions are alike rendered colourless by the same deoxidizing agents. 6. Both kinds of solutions are subject to decomposition by mere dilution with water. 7. The sulphuric solution evolves a peculiar odour; manganesic acid in vapour has a similar odour. 8. The addition of certain metallic salts to the supposed solutions of oxides, and also to solutions holding manganesic acid, give similar appearances; and indeed the similarity of the two sets of compared

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solutions is so great as to offer the highest probability that their powers depend upon a common cause.

It will now be my endeavour to place clearly the experimental reasoning which supports this new view of these solutions; and, as the bleaching properties of the ordinary red solutions has been first brought into consideration, I will show that the same effects may be produced by manganeseic acid under the same circumstances.

A solution of manganeseic acid in sulphuric acid was moderately heated in a retort; the portion distilled over did not bleach, but the fluid, still red, bleached indigo instantly. If much sulphuric acid be present, the mixture may in this experiment be heated for some time, and sustain an elevated temperature before the separation of oxide.

Sulphuric acid was added to chamelion mineral, and produced a deep crimson solution of the red manganesate of potassa, that instantly bleached a strong solution of indigo. This red fluid heated in a retort evolved volatile matter, which destroyed the blue colour; but there was no trace of chlorine in the bleached portions of indigo when tested by nitrate of silver.

These experiments with the sulphuric acid fluids containing manganous acid, prove that this state of manganese produces the same results as the crimson solution. That the sulphuric acid was not essential to these effects was thus shown: by dissolving mineral chamelion in water it gave a deep green solution, which, when boiled, became deep red, and then bleached sulphate of indigo; the resulting fluid was unchanged by nitrate of silver: an aqueous solution of manganous acid produced the same effect.

The crimson solution obtained by the alkaline binoxalate and oxides of manganese is the only other particularly pointed out as supposed to contain the deutoxide of manganese; although acid, it is not so strongly acid as the sulphuric fluid: on adding oxalic acid or binoxalate of potassa to solution of green chamelion, the rich colours of manganous acid appeared. This deep crimson solution is almost identical in colour with the solution in the former oxalic experiment, and, like it, also bleaches indigo.
Solutions of green chameleon and binoxalate of potassa were heated in a retort; the fluid soon became colourless, but without changing the dilute solution of sulphate of indigo into which the neck of the retort was introduced: the colourless fluid was incapable of affecting indigo.

Manganesic acid, mixed with oxalic acid, very powerfully bleached indigo: neither oxalic acid nor the binoxalates have any bleaching power over indigo, but manganesic acid and some of its combinations possess this property.

The colourless oxalate of potassa and manganese, which remained in the retort after distillation, was rendered acid by sulphuric and oxalic acids; and then, upon the addition of manganesic acid, a clear red solution was formed, which had strong bleaching powers; but it lost this property upon becoming colourless, which it did in a short time. A very concentrated solution of red manganesate of potassa was added to another portion of the same colourless triple oxalate; the red colour was more permanent, and the bleaching still more energetic, than in the preceding experiments. These cases afford experimental evidence that manganesic acid is capable of reproducing the characteristic properties of a crimson solution supposed to contain a deutoxide.

I shall now submit that the known properties of manganesic acid and protoxide of manganese are capable of explaining the action of acids upon the various oxides of this metal. The existence of either the red, deut, or peroxide in these fluids appears to me to be an assumption:—of course, the protoxide must always be considered as present, according to the usual reasoning upon the relations of oxides to acids. The admitted action of sulphuric acid upon peroxide is to form the protoxide, and thus leaves the formation of the red solution by either the red or the deutoxide quite unaccounted for; therefore it may be advantageous to examine the changes which may be supposed, in order to produce either of these oxides; and, 1st, it may be assumed, that the acid reduces the whole of the peroxide acted upon to the state of red oxide, or deutoxide which forms a red solution; or, 2dly, the acid reduces part of the peroxide to the state of protoxide, and another part to the state of red or deutoxide, and both are in solution together; or,
3dly, the formation of these oxides may be accounted for by supposing the peroxide reduced to protoxide, portions of which become again oxidized by oxygen evolved from other portions of peroxide. Since the affinities of these oxides are admitted to be so much inferior to the protoxide, which, with acids, produces definite and permanent compounds, it is not to be expected, therefore, that either the red or the deutoxide should alone be produced; and in the second, which is the simplest case of the action of sulphuric acid, where, by the loss of one proportional of oxygen, a protoxide is produced to form the constant base of the sulphate, there is no reason to suppose that another oxide should be formed at the same time, and held in solution by the same acid, but with which it is admitted that it cannot form salts; on the third view, the effect of additional oxygen, combining with the protoxide, would be to produce another oxide, acknowledged to have much less affinity for the acid than the protoxide, which, in fact, is to suppose that a weaker could subvert a stronger affinity, and that the feeble indefinite combination resulting (so feeble that even water can destroy it) could hold the place of a strong, definite, and neutral compound with the same acid. Even admitting that protoxide of manganese could combine with oxygen to form a coloured state, this notion would certainly be in favour of manganesic acid. So I suppose that protoxide is present in the red solutions; and it will presently appear that manganesic acid and protoxide may be in solution together.

On the supposition heretofore entertained, that an oxide is the cause of colour in the red fluids, it has not been stated whether this oxide be alone or with protoxide; but it has been admitted that when a red solution by decomposition precipitates a dark oxide, that much protoxide remains behind in solution*. Then this precipitate evidently does not contain the whole, or the same proportion of metal and oxygen which existed in the red solution. It has also been considered that the colour was due to the oxide thrown down; and as no mode has ever been pointed out for separating protoxide only, it follows, that when the manganese is precipitated from a crimson

* ‘3 grains of peroxide were precipitated, and after the action of water potash threw down 27 grains of oxide.’—Phillips, Phil. Mag., N. S., v. 216.
solution by an alkali, it will consist of the mixed states in previous solution; and hence, because a brown or red oxide may be thus obtained, it cannot be admitted as a proof that this is the same state which existed in the solution; and, on the contrary, the same precipitated oxide when dissolved by acids being resolved into the same states as before, therefore a crimson fluid obtained from a red or brown oxide is, in itself, no proof that the same oxygenated state of manganese is taken into solution which was acted upon by the acid; so that there seems no satisfactory evidence of either the red, deut, or the peroxide in the crimson solutions of manganese. The constant presence of either the red or deut oxides seems irreconcilable with the fact, that every degree of oxidizement higher than the protoxide will afford red solutions; this, however, agrees with the production of manganesic acid.

It has often been remarked as a singular circumstance, that so small a quantity of the red or deut oxide should be capable of causing deep tints*; but these observations will accord with the fact, that a minute portion of manganesic acid can produce intense colour. The experimental formation and the theoretical composition of manganesic acid afford arguments in favour of its presence in the ordinary red solutions. Dr. Forchammer † first separated this acid from bases; the process which he employed consisted in precipitating a solution of green chameleon by nitrate of lead: he describes the dark brown precipitate as a mixture of peroxide of lead and deutoxide of manganese, which, by digestion with sulphuric acid, formed sulphate of protoxide of lead, while the oxygen given off united to the deutoxide of manganese to form manganesic acid; here there is a mixture of oxides, one of which is resolved into protoxide. Now, if we substitute peroxide of manganese in the place of peroxide of lead, then, by analogy, the action of sulphuric acid is, as before, to resolve peroxide into protoxide, while the oxygen given off unites to the deutoxide to form manganesic acid; and upon this view the changes may be expressed as follow: each proportional of peroxide by the action of sulphuric acid loses one proportional

* Dr. Turner, Phil. Mag., iv. 31; and Phillips, Phil. Mag., N. S., v. 216.
† Annals of Philosophy, xvi. p. 133.
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of oxygen to become protoxide, whilst two proportionals of oxygen so evolved unite with one proportional of peroxide and constitute manganic acid, which instantly assumes an independent existence in the acid solution.

I have mentioned that the tints are not constantly the same: when oil of vitriol acts upon peroxide of manganese in the cold, the colour, at first pink, becomes ultimately deep crimson; a rich scarlet fluid is formed when sulphuric acid acts upon hydrated brown oxides; and I have also observed, where excess of oxide has been employed, that the subsequent additions of acid were paler and pinkish tinted. If strong oil of vitriol be added to the deep crimson solution, the colour changes to pink or to violet, bordering upon purple: on concentrating the same deep red solution by heat, it changes to a pink tint. These variations are enumerated to show that no arguments can be raised against manganic acid on the point of colour: for these colours are identical with those exhibited by manganic acid and manganates under different circumstances.

Assuming that manganic acid gave colour to the red sulphuric acid, then the addition of manganic acid should increase the colour and other properties; and it was found that manganic acid added to pink sulphuric acid decanted from an oxide, immediately heightened the brilliant pink tint, giving deep colour without any other change. After the fluid had been kept three months in a stopped bottle, the pink colour still remained stronger than that of the acid originally employed. The red manganate of potassa, with sulphuric acid, was also mixed with another portion of the same solution; the colour was increased to crimson; and although oxide of manganese was deposited upon the glass after some days, the tint still remaining was stronger than that of the acid in contact with oxides. A portion of the red sulphuric solution was decomposed by adding six times its bulk of water; dark brown oxide was separated; and the clear fluid evaporated to its former bulk; then red chamelion of potassa restored the red colour, which appeared exactly like that of the original fluid when they were compared. Two mixtures were prepared—one of sulphuric and manganic acids, the other of sulphuric acid
and colourless protosulphate of manganese; both were of similar temperature and density; they produced a clear bright red solution when put together, which, by dilution with water, decomposed, and appeared of an amber tint by the separation of oxide. Protosulphate of manganese may be added to red manganesate of potassa and sulphuric acid without any immediate precipitation. The triple sulphate of ammonia and manganese, when acid, was rendered pink by manganesic acid. The existence of manganesic acid in fluids containing protoxide, at once explains the origin of the rose tints observed in the salts of manganese.

These experiments relative to the addition of manganesic acid to the ordinary red solutions, show that the mixture exhibits the same colour as a solution of the same depth of tint, either of sulphuric acid and red manganesates, or of the ordinary red sulphate; but the properties so communicated in this case clearly depend upon the known state of manganese, and therefore the evidence is much strengthened in favour of the opinion that the whole of the phenomena of all red solutions of manganese are due to manganesic acid.

Having thus found that manganesic acid could do all that the red solutions performed, I endeavoured to obtain the acid itself from solutions formed in the usual mode, by acid and oxide; and as manganesic acid is capable of existing in a state of vapour, I hoped, if it were present, that a small portion might be volatilized by distillation. I had previously found that the bleaching alkaline chlorides could hold manganesic acid in solution; very many distillatory experiments were therefore made with the very deeply coloured red sulphate, and in some of them it appeared as though manganesic acid was driven over into the receiver, for a solution of indigo was bleached; and even the acid itself, by its pink tints and other properties, was evident in the solution of chloride, which received the extremity of the retort. These results became doubtful, when it was afterwards found that certain proportions of acid protosulphate of manganese when mixed with a solution of bleaching chloride of soda formed manganesic acid. The experiments were therefore repeated with a retort having a long neck, which was bent several times in order to collect
any acid fluid during the distillation; and according as greater precautions were taken, slighter indications of manganesic acid were obtained; but as the hot fluid which collected in the angles would obviously tend to absorb and interfere with any small portion of volatile matter from the body of the retort, and as the length of transit was also increased, under these circumstances no conclusion can fairly be drawn either way. While the above experiment throws doubt upon the results of vapourizing the crimson fluid, yet it establishes the fact, that manganesic acid is produced when oxides of manganese are in solution with substances evolving oxygen, and therefore supports the particular view I have taken*.

Experimental comparisons were then made between the red solutions and those containing manganesic acid, with regard to other properties; but the presence of protoxide of manganese in the red solutions, and the great excess of acid, are circumstances which interfere with and modify the results obtained by the action of some tests, for as yet no process has been devised for separating protoxide or the proto-salts.

Solution of green chamelion of potassa and oxalic acid formed a deep red solution, as also did red manganesate of potassa and binoxalate of potassa. Green manganesate of baryta and binoxalate of potassa produced a rich red solution. These were compared with the crimson fluid from a mixture of binoxalate of potassa and peroxide of manganese. All these solutions became colourless in some hours' time, and in concentrated solutions deposited white crystals. They are all rendered colourless by heat. Ferro-prussiate of potassa gave in all a peculiar yellow-green precipitate; hydriodate of potassa a reddish-amber tint in all. Red ferro-prussiate of potassa occasioned in the whole a similar red-brown turbidness; by transmitted light the edges appeared greenish. With tincture of galls all these solutions became colourless, and deposited light-brown oxide. Proto-muriate of tin rendered them colourless, and precipitated minute white crystals. Sulphuretted hydrogen destroyed colour in all, and rendered them turbid. Caustic

* Dr. John described the fact of the volatilization of manganese by distillation some years before manganesic acid was known; but he supposed it to be a new or different body, and remarks that the experiments must be made upon some pounds of the ore at once.—Annals of Philosophy, ii. 270.
potassa, soda, or ammonia, threw down brown oxide in them all.

Hydriodate of potassa produces no change in solutions of proto-salts, but it destroys the colour of manglesates, producing an amber tint of free iodine. The oxalic red fluid is similarly affected, and is identical in colour with the red manglesates, if oxalic acid be added to them.

Both mangesic acid and the mangesates mixed with oxalic acid give a bright yellow-green precipitate with prussiate of potassa; a similar precipitate may also be obtained in the ordinary oxalic crimson solutions.

By alcohol, the solutions of mangesates and binoxalates afford red crystals. The crimson fluid supposed to hold deutoxide also throws down red crystals, which appear to have precisely the same properties.

Thus, three solutions containing mangesic acid, and a red solution supposed to contain deutoxide, have similar properties.

I have not succeeded in forming red solutions by acting with muriatic acid upon oxides of manganese. Diluted muriatic acid and green mangesate of baryta gave a crimson solution, which, when added to concentrated and neutral muriate of manganese, threw down brown oxide; but a crimson transparent fluid resulted when the muriate had excess of acid. Muriatic acid changes green chameleon red, and muriate of manganese may then be mixed with it*.

Nitric acid and oxides do not form a red solution, but strong mangesic acid communicates pinkness to the solutions of proto-nitrate of manganese. All these solutions very soon become scarlet and turbid: however, these experiments show that mangesic acid, protoxide of manganese, potassa, or baryta, may exist

* H. Rose (Treatise on Chem. Analysis, pp. 91, 92) describes the properties of a dark brown solution of deutoxide of manganese in muriatic acid; and says that, 'by boiling, the perchloride is rapidly reduced to protochloride.' It is also stated that 'the peroxide of manganese dissolves in the cold acid, and also produces a dark brown solution of deutoxide of manganese.' It may be sufficient for me to observe, that, as these are not red solutions, they need not be considered as opposed to the view which I am supporting; and even if they should hereafter be proved to contain dissolved deutoxide, then it is obvious that this oxide will give a dark brown, and not a pink or crimson solution. Finally, the perchloride of manganese, when decomposed, forms muriatic and mangesic acids, according to M. Dumas, who discovered this compound.—Edin. Journ. Science, xv., p. 179.
Red Solutions of Manganese.

Together to a certain extent by excess of acid, which condition accompanies all the ordinary red solutions of manganese.

Acid solutions of sulphates of potash, soda, magnesia, and zinc, appear of a clear red colour after mixture with the red sulphuric acid, or with manganesate of potassa. The borates of potassa and soda are also reddened by both solutions; and the red sulphate, or the crimson oxalic fluid from oxides, may have their colour increased by the addition of manganesate of potassa. Thus both sets of solutions are similarly compatible with other fluids.

When a solution of chloride of lime (bleaching powder) is added to proto-salts of manganese, the protoxide, by the agency of chlorine, receives additional oxygen, and is converted into peroxide. On using the muriate, I observed that, after some days, the supernatant fluid became bright pink; and, expecting that protoxide of manganese would be absent from this solution, I examined it more particularly. The colour, increasing by time, became pink and violet, like a solution of pure manganesic acid, which I found was present, for potassa precipitated lime, and then changed this red fluid to blue and green chameleon. I have obtained red fluids in this way several times*.

The solutions of the bleaching chlorides of potassa and soda have frequently pink tints, supposed to be due to the introduction of manganese in some unknown state into the liquor, while chlorine gas was passing through the apparatus. Practically it was found that the fluids could only be obtained colourless by slowly disengaging the gas; for whenever the chlorine was rapidly evolved, if the solution was caustic potassa or soda, it always became pink or red.

I procured two specimens—the one was colourless, and the other pink; and I added an aqueous solution of manganesic acid to the colourless fluid, which instantly assumed a similar but much deeper tint than the coloured fluid: then the coloured fluid had its colour greatly increased by manganesic acid; neither fluid exhibited any other change by such addition. I found that manganesic acid and manganates were

* Mr. Phillips, alluding to the same fact, says, that 'the muriate of manganese should be as nearly saturated as possible, for the chlorine evolved by excess of muriatic acid occasions the acidification of a portion of the manganese.'—Annals of Phil., N. S., v., p. 216.
compatible with all the bleaching chlorides of caustic and carbonated alkalies; in some experiments the tints were unchanged after four months.

On the supposition that an oxide is the cause of the well-known tints of manganese, it seems difficult or impossible to account for the introduction of any of the oxides into these fluids; for neither the prot, red, deut, or peroxide, or any of their compounds, are volatile; and even if one of them be admitted to be present, it could not be retained in these solutions, because they are decidedly alkaline. Now, on the view which I advance, manganesic acid can exist in solutions of carbonates and bi-carbonates, and in these bleaching solutions. Manganesic acid is volatile, as are the perchloride and the fluoride of manganese, which, on decomposing, form manganesic acid*.

These facts I consider will satisfactorily explain the hitherto almost anomalous appearance of manganese in certain cases of experimental research, and in various processes in the arts †.

From all that has been advanced, it would appear that bleaching properties, which have sometimes been attributed to chlorine, in certain cases, belong to all red solutions; and that these solutions are similar to such as contain manganesic acid—for both solutions are alike in colour and in bleaching power; both become colourless by the same agents; both lose the bleaching power by losing the coloured state; both afford similar indications by reagents; both, while red, afford a red salt in crystals, which appears to possess the same properties; both, when they lose colour, afford a crystallized

* Quarterly Journal of Science, xxv., p. 486.
† Manganese was observed in a solution of carbonated alkali, into which it had been carried by chlorine, although the gas had been washed with water, and had also passed through an alkaline solution, before the oxide was deposited in the second bottle of Woulfe’s apparatus.—Quarterly Journal of Science, vol. xxv., p. 86.

Chloride of lime is frequently obtained which dissolves with a pink colour due to manganesic acid, although the chlorine gas had been transmitted through water.

Mixtures of chlorides of lime and potassa, prepared for some manufacturing purposes, constantly afford a deep red solution, and possess extraordinary bleaching powers.

Manganesic acid also colours the solutions in the formation of the salt called chlorate of potassa.
Red Solutions of Manganese.

colourless proto-salt; both are compatible with certain other solutions of other substances. From these close and numerous points of comparison, I conclude that the effects of all red solutions of manganese depend upon manganesic acid.

The production of manganesic acid in the cases investigated is agreeable to theory; and if its presence be considered as established, some important consequences follow. The soluble states of manganese are thus resolved into colourless protoxide and manganesic acid. When oxygen is liberated from peroxide by sulphuric acid, manganesic acid may be alternately produced and destroyed. The composition of any oxide which has been or may be obtained from a red solution will obviously depend upon the quantities of protoxide and manganesic acid; and this explains the variable proportions which have been obtained with such precipitates.

The view I have taken may have many interesting bearings. Thus the red oxide is composed of 28 metal and 10.66 oxygen, and the deutoxide of 28 metal and 12 oxygen; and these are assumed to form with acids very feeble indefinite compounds, existing only in the cases referred to, and not volatile. On the contrary, manganesic acid, the cause now assigned of the redness of the solutions, is constituted of 28 metal and 32 oxygen: it is capable of existing in the solid or anhydrous form, or in the state of vapour, and so be transferred from one situation to another. It is soluble in water, in certain alkaline fluids, in some acids, and in solutions of many saline compounds. It is capable of combining with alkalies and earths; when decomposing, it can impart oxygen to other bodies, and thus produce or modify effects which may have been referred to other causes, when its presence has not been suspected.

I have given the evidence as much as possible dependent upon the qualities of the solutions, and purposely so, in order that the view now brought forward may not be interfered with, should the proportions of any of the compounds be subject to correction. Having drawn my conclusions from experiments, I have adverted to the opposing statements of authorities only, to show the opinions entertained upon this subject of acknowledged difficulty.
ON THE COMPARISON OF BRITISH, FRENCH, AND DUTCH WEIGHTS.

BY DR. G. MOLL,
Professor in the University of Utrecht.

(Communicated by the Author.)

It is well known, that after the conclusion of the labours of the Commission for the establishment of the metrical system of weights and measures in France, in 1799, duplicates of the mètre and the kilogramme were presented by the National Institute to each of the foreign members of the said Commission, and also to each of the Governments then allied to France, who had sent commissioners, on this occasion, to Paris. These duplicates were authentic copies both of the mètre and kilogramme, made and adjusted, it is said, under the immediate inspection of the Commission of Weights and Measures, and carefully and minutely compared with the original standards. After this, they were marked with a particular stamp of the commission, being an ellipse divided into four quadrants, three of which are shaded, and in the fourth, not shaded, the number 10,000,000 is engraved; thus the form of this stamp is nearly the following.

![Stamp Image]

The late Professor Van Swinden was one of the Commissioners sent by this country to the general meeting at Paris, and in consequence he became possessed of a set of these authentic copies of the mètre and kilogramme, on which he always set the highest value, and which were constantly preserved with the greatest care. After Mr. Van Swinden's demise they came into my possession.

Being furnished with one of the well-known balances of Mr. Robinson of Devonshire Street, and with very accurate imperial British troy weights, made by the same artist, I was anxious to compare Mr. Van Swinden's authentic copy of the kilogramme with the British troy weight.
The beam of the balance which I employed is about eighteen inches in length; the knife-edge of the axis is about two inches and a half long; the knife-edges on which the scale pans are suspended are about one inch. When loaded with a kilogramme in each scale, \( \frac{5}{1000} \) of a grain are sufficient to show a derangement in the equilibrium of the beam.

In order to ascertain as much as possible how far the weights of Mr. Robinson might be relied on, and within what limits the errors, arising from some inaccuracy in the weights, were likely to be circumscribed, I requested Mr. Bate of the Poultry to make for me a very accurate brass copy of the one pound British imperial troy weight; and it is but justice to say, that this standard greatly excels in point of workmanship, and the care with which it is preserved from external injury, the rather unsightly appearance of the standard kilogramme of Monsieur Fortin's making.

This troy pound of Mr. Bate was found, by a mean of very many weighings, to be equal to 5759.935 of Mr. Robinson's grains, making a difference of 0.065 grains, or \( \frac{65}{1000} \) of a grain, or \( \frac{1}{88} \) of the whole. I regret that it lies not within my power to ascertain whether this slight difference lies with Mr. Bate or Mr. Robinson. But it will, however, appear that a difference like this has but small influence on the general result, as far as concerns the kilogramme.

It will scarcely be necessary for me to preface, that, in all these weighings, I constantly employed Borda's method, of avoiding any error arising from the inequality in the length of the arms of the balance beam.

With every precaution, then, I found the authentic brass copy of the kilogramme, brought from Paris by Mr. Van Swinden, adjusted by the celebrated commission of weights and measures, and made by M. Fortin, to be equal to

<table>
<thead>
<tr>
<th>Weight</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>1st</td>
<td>15432.350</td>
</tr>
<tr>
<td>2d</td>
<td>15432.320</td>
</tr>
<tr>
<td>3d</td>
<td>15432.305</td>
</tr>
<tr>
<td>4th</td>
<td>15432.280</td>
</tr>
<tr>
<td>5th</td>
<td>15432.290</td>
</tr>
<tr>
<td>6th</td>
<td>15432.265</td>
</tr>
</tbody>
</table>

Mean 15432.295
However, as I consider the last weighings much more accurate than the first, I have no hesitation in adopting 15432.265 of Mr. Robinson's grains as a very near approximation to the value of Mr. Van Swinden's brass standard kilogramme. From this we have the British Imperial troy pound equal to 373.244 grammes; the British troy ounce, 31.1037 grammes; the British troy grain, 0.0648 grammes; and the kilogramme, equal to 2 lbs. 0 oz. 3 dwt. 0.265 gr.

Besides this duplicate of the original kilogramme, I had an opportunity of examining many standards of the same unit of weight, said to be very accurate. They are the following:

2. A kilogramme sent by the French administration of the then imperial mint, at the time when Napoleon incorporated this country in his empire. This weight was intended to be used as a standard in the mint at Utrecht; it was made by M. Gandolfi, balancier de la Monnaie de Paris, and was called kilogramme modèle. It was stamped V. G., the initials of the maker, and M., signifying modèle.

3. A kilogramme, also sent by the French imperial mint administration in Napoleon’s time, to the mint at Utrecht, made also by Gandolfi, and marked V. G.

4. A kilogramme in brass, made, it is said, with great accuracy by M. Fortin in Paris, and belonging to the ministry of the interior of this country.

5. A kilogramme in brass, being the standard made use of at present in the mint at Utrecht, and made in this country by T. A. Nagel, inspector of weights and measures.

6. A kilogramme, by the same maker, belonging to the Royal Institute of Holland, and marked in consequence with the private stamp of that body.

7. A kilogramme, by the same maker, belonging to me, and stated to be made with great care. It is marked No. 2.

8. A kilogramme, by the same, belonging to me, and said to be very accurate.

These different kilogrammes, expressed in Mr. Robinson's troy grains, gave the results contained in this Table:
British, French, and Dutch Weights.

<table>
<thead>
<tr>
<th>No.</th>
<th>By whom the Kilogramme made.</th>
<th>To whom belonging.</th>
<th>Value in Mr. Robinson's British Troy Grains</th>
<th>Difference with Mr. V. Swinden's Standard.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>Fortin</td>
<td>Mr. Van Swinden's</td>
<td>15432.265</td>
<td>0.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Standard</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2.</td>
<td>Gandolfi, V. G. and M.</td>
<td>Mint at Utrecht</td>
<td>15432.730</td>
<td>0.465 gr.</td>
</tr>
<tr>
<td>4.</td>
<td>Fortin Modèle</td>
<td>Ministry of the Interior</td>
<td>15432.752</td>
<td>0.487</td>
</tr>
<tr>
<td>5.</td>
<td>{ T. A. Nagel, Amsterdam }</td>
<td>{ Mint Standard at</td>
<td>15432.920</td>
<td>0.655</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Utrecht }</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6.</td>
<td>Idem</td>
<td>{ Royal Institute of</td>
<td>15432.985</td>
<td>0.720</td>
</tr>
<tr>
<td></td>
<td></td>
<td>the Netherlands }</td>
<td></td>
<td></td>
</tr>
<tr>
<td>7.</td>
<td>Idem, No. 2.</td>
<td>Dr. Moll</td>
<td>15433.42</td>
<td>1.155</td>
</tr>
<tr>
<td>8.</td>
<td>Idem</td>
<td>Idem</td>
<td>15434.91</td>
<td>2.645</td>
</tr>
</tbody>
</table>

It is said that Dr. Kelly found a copy of a very accurate standard kilogramme, sent over from France to the British Mint, to weigh 15.433 British troy grains, but as I do not possess the Universal Cambist, I take this information at second hand.

Dr. Weber of Berlin was furnished with a brass standard of the British imperial troy pound, procured by Professor Schumacher, with one of Mr. Robinson's balances, and with a platinum kilogramme belonging to the Prussian government. He found 1 lb. British troy, or 5760 grains = 373.2484 grammes*: this makes the kilogramme equal to 15432.08222 grains, leaving a difference with Mr. Van Swinden's kilogramme of 0.183 grains. It is natural and just to suppose that Dr. Weber paid due regard to the circumstance, that the platina kilogramme, if equal to the weight of the two platina kilogrammes kept at Paris, one in the Archives Nationales, and

the other at the observatory, will be of unequal weight with a correct brass kilogramme weighed in air.

The result of all these experiments is, that we discover differences between weights, which ought to have been equal, which altogether appear intolerable, and there remains an obscurity about the real value of these weights, which it is difficult to account for. Yet, if we are to look to the comparative weights of the kilogramme, and the British troy pound, as deduced, not by actual weighing, but by computation, we shall find differences still greater and more bewildering.

1st. In 1769, Tillet, the French academician, determined, by actual weighing, the relation of the British troy pound and French poids de marc; hence by the known proportion of this poids de marc to the kilogramme, as determined by Lefevre Gineau, we may compute the value of the kilogramme in British troy grains.

2nd. In Dr. Young’s Lectures on ‘Natural Philosophy,’ is a calculation of the comparative weight of the kilogramme and British troy, from the known ratio of the metre and British yards, and the experiments of M. Lefevre Gineau and Sir George Shuckburgh on the weight of a cubic decimeter and a British inch of water at a certain temperature.

3rd. In the Appendix to the third report of the British commissioners for weights and measures, the result is given of a similar computation drawn from the same source and the repetition of Sir George Shuckburgh’s experiments by Captain Kater.

4th. Mr. Mathieu of the Paris Observatory, computed from French and English experiments the value of the kilogramme in British troy grains.

From all these operations the following different relations of the kilogramme to British troy grains was constructed, to which the value of Mr. Van Swinden’s standard kilogramme has been prefixed. We may perhaps, all at once, lay aside the result deduced from Tillet’s operations, as probably the
scales and weights used in his time could hardly possess that accuracy and sensibility which are required at present.

<table>
<thead>
<tr>
<th>No.</th>
<th>By whom the Calculations were made</th>
<th>Kilogramme weighs in British Troy Grains</th>
<th>Differences with V. Swinden's Standard</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>Tillet, Van Swinden</td>
<td>15432.265</td>
<td></td>
</tr>
<tr>
<td>2.</td>
<td>Sir George Shuckburgh, Dr. Young</td>
<td>15444.03</td>
<td>13.451 grs.</td>
</tr>
<tr>
<td>3.</td>
<td>Sir George Shuckburgh, Mr. Fletcher, Captain Kater</td>
<td>15444.0</td>
<td>11.765</td>
</tr>
<tr>
<td>4.</td>
<td>Mr. Mathieu</td>
<td>15438.355</td>
<td>7.735</td>
</tr>
</tbody>
</table>

Differences amounting to such an enormous extent are truly appalling, and some secret cause must exist why all the comparative values of the two weights, as found by calculation, are so widely different from what they are found by actual ponderation.

Next, all the kilogrammes, which were actually weighed, differ amongst each other, and also from that standard which was made under the immediate inspection of the contrivers of the metrical system. Of all these, the platina kilogramme used by Dr. Weber is the lightest; then follows the brass standard of Mr. Van Swinden, and all the rest are considerably heavier. It might be argued that those made in this country were adjusted with no great nicety; but what are we to say or to think of the differences between those made by Fortin and by Monsieur Gandolfi, balancier de la Monnaie de Paris? Some gross error must undoubtedly lay concealed in some parts of the operations, and strange suspicious as to the source of these errors must arise in the minds of any one who looks into the matter; perhaps it is not irrational to suppose that errors lurk, where the least light is thrown in.

We are in full possession of the facts, on which the determination of the metre rests, but we are far from having such
full and complete information as to the means by which the kilogramme was determined. The coincidence of the experiments on the length of the pendulum made by Captain Kater and Monsieur Biot affords sufficient evidence of the accuracy with which these operations were conducted; but how the kilogramme was come by, has never been very satisfactorily explained. Indeed, M. Delambre, in the 'Base du Système Métrique,' having explained how the metre was determined, says, that the account of the fixation of the unit of weight ought to have followed; but that the multitudinous avocations of Mr. Lefevre Gineau, as professor, as an inspector of studies, and finally as a member of the legislative chamber, did not allow him sufficient leisure to lay the finishing hand to the arrangement of his papers, although the plates were engraved long ago. In consequence, not the particulars of the experiments, but the account of what was done, as drawn up by Tralles, is given as it was read before the Institute. But this is a report of the experiments, not the description of the experiments themselves; we have the shadow, not the thing itself, and we are entirely in the dark as to the particulars of so interesting and intricate an operation as the determination of the unit of weight.

It must be observed that the operations by which the kilogramme was determined were closed in 1799, and that M. Delambre's evidence as to the supineness of M. Lefevre Gineau is given in 1810. Thus, in eleven years M. Lefevre could not spare time for a work so important, and on which his scientific reputation was chiefly to rest: for it must be recollected, that great as the merit of M. Lefevre as a legislator and as an inspector of the University possibly might have been, he has not attached his name to any other scientific operation than the determination of the kilogramme. All this would certainly have been entirely different if the ingenious and lamented Borda, who was the soul of all the operations for determining the metre and kilogramme, had not been untimely snatched away. But, taking the matter as it stands now, it must be confessed, that we know very little of the means by which the unit of weight was determined.

But whatever is the degree of uncertainty prevailing about
the real value of the kilogramme, at least the different duplicates of this weight should have been equal amongst each other, which, however, is far from being the case. I am sorry to add, that some such variation is, to a certain extent, prevailing as to the British troy pound. I have noticed already, that there is some slight difference between Mr. Bate’s troy pound and Mr. Robinson’s 5760 grains, but there are instances of much greater differences.

In 1818 the British consul at Rotterdam applied to the general masters of the mint at Utrecht for a standard copy of that weight, which, at that time, was used in the Mint of this country. In consequence, a standard copy of the Dutch troy pound was prepared and forwarded to the British consul, and he was requested to procure in return a standard of the British troy pound. Agreeably to this, two copies of a brass standard British troy pound were adjusted at the Mint in London, and sent to this country, together with a certificate from a Mr. Field of the London Mint. I have examined both these copies of the British troy pound, and one is actually in my possession, whilst the other is kept at the Mint office in this city. Upon each of these brass troy pounds the following inscription is engraved, which stamps them, as it were, with a character of officiality:

**BRITISH TROY POUND**

= 5760 GRAINS.

FROM

HIS MAJESTY’S MINT.

Notwithstanding this certificate of authenticity, and a paper belonging to them, in which a Mr. Field, an officer, as it would appear, of the Mint, asserts that they were carefully adjusted, both these weights are unequal to each other, and to the 5760 of Mr. Robinson’s grains.

That which is at present kept in the Mint Office of Utrecht weighs 5758.57 grs.
The second, now in my possession . . . 5758.40 
Whilst Mr. Bate’s imperial troy pound holds . . . 5759.935

It is exceedingly vexing to see weights adjusted in the Mint of England, and on which expense has not been spared*, differing more than \(\frac{1}{10}\) of a grain from each other. Furthermore, the difference of these weights, Mr. Robinson’s grains

* The sum of 5s. 5s. was charged for making and adjusting these weights.
and Mr. Bate's pound, is so great, that we cannot help thinking but that there must exist some notable difference between the Mint standard of troy weight and that according to which both Mr. Robinson and Mr. Bate made their copies.

By an act of the 5th George IV., the standard of 1758, in the custody of the Clerk of the House of Commons, is declared standard of the British troy weight. This standard was made or adjusted in 1758 by Mr. Harris, then Assay-master of the Mint. We cannot, therefore, but admit that, since 1758, an enormous and unaccountable change took place between the troy weight as used in the Mint of England and that copy which remained unaltered in the custody of one of the principal officers of the House of Commons.

Although the weights formerly in use in this country have been abolished and replaced by the French kilogramme, it may not perhaps be altogether superfluous to compare these old weights with the British troy and avoirdupois, as established by the act of 17th June, 1824, 5th George IV.

Formerly there were three different sorts of Dutch weights in use in Holland.

1. The Dutch troy weight, differing essentially from the British troy. It was used in the concerns of the Mint, in the weighing of gold and silver, and also for medical, pharmaceutical, and philosophical purposes. In the province of Friesland it was the general weight for commercial transactions, and no other weight was employed there.

2. The Amsterdam commercial weight, generally used throughout the country for all commercial concerns on a large scale; it was originally derived from troy weight.

3. In shops, and for the retail trade, a light weight was almost generally used, except in Amsterdam. It was also employed, in some cases, in the iron trade. It is originally the Antwerp weight.

The general opinion is that troy weight derives its name from the city of Troyes in Champagne, in which place it appears that very heavy weight was used in old times. It would seem, however, that there is no document in existence at present, in the records of that town, which is calculated to throw any additional light on the history of the introduction of this
British, French, and Dutch Weights.

weight, which, with different modifications, has been adopted both in Holland and England.

In the fifteenth century, troy weight was used in Amsterdam; and in 1520, the Emperor Charles V., then sovereign of the Low Countries, ordered that the mint-masters in the several provinces should adjust their weights to the standard of troy weight kept in the offices of the several courts of accounts (cours des comptes) in the different provinces. The old standard of this weight, even now existing in the Hague, has been adjusted in 1554 to the standard of the court of accounts of Brabant at Brussels. The same regulations as to the use of troy weight have been reinforced by the statute of the Earl of Leycester in 1586, and by that of the States-General of 1606. However, the troy weight in use in the Seven United Provinces for upwards of a century down to the time of its being abolished in 1819, was somewhat different from the old standards; but these matters, being of a local nature, do not require to be stated here in all their particulars.

Mr. Van Swinden, before he set out for Paris in 1798, to attend the meetings of the great commission of weights and measures, procured a copy of the Dutch troy weight, as it had been used for more than a century, and had it adjusted with great care. This duplicate Dutch troy weight he took to Paris, and by its means, he, M. Aeneae, the second commissioner, and M. Lefevre Gineau investigated and determined, with all the accuracy which they could, the relative proportion between the Dutch troy weight and the kilogramme. This standard, religiously preserved, came also into my possession, and I have several times weighed it against Mr. Robinson's British troy grains, with all the attention which I could master. Accordingly the weight of

One brass pound of Dutch troy weight is 7594.975 grains of Mr. Robinson.

\[= 15 \text{ oz.} 16 \text{ dwt.} 10.975 \text{ grs.}\]
\[= 15 \text{ oz.} 6 \text{ drachms}, 1 \text{ scruple}, 14.975 \text{ grs.}\]

And one pound Dutch troy \[= 0.7583961764193 \text{ lb. British troy.}\]

One ounce Dutch troy \[= 474.6856 \text{ grs. British troy.}\]

One grain Dutch troy \[= 0.989 \text{ gr. British troy.}\]

One grain British troy \[= 1.0112 \text{ Dutch troy.}\]

And finally, assuming the kilogramme to be equal to 15432.265 of Mr. Robinson's grains, we have one pound Dutch troy weight equal to 492.14907857 grammes.
Dr. Moll on the Comparison of

The Dutch troy weight, as used for weighing bullion and the precious metals in general, has sixteen ounces, but for medical and pharmaceutical purposes, there are twelve ounces in the pound. But the ounces are the same in both cases, and contain 480 grains.

For mint purposes and the weighing of bullion, the division of the Dutch troy was as follows:

- 1 lb. Dutch troy = 2 merks,
- 1 merk = 8 ounces
- 1 ounce = 20 sterlings (English) = 480 grains
- 1 sterling = 32 aas.

Thus 1 lb. Dutch troy = 10240 aas = 7680 Dutch troy grains.

= 7594.975 British troy grains.

For medical and pharmaceutical use, the division of the Dutch troy ounce was as follows:

- One Dutch troy ounce = 8 drachms = 480 grains.
- = 1 drachm = 3 scruples = 60 grains
- 1 scruple = 20 grains.

As the British avoirdupois contains 7000 grains, British troy, it is a matter of computation to deduce from thence the relation of the Dutch weight to English avoirdupois.

\[
1 \text{ lb. avoirdupois} = 0.92166202 \text{ lb. Dutch troy weight,}
\]

and inversely,

\[
1 \text{ lb. Dutch troy} = 1.0849964 \text{ lb. avoirdupois.}
\]

In the golden old times of trade, the magistrates of Amsterdam were anxious that the commercial weight of that city should be heavier than that used in other trading-places. Therefore it was established by law, that the Amsterdam commercial pound should be 40 aas in the pound heavier than troy weight. In consequence of this regulation, we have the following relations between British and Hollands commercial weights.

\[
1 \text{ lb. avoirdupois} = 0.9180754 \text{ lb. Amsterdam weight.}
\]

\[
1 \text{ lb. Amsterdam} = 1.0892347 \text{ lb. avoirdupois.}
\]

* The Amsterdam pound is divided into thirty-two parts called looden, for which I know of no English corresponding word.
An English hundredweight, or 112lb. avoirdupois = 102.824lb. Amsterdam weight.

How far the use of heavier weight than our neighbours may be beneficial in a commercial point of view, and whether such attempts may lead to any practical result, is not for me to investigate.

I. ON A REMARKABLE CASE OF CORYZA PHLEGматICA.
II. ON THE DIRECT FORMATION OF OXIDE OF IODINE AND IODOUS ACID.
III. ON NITROGEN IN NATURAL WATERS.

By the CAVALIERE LUIGI SEMENTINI, of Naples, M.R.I. &c.

In a Letter addressed to the Secretary of the Royal Institution.

I. CORYZA PHLEGматICA.—The following account of an uncommon disease, which has recently come under my observation and cure, may not be unacceptable. A similar affection has been called by Sauvages and Borsieri, Coryza Phlegmatica or Phlegmatoragia.

The patient, who was about fifty years old, abounding in humours, and of a sanguine temperament, after a strong fit of sneezing, was suddenly affected with a constant running from the right nostril of a clear liquid, in large drops, to the number of twenty-five in a minute, and of twelve at the least; but the diminished number occurred only during the hour after dinner. The discharge increased to the larger number of twenty-five drops per minute in the evening, and continued without interruption during the whole course of the night. Hence the patient was obliged to take his rest in an uneasy position, with his head inclined forward, without which precaution he would have been suffocated. In walking, on the contrary, he went with his head erect, in order that the liquid might fall into the fauces, from whence he discharged it by large mouthfuls, thus avoiding the disgust of having his face and clothes continually soiled.

The fluid in question had a strong saline taste, and the patient was obliged to muffle up his nose during meals, in
order that it might not mix with his food. Nevertheless, he
was not afflicted with head-ache, nor with any uneasy sensa-
tion in the fore-part of the head, nor indeed with any of the
ordinary phenomena accompanying such diseases. He only
lost a little flesh, and in other respects enjoyed perfect health.

This disease, which, in my time at least, has not been known
to occur in Naples, is described by the celebrated Morgagni in
his fourteenth Medico-Anatomical Letter, where he treats of
the diseases of the head. But the case he describes of a Veneti-
ian lady, who was affected with it, and cured by him, differs
from that which came under my observation, in that the quan-
tity of the fluid discharged was not above one half, and that
it continued for near a year, while in my patient it continued
only four months; and because the health of the lady was so
sensibly affected by it, as to endanger her life.

The disease was for a long time treated by me in the accus-
tomed derivative method of foot-baths, blisters, sudorifics,
purgatives, &c., but in vain. Afterwards, remembering that
the afflicted person was subject to the gout, though in an
anomalous and irregular form, I was of opinion that a portion
of the gouty humour might have affected the olfactory nerves;
and the result of my experience being that no means were
more effective than James’s antimonial powders to reduce this
disease to a state of regularity, and therefore probably to
remove this humour from the unaccustomed seat it occupied,
I subjected the patient to a course of that medicine. At the
end of four days, his head, which had always been free from
pain, became slightly affected; and at length, after profuse
perspirations, the disease, which had come suddenly, ceased in
a manner quite as sudden.

The fluid discharged, although transparent and limpid when
it first issued from the nostril, soon became turbid, and acquired
a light yellow colour, depositing a flocculent animal substance.
In its composition, beside the muriate and phosphate of soda,
I found a considerable quantity of urate of soda, especially in
the flocculent deposit: there was scarcely any free soda.
II. IODINE AND OXYGEN.—I presume that you are acquainted with my experiments on the combinations of iodine constituting the oxide of iodine and the iodous acid*. This second substance was obtained by triturating the chlorate of potassa with iodine, and heating the mixture in a distillatory apparatus. But Woehler having objected that this substance was a chloride of iodine, and not a distinct acid, I was obliged to vary the experiments, the results of which I am anxious to communicate to the members of the Royal Institution without delay.

In replying to Woehler, I undertake to prove that, on all occasions when iodine is brought in contact with pure oxygen gas, or even with common atmospheric air, at a high temperature, combination takes place, forming first oxide of iodine, and afterwards iodous acid, without the intervention of any other substance. I request your attention to the circumstance of an elevated temperature, because, in the French translation of Berzelius's 'Chemistry,' published at Paris, a low temperature is mentioned, which would prevent the success of the experiment to which I have the honour to refer you below.

The tube A B of cast brass should be of the diameter of a barometer tube, and ought to terminate at the end B in a capillary aperture. It should be fixed to the supports E C, D F. At the extremity A, a bladder G with a stop-cock should be fixed on, which is to be filled with oxygen gas, or even with mere air. The oblong spirit-lamp H H is to be so placed that the flame of its burners may act upon the whole length of the brass tube A B. The extremity B of the tube is to enter the tubular opening of the empty retort K, into which it is to be fixed by luting. Under the bulb of the retort is to be placed the large spirit-lamp L.

The apparatus thus disposed, the burners of the lamp H H being lighted, and that of the lamp L placed under the retort; when the tube and the retort are very hot, the bladder at the end of the tube is to be pressed by an assistant, forcing the gas to issue from the capillary end of the tube, the experimenter, at the same time, introducing a spoonfull of iodine into the neck of the retort, so that the bowl of the spoon may come immediately under the capillary opening of the tube A B. In this operation the iodine is soon raised into vapour, which coming immediately into contact with the heated oxygen gas, combines with it, and assumes the form of an amber-coloured vapour, which condensing in the neck of the retort, becomes a dense oily fluid, which is the oxide of iodine, first discovered by me. By continuing the jet of oxygen gas, I have upon two occasions obtained iodosous acid, but it is incumbent on me freely to confess, that in following up the experiments lately I have not had the same result, without being able to account for the cause of such irregularity.

Yet, nevertheless, by these or by any other means, when iodine is made to combine with oxygen at a high temperature, the oxide of iodine is constantly obtained. The following is a new experiment by which it may be obtained in considerable quantities without complicated apparatus, and by the employment of such simple means that no doubt or discussion can arise as to its nature.

The deutoxide of barium is to be triturated for some time with iodine, in such proportions that the iodine is in excess, and that the mixture acquires a blackish colour. This powder is afterwards to be introduced into a small retort with a long neck, and the heat of a large spirit-lamp applied. The violet-coloured vapour which first appears is soon succeeded by the yellow vapours of the oxide of iodine, which substance collects in the neck of the retort, and ultimately drops from it. The same result is obtained with the protoxide of barium, but the effects are less sensible and the product not so abundant; in operating with the protoxide as well as the deutoxide, oxygen gas is always developed.

When the substances employed are free from water, the following are the properties by which the oxide of iodine is distinguished:—
1. It has a yellow amber colour.
2. It is soluble in water and in alcohol, forming coloured solutions.
3. It is thick like oil, and it is frequently necessary to heat the neck of the retort in order to collect it, after which it remains more fluid. The excess of iodine frequently causes the violet-coloured vapour to pass over with the oxide of iodine; which, however, does not alter the result of the operation, as the two substances do not mix. If the iodine and the oxide of barium are not perfectly dry, the oxide of iodine is much more fluid. It changes the colour of blue litmus paper to an emerald green.

Oxygen is so slightly united to iodine, that the simple contact of a combustible body at any temperature suffices to disunite them, and the iodine is separated with all its properties. Whether the oxide be concentrated or weak, its decomposition is effected by mere contact, even with a piece of white card, which soon becomes covered with a black stratum of iodine.

When the oxide is in its state of greatest density, it ignites phosphorus and potassium by simple contact.

By such evident proofs, therefore, the formation of the oxide of iodine is demonstrated; that of iodous acid becomes very simple and clear, as I shall have the honour of communicating in a future paper in which I purpose to treat of the iodites.

The few facts above stated have not yet been published, with the exception of that which relates to the action of air or oxygen gas with iodine at a high temperature, and I communicate them to the Royal Institution as a mark of my respect, in order that they may be published in the Journal, should they be thought worthy of it.

III. NITROGEN IN THE WATERS OF CASTELLAMARE—A commission, of which I am a member, has undertaken the analysis of the mineral waters of Castellamare, but owing to my indisposition its labours are not yet finished.

Reserving to myself the pleasure of communicating to you the results when completed, I will merely now state that their
composition is very similar to that of the waters of Spa. Every season the concourse of English visitors to those waters is considerable: it may not, therefore, be uninteresting to medical men to learn, that these waters contain azote in considerable quantities—a fact not new in chemistry, but yet not common, and of great importance.

ON THE DIRECTION OF THE RADICLE AND GERMEN DURING THE VEGETATION OF SEEDS.

By THOMAS ANDREW KNIGHT, Esq., F.R.S.,
President of the Horticultural Society, &c.

IN the 'Quarterly Journal of Science' of the last year, (of which publication I regard the 'Journal of the Royal Institution' to be, in some degree, a continuation,) a communication, made by M. Poiteau to the 'Société d'Horticulture' of Paris, is noticed; in which that writer considers himself to have totally refuted and annihilated my hypothesis respecting the descent of the radicle and ascent of the germ of germinating seeds, which was published in the 'Philosophical Transactions' of 1806. M. Poiteau proceeds to slay the slain; for after having, as he supposes, proved that my inferences are not physiologically correct, he goes on to say that they are not at all physiological, and that 'Il convient donc que les botanistes raient l'expérience de MM. Knight et Dutrochet du catalogue des expériences de physiologie végétale,' having previously determined that 'il n'y a rien de physiologique dans l'expérience de M. Knight.'

I had previously seen several attempts to refute my hypothesis; but, in all these, it was either misunderstood or widely misrepresented. I must give M. Poiteau, however, the credit of having fully understood and fairly represented my hypothesis; and the only grounds upon which I can object to his conclusions are that, as far as they are connected with vegetable physiology, all his premises and all his inferences are false; and that if all his premises had been perfectly true, all his inferences would have been totally erroneous.

M. Poiteau attached pieces of metal, of the form of the
seeds of gourds, to the circumference of wheels, similar to those upon which I bound germinating seeds, each piece of metal being perforated near its more pointed and smaller end, and fixed to the wheel by a pivot, round which it was left at liberty to revolve. When these pieces of metal were subjected to the operation of centrifugal force, their heavier ends necessarily receded from the axis of rotation, and the lighter ends were necessarily made to point towards it. M. Poiteau conceives that nothing more occurred in my experiments and those of M. Dutrochet, who repeated the same experiments with very superior machinery, and with the same results; and that the sole cause why the germs approached the axis of rotation was, that their specific gravity is not greater than one-third that of the radicles. I was not so fortunate as to be able to comprehend this; and though I gave M. Poiteau full credit for accuracy respecting the different degrees of specific gravity of the substance of the radicles and germs, I thought the fact a very extraordinary one. I therefore planted a couple of dozen seeds of the plant (phaseolus vulgaris) which was the subject of M. Poiteau’s experiment, and I then discovered, to my astonishment, I confess, that the radicles, instead of possessing a degree of specific gravity three times greater than that of the germs, were really the lighter body of the two, the germs having all sunk to the bottom of the same vessel of water in which all the radicles rose to the surface. I repeated this experiment several times, and always with the same result, having detached both the radicles and germs from their cotyledons as soon as the germs became visible above the surface of the ground.

The seeds in my experiments were bound firmly to the circumference of the wheels, instead of being, as M. Poiteau’s pieces of metal were, left at liberty to revolve upon pivots; and the direction taken by the radicles and germs of seeds is totally independent of each other. The germ is never made to deviate, in any degree, from its perpendicular line of growth upwards by any obstacle which the radicle meets with in its descent; nor is the direction of the radicle ever influenced, in any degree, by that taken by the germ. If the seed of a peach, or pear, or other tree which nature intended to support...
itself be planted, its germ will be seen to incline towards any point from which it receives most light; whilst the germ springing from the seed of ivy, or other plant which nature intended to rely upon the support of some other body, will recede from light and seek the shade; but the radicles of all will be found to proceed alike perpendicularly downwards; and, therefore, if the specific gravity of the radicles had exceeded that of the germs to the extent conceived by M. Poiteau, that circumstance, as the seeds in my experiments were bound firmly to the circumference of the wheels, could not, in any degree whatever, have caused the germs in their growth to have approached their axis of rotation. M. Poiteau must, therefore, allow me to consider his production as a succession of blunders from beginning to end.

I wished M. Dutrochet to refute M. Poiteau's hypothesis; but he seemed to think that M. Poiteau's errors would be obvious to every reader before he could publish his exposition of them; and I should have agreed in opinion with M. Dutrochet if M. Poiteau's communication, instead of his assumption, had been published in the 'Quarterly Journal of Science;' but, as it was not, and as the knowledge of the influence of gravitation upon the moving fluids of plants, and consequently upon their growth, forms, and produce; is important to the scientific gardener; and as I had the honour to receive from the Royal Society Sir Godfrey Copley's medal for the memoir which gave a statement of my facts and hypothesis, I have thought it proper to shew that M. Poiteau's conclusions are not quite so accurate and unquestionable as he appears to have imagined them to be.

Downton, July 8, 1831.
ON DISINFECTION AND THE PRACTICE OF QUARANTINE; WITH SOME REMARKS AND COMMUNICATIONS RELATIVE TO CONTAGIOUS DISEASE, AND ESPECIALLY THE CHOLERA.

By ANDREW URE, M.D., F.R.S., &c. &c.

THE remarkable power of chlorine, and of its officinal compounds, chloride of lime and soda, in decomposing and destroying the fetid effluvia of animal and vegetable bodies in a state of putrefaction, has been so long known, has been verified in so many instances, and is susceptible of such direct demonstration, as to be beyond the cavils of medical pyrrhonism in its most wanton mood. That these effluvia are capable of making morbid impressions on the living body, is also placed beyond any reasonable doubt, not only by the sickness they instantly occasion, but by the many recorded cases of fevers of a putrid or low typhoid type, brought on by incautious exposure to masses of animal matter far advanced in putrefaction. The power of such matter to produce fevers by inoculation, has been often fatally exemplified in the dissecting schools; and the power of a lotion of chloride of lime or soda to counteract danger from such inoculation, is now equally well ascertained. In a letter just received from my son, at present House-Surgeon of the Glasgow Royal Infirmary, he says, 'Having performed several post mortem dissections of persons who have died from malignant fevers, dysentery with extensive ulceration of the mucous membrane of the large intestines, peritonitis with purulent effusion into the abdomen, hectic from suppuration, gangrene, &c., I have never suffered the slightest inconvenience. Yet these are the cases in which that peculiar animal poison is especially generated which has occasionally proved fatal to the demonstrator of disease. I attribute the immunity I have enjoyed, in a great measure, to my washing my hands immediately after each inspection with the chloro-sodaic liquor of Labarraque; this I prefer to the solution of chloride of lime, as it is not so apt to injure the skin.

'A young gentleman, who acted as my colleague during
Dr. Ure on Disinfection.

part of last winter, but who did not adopt the above precaution, having imbibed through a minute breach of surface on his little finger a portion of this virus, was in a few hours thereafter attacked with acute inflammation of the absorbents of the arm, accompanied with high symptomatic fever, which confined him to his bed for many weeks, and required the most powerful antiphlogistic measures to subdue the inflammatory symptoms. I could cite instances of my predecessors having suffered from the same cause, but I deem it unnecessary, as the fact is indisputable.'

A mournful example of the danger of putrefactive effluvia occurred a considerable time ago in the north of Scotland. Two young medical men, desirous of examining a body which had been interred without dissection, in consequence of the prejudices of the relations of the deceased, went in a very dark night to exhume it, but having mistaken the grave, laid open a coffin replete with such noisome corruption, that the gentlemen instantly sickened with the fetor, were hardly able to go home, where they forthwith took to bed with symptoms of malignant fever, and died. MM. Orfila, Leseure, Gerdy, and Hennelle, were employed, about seven years ago, in Paris, to examine the body of an individual who was supposed to have been poisoned, and who had been dead and buried for nearly a month. Had they rashly proceeded to the inspection, they would most probably have fallen victims to their imprudence; but the smell was intolerable, and the body could hardly be approached; they had, therefore, recourse to chloride of lime, sprinkling a solution of it over the putrid corpse, which produced, after a few aspersions, such a wonderful effect, that the nauseous effluvia were instantly quenched, and the dissection was performed with comparative comfort.

Chloride of lime has been repeatedly used since with equal efficacy in similar cases; it has become a familiar anti putrescent agent in the anatomical theatre, and has been applied to destroy the stench of bilge-water and common sewers with unfailing efficacy. Its operation on fish so much tainted as to be hardly fit for the table I have myself repeatedly tried, and I have found that a dish of such fish cleaned and opened up, by immersion in a dilute solution of the chloride for a few
minutes, loses the dark colour at the bone and all offensive scent; and after being washed in water, when boiled it possesses the curdy firmness, sea-air flavour, and taste of newly caught fish. An ounce of good chloride of lime is sufficient to sweeten a very large dish.

The phenomena of putrefactive fermentation seem to show that the fetor resides in certain hydrogenated compounds, containing carbon, sulphur, phosphorus, azote, &c.; for gaseous matter of this kind is eventually disengaged in the larger cavities of the trunk, as well as in the cellular tissue, causing a general intumescence. There is every probability, likewise, that the diffusible fomes of contagious disease resides in some analogous compounds, but of so subtle a nature as hitherto to have baffled every effort of chemistry to collect and analyze. The same thing may be said of the miasmata of marshes. The infectious virus of plague, small-pox, and putrid bodies, resembles in some measure the poisonous secretion of venomous reptiles, and is of a more durable composition and less volatile (so to speak) than the effluvia of typhus, scarlatina, and measles. We can therefore easily understand why an agency capable of decomposing the former morbific powers, may be feeble to grapple with the latter, embodied as they are in a too palpable humour or a solid crust.

Guyton Morveau appears to have been the first man of science who directed the resources of pneumatic chemistry in a regular manner to the purpose of disinfection. The Cathedral of Dijon had been for several years infested with a febrile fomes or miasma, which occasioned fever in many of its pious visitants, and it had become in consequence nearly deserted as a place of worship. Being then (1774) Professor of Chemistry in the Academy of Dijon, M. Guyton was naturally induced to exercise his science in expurgating the air of the church. He accordingly filled the whole capacity of the building with muriatic acid gas, disengaged from a mixture of salt and sulphuric acid distributed in a number of stone-ware dishes. The doors and windows were kept close for two or three days, to prevent the dissipation of the acid fumes. At the end of this period a free ventilation was given by opening the doors and windows, after which the church was found
Dr. Ure on Disinfection.

to be deprived of its unpleasant smell and unwholesome effluvia.

In 1796, Dr. Carmichael Smith applied the fumes of nitric acid, disengaged from nitre by sulphuric acid, to the disinfection of a ship's hospital, for which he received a considerable parliamentary reward.

Since that time the progress of chemical research has made us more fully acquainted with the intense affinity which exists between chlorine and all hydrogenated compounds, and with the resulting anti-putrescent quality of chloride of lime. Hence chlorine has naturally come to be regarded as the most energetic antiloimic agent. In this respect, likewise, the merit of its introduction belongs to M. Guyton, who recommended medical men, nurses, and other attendants on contagious disease, to carry about with them phials containing manganese and muriatic acid, and to open the glass stopper from time to time in situations replete with infectious effluvia, in order that the chlorine exhalations might decompose them, and preserve a healthy atmosphere for respiration. In the sequel of the present paper, facts will be adduced apparently proving the efficacy of this antidote to the contagion of cholera.

As gaseous chlorine in the state in which it is evolved from muriatic acid and manganese, has been thought to be too concentrated for diffusing in apartments occupied by the sick, recourse has been had in a great variety of cases to the exhalations that spontaneously rise from chloride of lime exposed in an extensive surface, either in its pulverulent form or dissolved in water. It is true, indeed, that under both of these forms the chloride exhales its peculiar odour, but it gives out no appreciable or operative portion of gas, and instead of losing, it gains weight. I have suspended a piece of moist litmus paper within three inches of good chloride of lime, in a stoppered phial for upwards of an hour, without its being blanched; nay, the paper retained much of its colour at the end of twenty-four hours. As the paper would have become white in a few minutes by the admission to the phial of one-tenth of a cubic inch of chlorine gas, it is obvious that even that minute volume was not disengaged from the chloride, which amounted to nearly 500 grains. But by the agency of
muriatic acid, that quantity of the said chloride would have evolved about 145 grains, or 190 cubic inches of pure gas. I may remark, that few samples of the bleaching-powder found in the market are impregnated like the above with fully 29 per cent. of chlorine, and the stuff retailed in many shops under that name seldom contains more than 16 per cent. As for the liquid chloride of lime, the two-shilling bottles occasionally possess no more virtue than would be found in two-pennyworth of Messrs. Tennant's dry bleaching salt. Nothing, therefore, can exceed in absurdity the fashionable nostrum for disinfecting apartments charged with contagious fomes, by placing in them one or more saucers filled with chloride of lime. To place this dangerous fallacy in the plainest light, I need merely state that moist litmus paper may be hung for a day a very few inches above such a saucer without perceptibly losing colour; whereas the affusion of a few drops of muriatic acid on the same chloride, even after the above period, will instantly blanch the suspended paper.

It has been supposed that the carbonic acid present in the atmosphere displaces the chlorine from the lime; but how slowly and insignificantly the preceding experiment may show. The following facts have been long before the medico-chemical world. 'After passing a current of this gas (carbonic acid) for a whole day through the chloride diffused in tepid water, I found the liquid still to possess the power of discharging the colour very readily from litmus paper.'

Chloride of lime laid out in the air passes rapidly into a deliquescent paste, consisting of muriate of lime, and lime with an obscure displacement of oxygen. If the chloride be surcharged with chlorine, it speedily gives off the excess and becomes commercial chloride. The best manufacturers, aware of this circumstance, never push the impregnation beyond a certain pitch; in which state the chloride does not spontaneously emit in the air one-thousandth part of its condensed chlorine. To pretend, therefore, to suffocate the hydra of contagion by subjecting it to the simple smell of chloride of lime

* On the Manufacture and Composition of Chloride of Lime, by Dr. Ure, Quarterly Journal of Science and the Arts, for July, 1822.
in a saucer, is just such a mockery as it would be to appease the famished stomach by the smell of a cook-shop. The subtle effluvia of a pestilence must be combated by more energetic means; they must be environed with an atmosphere of chlorine adequate to effect their destruction. Every thing short of this consummation is paltering with the safety, not of a few individuals, but possibly of a nation.

But I shall be asked, whether chlorine gas can be diffused through the air of a chamber without injuring the lungs of living beings, as well as the furniture and goods? I answer yes, when it is distributed on philosophical principles. But I might ask the medical practitioner, in return, whether the corrosive sulphuric and nitric acids may be administered internally? Yes, he would be ready to reply, when sufficiently diluted; and the same answer will serve for chlorine. I have been a frequent inmate of manufactories of chloride of lime on the greatest scale, and I have occasionally found the atmosphere, in certain departments of the works, to be impregnated, in a sensible degree, with chlorine gas. Moist litmus paper would have speedily lost its colour in such an atmosphere, although dyed woollen and calico stuffs, in the dry state, suffered no perceptible change. The workmen who habitually respired this chlorified air experienced no evil effects on their health, nor, indeed, any inconvenience at all, unless an accident befel some joint of their apparatus. These facts prove the safety of immersion in chlorine largely diluted with air, yet still strong enough to blanch moist litmus paper; which may be regarded as a satisfactory criterion of its activity when directed against contagion.

In applying chlorine gas to apartments, we should always bear in mind, that it is one of the heaviest of elastic fluids, and therefore it tends to occupy the lower region in preference to the upper. If, in the little cave near Naples, called the Grotto dei Cani, the carbonic acid adheres closely to the floor, so that, by rising hardly above the knee, a man continues to breathe in perfect ease, unconscious of the presence of his invisible foe, while the dog at his foot is instantly suffocated, we may judge how much more closely a stratum of chlorine should adhere—a gas nearly double in density to
carbonic acid; for air, carbonic acid, and chlorine are in specific gravity respectively as 2, 3, and 5. We need not be told that chlorine, like other gaseous matter, has the faculty of diffusing itself slowly upwards through atmospheric air; but this is only when it has nothing else to do, for when it encounters substances on which it can exercise its pre-eminent affinities, it will combine with them, probably to their destruction, and certainly to its own, as an influential gas. In proof of this position, I have many experiments to adduce, one of which was exhibited before the Marquess of Lansdowne, Sir Henry Halford, and the whole Board of Health in the Royal College of Physicians, on Friday evening, the 24th of June. Having had the honour, two evenings before, to submit to that Board a plan for disinfecting the cargoes of ships, by distributing dilute chlorine through their holds by the apparatus, figured No. 1, illustrative of this paper, doubts were strongly, and very naturally, expressed by many members of the Board, as to the penetrability of dense bales of hemp, wool, and cotton by the chlorine gas. I was asked, whether I could satisfy them on this head by an experiment; and if so, how soon. I undertook to make the experiment in two days; but an anxiety being shown to have it tried next day, I promised to do my endeavour, with such apparatus as I could command. Accordingly, on the 23rd of June, at four o'clock P.M., miniature bales of hemp, wool, and cotton were made up as dense as possible, the latter two being moreover inclosed in thick canvas bags. They were all put into a tall glass cylinder, open at top, the hemp being placed at the bottom. Chlorine was now introduced through a glass tube, which descended beneath the middle of the jar. In the centre of each parcel, a bit of moist litmus paper was placed before it was bound up. Next evening at nine o'clock, the Board having met again, the little bales were opened, and the papers in their centres were rendered snow-white, clearly proving the penetration. The external fibres of the hemp parcel were so corroded by the chlorine as to be easily torn asunder; while the fibres of the canvas bags, placed above, were not in the slightest degree affected, nor the wool and cotton within them. I have since
found that pure chlorine is pretty quickly absorbed by unbleached hemp, with the extinction of the peculiar pungent smell of the gas; but that dilute chlorine blown through among its fibres will blanch moist litmus-paper enclosed in a compact bale, without impairing the tenacity of the hemp in the slightest degree. If merely so much chlorine be introduced without agitation, through a tube, into a vessel, as to fill its lower half where a hemp package lies, the gas will not spontaneously mount to the upper half, but will concentrate and expend its energies on the organic fibres below. In like manner, if chlorine be made to exhale from capsules placed on the floor of a still apartment, containing beds and other furniture, the gas will be arrested in its diffusive ascent, and will never reach in adequate force the upper walls or ceiling to which the hot effluvia of contagious pyrexiae (as typhus, scarlatina, small-pox, &c.) naturally rise. Should the walls of the apartment have been recently washed with milk of lime, the gas will be condensed on them; but, if washed with whitening, no absorption will ensue; for chlorine does not displace carbonic acid from lime, nor does it combine with the calcareous carbonate.

We are thus clearly led to the conclusion that chlorine-gas, when used as a disinfectant, should be considerably diluted with air before it is distributed into apartments, in such a degree and manner as neither to injure furniture nor merchandise, nor materially to annoy respiration. We must throw out of view those constitutions indeed which are so delicate or fastidious as to be intolerant of even the smell of chlorine. The said aërial mixture should be introduced into the middle or upper regions, in preference to the lower, and its diffusion should be promoted by propulsion. Moist litmus paper, suspended in various parts of the chamber, will serve to show when the chlorine has done its duty.

Figures 1 and 2 (pp. 91 and 93) exhibit two forms of apparatus for disengaging chlorine in regulated quantities, for mixing it with air in any proportion, for blowing it into any infected space, and for ascertaining the degree of its dilution at any stage of the operation. Figure 1 has been constructed in the Dock-
yard at Woolwich, by order of Sir T. Byam Martin, Comptroller of the Navy, from a drawing furnished by me, the copy of that laid before the Board of Health on the 22nd of June. The object of this construction is to show how the cargo of a ship may be imbued with dilute chlorine, without injuring its quality or disturbing its position. Such an easy, quick, and safe immersion in this expurgative gaseous medium will, I presume, be deemed by all persons acquainted with the affinities of this most energetic element, to be a surer safeguard against the importation of contagion in merchandize, than the mere exposure of the goods to the air, as practised under the actual laws of quarantine. At the present crisis of the Russian cholera, the cargoes of hemp, wool, hides, &c., now in the course of arrival on the British shores from the Baltic, and immediately placed under quarantine, are so immense as to require, it has been said on official authority, the decks of ninety-five line of battle-ships for their adequate exposure. Supposing infectious fomes to exist in the merchandize, and the quarantine laws act solely on that presumption, what a formidable mass of contagion will be let loose in our atmosphere, and what a cruel duty is imposed on the sailors imured in the pestilential focus! It appears to me that the danger, expense, trouble, and delay of quarantine may be saved by a just application of the antilomie virtues of chlorine.
The cask A, fig. 1 and 2, is destined to receive the chloride of lime and muriatic acid. The strong bleaching salt with which the Messrs. Tennant supply the London market, chiefly for the use of the paper-makers, contains on an average about 29 or 30 per cent. of chlorine gas, which it most readily gives out on the affusion of any liquid acid. I find that one pound of such chloride requires for saturation of the lime, one pint (imperial measure) of the muriatic acid of the London shops, of specific gravity 1.160, and evolves a cubic foot and a half of pure chlorine. This volume diluted with about twelve times its bulk of air, is adequate to the disinfection of a small apartment. It will be convenient in practice to introduce at once into the cask A by the hole B, six or seven pounds of the chloride, diffused among seven or eight pints of water, and then to call forth the chlorine as it is wanted for distribution, by successive affusions of muriatic acid through the syphon-funnel C, remembering that every pint of the acid will disengage about a cubic foot and a half of the chlorine. The gas thus liberated will pass along the horizontal pipe D, fig. 1, into the side of the wide vertical pipe E, and falling down into the ventilating vessel, will by the motion of its central fanner F be diluted with air in any desired measure, according to the velocity of the rotation. The air of dilution is drawn in at the axis through the open upright pipe E, and the mingled gas is blown forth through the pipes G G, whence it may be conducted and applied whithersoever the operator may choose, upwards, downwards, or horizontally, by connexion with wooden or leaden pipes of communication.

Fig. 1 exhibits an apparatus, which may be got up by any ship-carpenter in a day or two with a couple of casks, one small and the other large. This has been actually done at Woolwich. Instead of turning round the axis H of the fanner, by a horizontal motion of the hand, it may be made to revolve more conveniently by a vertical motion, provided the upper end of the axis be furnished with a couple of bevelled-toothed wheels, placed at right angles to each other, as shown by the dotted line L. The stop-cock M, serves to draw off a phial-full of the gaseous mixture, for analysis, by water or milk of lime.
Fig. 2 is a more powerful and convenient form of the same apparatus, for disengaging chlorine, for diluting it with air, and for propelling the mixture along pipes with very considerable velocity, so as to ensure its thorough diffusion among the timbers of a ship, and bales of merchandize, however closely stowed. Every ship of war should be provided with such an apparatus on each of its decks, and a few minutes working of it would sweeten and disinfect, infinitely better than an hour's combustion of gunpowder wetted with vinegar, the delusive fumigation at present practised in the navy, under the appropriate title of the Devil, the patron of falsehood. The slightest consideration of the gaseous products of burning gunpowder, shows that they can exercise no decomposing influence whatsoever on contagious or fermentable filth, which chlorine unquestionably does. In fig. 2, the orifice B of the gas generator is tubular, and rises about 18 inches high, so as to preclude the chance of the relatively dense chlorine overflowing, to the annoyance of the operator. But a few turns of the fanner will draw off the chlorine from A, however quickly it may be evolved. Through the pipe B, the chloride, previously mixed with water, is introduced, and by the syphon-funnel
C, the muriatic acid is to be poured in at proper intervals. This syphon may, probably, in the present case be dispensed with, and the acid may be introduced at B. The disengaged gas will flow off along the horizontal pipe D, into the fanner F F. When its vanes are made to revolve, atmospheric air will enter freely by the open pipe B, and passing across the vessel A, will dilute the chlorine to any degree. The mixed gases will be drawn in through the pipe D, and projected with centrifugal force through any outlet in the circumference of the blowing cylinder F. The horizontal axis of the fanner revolves at one end in the conical bush of a step or stirrup I, and at the other in a stuffing-box K. With this apparatus, chlorine may be readily propelled in any state of dilution, in any quantity, and in any direction, through apartments of any kind. Such an effective application of this anti-putrescent antiloimic element will infallibly exercise an expurgatory influence, no less sweetening to the senses, than salubrious to the system; and ought to banish for ever the sham or misdirected agency of chloride of lime or chlorine, with which medical men have so often deceived themselves and the public *. Wherever chlorine has failed to extinguish infectious fomes, the operator, and not the chemical agent itself, has been in fault. Let us suppose, for example, that the fetid air of a dissecting-room is to be sweetened; and that this is attempted by setting on the table or floor a few saucers filled with chloride of lime. If the air remain fetid, is chlorine to be deemed inert and inefficacious? No, surely; for the operation was unskilfully performed. Thus also a small portion of chlorine, liberated on the floor of an apartment containing beds and furniture, may never rise in adequate force to the line of the walls where contagious virus may lurk. This remark is peculiarly applicable to the less fugacious infections, as the variolous, which require an energetic dose of chlorine. In fine, one rule may serve for the bleacher and disinfector by this element; that is, to employ it in doses proportional to the stubbornness of the colouring or morbidic matter.

* Mr. Faraday's elaborate fumigations of the Milbank Penitentiary do not fall under this censure.
The distribution of dilute chlorine through the cargo of a ship, and the due impregnation of the interior of the bales, may be easily accomplished by the above-described apparatus.

From the pipes G G, tubes of lead, leather, or the waterproof double cloth, are to be led down a few feet into the hold, under the main hatchway, so that by the action of the fanner the mingled airs may be driven through every interstice, till they envelope every package. The quantity of chlorine, and the continuance of the operation, must be regulated by the capacity of the ship, and the nature of the bales; but in general a couple of hours will suffice. All the openings in the deck should be carefully closed, except a small one near the stem and another near the stern, to permit the discharge of the atmospheric air and the ready circulation of the disinfecting gas. Eventually, traces of chlorine issuing from these openings will be observed by the smell or by the white cloud surrounding a feather moistened with water of ammonia (spirits of hartshorn). The process may now be regarded as complete; and after the interval of a few hours, all the hatchways and windows may be thrown open, and a free ventilation given to the ship. The residuary chlorine in its discharge into the air being wafted round the bodies of the sailors will disinfect their dress, and give final security against the importation of contagious fomes.

An apartment may be conveniently disinfected by placing on a shelf or support near the ceiling a small basin or pipkin, containing chloride of lime, having set over it a glass or earthenware funnel with muriatic acid diluted with about its weight of water; the beak of the funnel being partially closed with a cork, so that the acid may drop slowly down on the chloride. Eight ounces of good chloride thus treated with ten ounces of muriatic acid, will suffice to fumigate and sweeten the air of a common-sized chamber.

After the preceding observations, it will not be expected that I should ransack medical repositories, in proof of the antiloimic powers of chlorine. But less fallacious evidence may be found. In the neighbourhood of the city of Glasgow, there are several large factories, the atmosphere of certain parts of which has been for a long series of years more or less
impregnated with chlorine; I allude particularly to the chemical works of Messrs. Tennant, at St. Rollox, to those of Messrs. White, at Shawfield, and to Messieurs Monteith's calico-print field, at Barrowfield. In the last-mentioned establishment, a great many tons of chloride of lime were for many years treated every week with sulphuric acid, in order to obtain a strong aqueous solution of chlorine. When the sulphuric acid was poured into the clear watery solution of the chloride, contained in a large leaden cistern, a very considerable quantity of chlorine gas escaped into the air, which communicated its peculiar odour to the whole vicinity. Chlorine was also continually emitted from the above discharged liquor, in the course of its application to Turkey-red cloth, for producing the white figures of Bandana calicoes. Mr. George Rogers, the very intelligent conductor of this magnificent establishment, has just favoured me with the following letter relative to the anti-contagious influence of chlorine.

'My dear Sir,—In answer to yours of the 24th, I have long been convinced of the efficacy of chlorine in purifying contaminated or foul air, and in arresting the progress of contagious diseases, more particularly typhus. During the long period of thirty years that I have conducted this establishment, with a population of two or three thousand, including their families, I am not aware of a single case that could be classed as contagious; and in many hundred cases in which I have recommended chlorine in the village (Barrowfield), its good effects have been apparent in arresting the progress of typhus and other fevers.—I am, my dear Sir,

'With much respect, yours,

(Signed) 'George Rogers.'

'Dunoon, 31st June, 1831.'

Mr. White, who has given up making chloride of lime for a good many years, and who has no interest in the sale of the article, writes me, 26th June, 1831, as follows:

'All that I can state is merely the impression among our workmen, of their total immunity from fever;—and this impression is justified by the circumstance, that while typhus was
Dr. Ure on Disinfection.

prevalent in the neighbourhood, none of the workmen employed in the manufacture of chloride of lime were ever its victims.'

(Signed) 'JOHN WHITE.'

As Messrs. Tennant, the original patentees of chloride of lime, are also the greatest manufacturers of it in the world, their testimony might be received as that of interested persons. But the following document from Dr. Corkindale, physician to the gaol of Glasgow, and celebrated for his skill in medical jurisprudence, is above any such suspicion.

'Glasgow, 1st July, 1831.

'In the year 1824, a suit was brought against Mr. Tennant's chemical works at St. Rollox, on the score of nuisance, proceeding on the allegation that the fumes arising from the processes there carried on were injurious to the health of the neighbourhood. Around the works there are houses for the accommodation of about twelve families of the workmen. These persons have continued to reside there for various periods from two to twenty years. I examined the condition of these people, and made inquiries as to the history of their health during their residence, as detailed by themselves. I found that their condition, in this respect, was nearly the same as other persons of the same rank of life, in ordinary situations; but it was the uniform statement of the whole of them, that no person residing on these premises had been affected with typhus at the different periods when that epidemic was very prevalent in Glasgow. It was evident from inspection that this immunity was not owing to superior cleanliness and ventilation, for the apartments were as dirty and crowded as the ordinary habitations, where I know typhus had prevailed. The vapours from the works were various, but by far the most prominent was chlorine, rising both from the preparation of the chloride of lime, and from the treatment of the residuum for the manufacture of soda.

(Signed) 'JAMES CORKINDALE, M.D., LL.D.'

I have been favoured by M. d'Epinay, agent of the island of Mauritius to the British government, with an excellent Vol. II. Aug. 1831. H
account of the introduction into that island of the Oriental cholera, which, having recently transmigrated the middle of Asia and the north of Europe, now desolates the western provinces of Russia, and hovers like an incubus over our shores. The facts it relates will be found interesting and instructive in no ordinary degree.


‘My dear Sir,—I proceed to perform the promise I this day made of furnishing you with some details concerning the introduction of the cholera morbus into the Island of Mauritius. This disease was imported there by the British frigate, the Topaz, commanded by Captain Dumby. It is in its nature eminently contagious; and although this opinion exposed the colony to which I belong to the most violent calumnies on the part of its Governor, General Darling, and to the anger of Lord Bathurst, then Secretary of State for the Colonies, I must persist in maintaining it, because it is proved by the facts about to be related.

‘The Topaz arrived at Mauritius the 28th of December, 1819, having just come from Ceylon, where the cholera prevailed. This fact is notorious, and is indeed fully verified by the following extract from the Asiatic Mirror, published at Calcutta, the 24th of December, 1819.

‘“We announce with regret that the news brought from Ceylon are very distressing. Fevers, dysentery, and the cholera morbus are spreading in an alarming manner. The 7th regiment, and a detachment of the 45th, which have been in the island only for a week, have suffered considerably; thirteen officers of the former, and thirty soldiers of the latter, having fallen victims to this terrible scourge.”

‘The report of the physicians who visited the frigate on its arrival stated, “that the dysentery and the cholera morbus prevailed on board of it.”

‘Notwithstanding this, the Physician-General of the Forces in Mauritius and the Governor had the culpable weakness to permit communication between the frigate and the shore. The rumour being universally spread that several men of the frigate were ill of the cholera; the representation of the Colonel of
a regiment, who opposed the patients of the vessel being carried to the Military Hospital; tents mounted to receive them in the Ile aux Tonneliers; were circumstances which caused uneasiness; and the members of the commune (parish) imparted their feelings to the Governor, who gave for answer, "that he was very sorry for them, but that he was ignorant of the laws of the colony;" an ignorance supposable enough in a military man, but not the less reprehensible in a Governor.

On the 5th of November, he wrote to the commune, to inform them that the Journal of the following day would contain an opinion from the Physician-General, which would render useless every other measure relative to the disease which had prevailed on board of the Topaz.

On the 19th of November, two negroes fell down in the street, and died before there was time to assist them; and the disease began to spread through the town. On the 23rd, the frigate brought-to, and visited a boat from the shore, as it came out of the harbour, on its way to the river Rempart. The crew of this boat were soon thereafter attacked with the cholera, which they communicated to the establishment of M. Carcenau, their master, who lost forty slaves, and died himself of the same disease. This was the first plantation where the cholera showed itself, although it was six leagues from the town. It soon made the tour of the island, terrible in its first ravages, but becoming milder by degrees; more fatal in the neighbourhood of the sea, and unknown in elevated regions.

The communications with the island of Bourbon, thirty leagues from Mauritius, being open, the disease was not long in being carried thither. The inhabitants, taking alarm, formed immediately a cordon round the town of St. Denys, and the punishment of death was decreed against all who should dare to break through it. This scourge did not extend beyond the limits which wise and courageous men had here traced around it.

Very different was the case at Madagascar, into which the cholera was imported from Mauritius, and exercised the greatest ravages.

It was computed that, in our island, the number of its victims amounted to a tenth of the population; and I concur
in this estimate. It was chiefly among the lower classes, and persons given to intemperance, that the cholera was most fatal.

'A healthy regimen, great cleanliness, exercise, recreation, and courage, were found to be the best preservatives against its attack. Individuals who wore flannel generally escaped. The most successful remedy was saline purgatives, repeated in half-ounce doses every quarter of an hour, till the alvine discharge assumed a natural colour.

'I could enter into other details which go to fortify the opinion I have always entertained of the contagious nature of cholera; but I think those I have adduced, and particularly what happened at Bourbon, ought to convince you that too many precautions cannot be taken against permitting communications with vessels coming from districts infected with the malady. Believe me to be, my dear Sir,

'Your devoted Servant,

(Signed) 'AND. D'EPINAY.'

From the same. 'July 7, 1831.

'I told you that, in the Isle of France, during the cholera, we employed as a disinfector a mixture of oxide of manganese and muriatic acid. We provided small phials of it, which were carried about in all the infirmaries, and by the people who entered the hospitals. They were also carried about by the women and children; and it was remarked that none of those so protected by the disinfecting phials were attacked with the disease. Was this from the virtue of the composition, or from the confidence inspired by it? I cannot answer these questions, but content myself with stating the fact.

(Signed) 'AND. D'EPINAY.'

** The cask A (in both cuts) should have a plug-hole or stop-cock near the bottom (not shown in the figure), for discharging the liquid muriate of lime.—I have omitted to state that dyed silk may be treated without injury with dilute chlorine.
ON THE PENETRATIVENESS OF FLUIDS*

By J. K. MITCHELL, M.D., Lecturer on Medical Chemistry in the Philadelphia Medical Institute.

[The generality and importance of this paper is such that we think it quite impossible to convey any idea of it by an abstract, and feel ourselves bound to bring it before our English readers at full length.]

In 1829, I read before the Philosophical Society, a short memoir on a new method of forming gum elastic into thin plates, sheets, and bags. In some instances the balloons formed by the process then described had, when filled with hydrogen gas, the power of ascending to a considerable height in the atmosphere. Those which were confined to the atmosphere of my lecture-room, at the Medical Institute, descended again after a period of time, varying from an hour to two days. The cause of the descent, which did not seem of easy explanation, became a subject of investigation.

The gas might have escaped from the balloons at the ligature, or by permeating the dense wall of gum elastic, or by uniting chemically with the internal surface of the latter. To free the gas from the compression to which it is subjected in a balloon, I confined it in a wide-mouthed bottle, over the aperture of which I tied very firmly a thin sheet of the elastic membrane. In a few hours the descent of the cover into the cavity of the bottle gave evidence of a diminution of the contained gas, and finally the cover was burst inwards by the pressure of the atmosphere, so great had been the rarefaction of that which remained in the bottle. On weighing the membranous cover, no gain in weight could be perceived, so that I presumed that the gas had escaped. By repeating the experiment, and covering the bottle with a small bell-glass holding atmospheric air, I found, after a time, in the latter vessel, an explosive mixture, while the contents of the bottle itself were found to be pure or nearly pure hydrogen. Evidence was thus afforded that hydrogen penetrated the membrane not by any vis a tergo, for no pressure was applied, but by some inherent power of considerable amount. The facility of permeation appeared also much greater in the hydrogen than in the atmospheric air, which, if it entered at all into the bottle, did not penetrate in any appreciable quantity, when fully one-half of the hydrogen had made its escape.

In the next experiment the arrangement of the gases was altered: common air was inclosed in the bottle, and a bell-glass confined around it in an atmosphere of hydrogen. As was expected, the

* Philadelphia Journal of Medical Sciences, xiii. 36.
hydrogen entered the bottle rapidly, raised up the tense membrane, formed it into a globe, and finally burst through it, and thus made its escape from the confinement to which it had been spontaneously subjected.

The minuteness of the atom of hydrogen might readily enough account for the greater facility with which it penetrated the membrane, but could not be considered a good reason for the energy with which the penetration was accomplished. A gas having a heavy atom was therefore selected for further experiment, and carbonic acid, subjected to the same sort of confinement, was found to permeate the membrane with as great power, and very much greater facility. In succession, most of the gases were submitted to the same ordeal, and all of them found, except nitrogen gas, to exercise the same power, but with very different degrees of rapidity. The power was ascertained by comparison with common air, and the rate of action both in that mode and by comparison with each other. The depression or elevation of the membranous cover clearly indicated the escape or entrance of a gas, and when two active gases were placed one on each side of it, its rise or fall expressed the difference of rate, because each was, at the same moment, in the act of permeation, as proved by many examinations of the contents of the bottle and bell-glass.

Having once ascertained the rate of action of each gas relative to air, a prediction could be made as to their rate in reference to each other. Hence gases which operated on air with nearly equal velocity, affected the horizontality of the membrane very little when placed on opposite sides of it. Thus carbonic acid and nitrous oxide act with great facility on common air, and in nearly equal degree; and when placed on opposite sides of the membrane, penetrate it rapidly, but cause a very slow change in its position. The facts here presented warrant the conclusion, that if two gases, equally penetrant exactly, could be found, they would, under the above described arrangement, mix uniformly, without in the slightest degree altering the state of the membrane *.

The greatest possible degree of effect on the membrane arises, when we place on opposite sides of it the slowest and most speedy penetrator; for instance, nitrogen and sulphuretted hydrogen. In that case the change is immediately visible.

* Subsequently having discovered that oleifant gas and arsenuretted hydrogen have, with reference to common air, exactly equal rates, they were placed on opposite sides of a membrane, with a full expectation of sustained horizontality on the part of the membrane; which was confirmed by the result.
As in all the previous experiments, different gases were placed in comparison, I placed the same gas on both sides, and expected, for the 'sufficient reason,' no change. The experiment accorded with expectation. The membrane remained stationary.

The circumstances essential to the transmission of gases through the membrane formed an interesting subject of inquiry.

My first attempt was to produce a vacuum, by placing the gas in a bottle, and exhausting, by means of the air-pump, the bell-glass which covered it. The gases effected their escape from the bottles thus treated, with a velocity proportional to the rate of permeation already ascertained; sulphuretted hydrogen passed out more rapidly than carbonic acid, and that than hydrogen. Still as some air is always found in an exhausted receiver on the finest air-pump, I passed a tube containing carbonic acid into a Torricellian vacuum, where it very speedily escaped and caused the descent of the mercury. Even this experiment could not prove perfectly satisfactory, as mercurial vapour occupies the barometric vacuum. A perfectly empty bag carefully closed was placed in carbonic acid and nitrous oxide successively, without undergoing the slightest inflation. If a very small portion of any kind of air remained in the bag, inflation followed, provided the bag were exposed to a different gas.

By another arrangement I obtained my object more unexceptionably. Having found, by inverting a bottle holding confined gas, and thus plunging it into mercury, that no gas escaped, and that consequently mercury could not promote or sustain the permeation of the gas, I reached my object by the following means. Closing a tall cylindrical lamp-glass at one end with gum elastic, and filling it with mercury, it was placed, so filled, on the shelf of the mercurial trough, having the end closed by the membrane uppermost. Through this fine film the mercury could be plainly seen in close contact with its under surface, while the deep depression of the membrane showed the power of the column of mercury by which it was drawn down. By leaving it in the air, or by placing over it a bell-glass of any gas, more slowly, but at their settled rates, the gases penetrated the membrane and accumulated in the cylinder, thus permitting the descent of the mercury. The process continued long after the mercury had abandoned the surface of the membrane, and the space was occupied by the gas, in, of course, a rarefied state.

It became then evident, that anything which could remove the gas from the surface of the membrane at which it had arrived by penetration, would continue its transmission. Of course then
agents chemically attractive of a particular gas, when placed beneath the membrane, would promote its permeation. In fact, lime water and solution of baryta were rapidly carbonated by the transmission of carbonic acid, and sulphuretted hydrogen almost instantly precipitated the lead of the acetate placed in solution on the opposite side of the membrane, which became black on the side of the solution. A neater mode of performing this experiment is the following: Inject by means of a gum elastic bottle and pipe, into a very small bag of gum elastic, stretched until fully transparent, a solution of the substance to be acted on. Carefully tied, washed, and dried, the bag is to be passed up through mercury into a receiver holding the gas, which for solution of baryta should be carbonic acid, and for that of acetate of lead, hydrosulphuric acid. In a few moments, in the former case, a white coat is seen to completely line the internal surface of the bag, and in a few minutes to fall down and accumulate at the bottom of it. In the latter case, the inner coat is dyed indelibly black. In either case, if water be alone placed in the bag, it will absorb a considerable quantity of either of these gases, and their presence may be ascertained by the usual tests.

If any suspicion had arisen that the gases escaped or entered by the route of the space included under ligature, it was dissipated by all the experiments mentioned in the last section; inasmuch as in the first experiment, that with the lamp-glass, the gas was seen to stud beautifully the under surface of the membrane, standing on it in minute drops or bubbles, mistaken at first for water. In the experiments with baryta and lead in bags, the whole surface was covered, the precipitation taking place only there. Especially was it manifest in the last experiment, where the inner surface was stained black, while the solution remained clear and colourless. The gas, therefore, penetrates through every part of the membrane.

Being desirous of ascertaining more accurately the relative facilities of transmission, I solicited the assistance of my friend and pupil, Professor J. K. Finley, to whose patience, skill, and delicate manipulation, I owe much of the certainty of the following experiments.

Having constructed a syphon of glass with one limb three inches long, and the other ten or twelve inches, the open end of the short leg was enlarged and formed into the shape of a funnel, over which finally was firmly tied a piece of thin gum elastic. By inverting this syphon and pouring into its longer limb some clean mercury, a portion of common air was shut up in the short leg, and was in communication with the membrane. Over this end, in the mercu-
rial trough, was placed the vessel containing the gas to be tried, and its velocity of penetration measured by the time occupied in elevating to a given degree the mercurial column in the other limb. Having thus compared the gases with common air, and subsequently by the same instrument, and in bottles, with each other, I was able to arrange the following gases according to their relative facility of transmission, beginning with the most powerful:—ammonia, sulphuretted hydrogen, cyanogen, carbonic acid, nitrous oxide, arsenuretted hydrogen, olefiant gas, hydrogen, oxygen, carbonic oxide, and nitrogen.

Ammonia transmitted in 1 minute as much in volume as sulphuretted hydrogen in 2½ minutes—cyanogen, 3½—carbonic acid, 5½—nitrous oxide, 6½—arsenuretted hydrogen, 27½—olefiant gas, 28—hydrogen 37½—oxygen, 1 hour and 53 minutes—carbonic oxide, 2 hours and 40 minutes.

Nitrogen has a rate of penetration so low as to be difficult to ascertain, because there is no gas of a lower rate with which to compare it. Only by causing it to pass through a membrane by means of a column of mercury, is the fact of its transmission known. In that way, the quantity being compared with that of carbonic acid, its rate was found to be about three hours and a quarter *. This experiment, made but once, is not confidently relied on; but the rate of nitrogen is unquestionably less than that of carbonic oxide.

Chlorine immediately altered the texture of the membrane, as did muriatic acid gas, sulphurous acid, nitric oxide, and some others, so that it was impossible to reach, for their rate of penetration, accurate results.

In every case the movement of the gas through the membrane became progressively slower, until it totally ceased; and finally, but more slowly, the mixed gas returned, as indicated by the descent of the column of mercury. The retrogradation ceased only when the two columns came to equilibrium, or, failing the possibility of that, when the mercury in the shorter limb had reached the membrane, through which mercury has not been found able to penetrate.

Acquainted with the fact, and the relative rate of the penetrativeness of gases, the degree of force became the next subject of inquiry:

* A vessel filled with atmospheric air and closed by gum elastic was submerged under water for two weeks, when it was found to contain only nitrogen gas. Possibly this arrangement may furnish a new eudiometer. It offers a new mode of obtaining nitrogen gas.

A phial containing atmospheric air, after being closed by a membrane, was placed in a receiver holding nitrous oxide. In about two weeks only nitrogen was found in the phial. These facts show the mechanically sluggish character of nitrogen gas: with its chemical inactivity we have been long acquainted.
that it was considerable could be seen by looking at the stout membranes broken by it.

By greatly increasing the length of the taller limb of an inverted syphon, similar to the one already described, I was able to bring to bear on the common air imprisoned in the shorter limb, a very considerable column of mercury. Up to a pressure of sixty-three inches of mercury or rather more, equal to more than the power of two atmospheres, the penetrative action was found capable of conveying the gases, the subject of the experiment, into the short leg, through the gum elastic membrane. The entrance of the gas into the short leg was expressed by the ascent of the long column of mercury in the other, which as it entered, it was compelled to heave up. At the height of sixty-three inches, the membrane, though supported by cloth, could scarcely sustain the weight, and would not bear any increase of height. Although, therefore, at present, I do not know the limit of this power, I believe it will be found very much greater, because the power of the column which was tried did not, until a leak was sprung, seem to very sensibly affect the rate of entrance.

To the mind of a physician, the repetition of the foregoing experiments, substituting animal membranes for gum elastic, would naturally suggest itself. Should animal membranes present the same phenomena, the interest of the investigation would be vastly enhanced, and a very important service done to the cause of 'Physiological Medicine.' That animal membranes would act in the same manner was rendered probable by the well-known experiments of PRIESTLEY, who affected by means of oxygen the colour of blood confined in a bladder. It had also been observed by him that a closely tied bladder, containing hydrogen gas, is found, after a considerable lapse of time, to contain only atmospheric air, and that in quantity perhaps equal to the hydrogen lost. Several other facts of the same kind are detailed by him. Finally in the Journal of the Royal Institution, I find the following 'Notice of the Singular Inflation of a Bladder. By THOMAS GRAHAM, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.'

'In the course of an investigation of mixed gases through capillary openings, the following singular observation was made. 'A sound bladder with stop-cock was filled about two thirds with coal gas, and the stop-cock shut; the bladder was passed up in this flaccid state, into a bell-jar receiver, filled with carbonic acid gas over water. The bladder was thus introduced into an atmosphere of carbonic acid gas. In the course of twelve hours, instead of being in the flaccid state in which it was left, the bladder was found distended to the utmost, and on the very point of bursting, while most of the carbonic acid gas in the receiver had disappeared. The
bladder actually burst in the neck, in withdrawing it from under the receiver. It was found to contain thirty-five parts carbonic acid gas by volume in one hundred. The substance of the bladder was quite fresh to the smell, and appeared to have undergone no change. The carbonic acid gas remaining without in the bell-jar had acquired a very little coal gas.

' The conclusion is unavoidable, that the close bladder was inflated by the insinuation of carbonic acid gas from without.

' In a second experiment, a bladder containing rather less coal gas, and similarly placed in an atmosphere of carbonic acid gas, being fully inflated in fifteen hours, was found to have acquired forty parts in one hundred of this latter gas, a small portion of coal gas left the bladder as before.

' A close bladder, half filled with common air, was fully inflated in like manner, in the course of twenty-four hours. The entrance of carbonic acid gas into the bladder depends, therefore, upon no peculiar property of coal gas. The bladder partially filled with coal gas did not expand at all in the same jar containing common air or water only.

' M. Dutrochet will probably view, in these experiments, the discovery of endosmose acting upon aeriform matter, as he observed it to act upon bodies in the liquid state. Unaware of the speculations of that philosopher, at the time the experiments were made, I fabricated the following theory to account for them, to which I am still disposed to adhere, although it does not involve the new power.

' The jar of carbonic acid gas standing over water, the bladder was moist, and we know it to be porous. Between the air in the bladder and the carbonic acid gas without, there existed capillary canals through the substance of the bladder filled with water. The surface of water at the outer extremity of these canals being exposed to carbonic acid gas, a gas soluble in water would necessarily absorb it. But the gas in solution, when, permeating through a canal, it arrived at the surface of the inner extremity, would rise as necessarily into the air in the bladder and expand it. Nothing but the presence of carbonic acid gas within could prevent the disengagement of that gas. The force by which water is held in minute capillary tubes might retain that liquid in the pores of the bladder, and enable it to act in the transit of the gas even after the pressure within the bladder had become considerable.'

A careful perusal of Mr. Graham's notice will excite in every one who knows the value of experimental interrogation, an expression of surprise, at the failure, on the part of that intelligent and ingenious chemist, to pursue, in the only true spirit of science, the investigation of a principle, one of the most striking manifestations of which had thus been placed conspicuously before him. Content with a single additional experiment, he comes, in the ancient method, to immediate conjectural explanation, and has thus lost an easy opportunity of making a beautiful, and, perhaps, extensively useful
discovery. Made at an earlier period, his observation was published
in the Journal for October, 1829, and has since attracted appa-
rently no scientific attention. Such is usually the fate of the most
pregnant facts which are not perceived to bear on some generality.
This one passed from my mind along with all the other isolated phe-
nomena of that number of the Journal, and only shone importantly
when illuminated by the reflected light of an extensive principle,
subsequently developed. These remarks are made, not to throw any
discredit on the character of the accomplished gentleman to whom
they refer, but to correct the baneful error of ancient dogmatism,
which yet weighs so heavily on the cause of nature and truth. It
was true that the carbonic acid entered a closed bladder, and that
too with power, and it was equally true, that oxygen had done
the same thing in the experiment of Priestley, and that, in his
hands, even common air had penetrated to replace hydrogen in
a similar viscus, and yet he ascribed the phenomenon observed
by him to the capillaries, and the conducting power of aqueous
canals.

In what manner the power of 'rising into the air' was given, and
whether it was dependent on the force of water, or some other cause,
does not and could not be made to appear from the single fact, as
presented by Mr. Graham. A very little practical interrogation,
following the word just uttered by nature, would have obtained an
answer fraught with new and important truth.

But to return to the immediate subject of this essay.—Analogy,
the experiments of M. Dutrochet, and the observations of Priestley
and Graham, gave me almost the certainty of finding animal mem-
branes performing relatively to the gases the same function which
belongs to those formed of the inspissated juice of the Jatropa elas-
tica. Accordingly, each gas was subjected to the action of animal
membranes, which replaced the gum elastic at the mouth of the
short limb of an inverted syphon. Dried bladder, and gold-beater's
skin, moistened to cause an approach to a normal state, and sections
of various recent tissues, were successively tried, and found to act
on the gases in the manner and order in which they were affected
by gum elastic. The more recent the membrane, the more rapid
and extensive the effect produced; and in living animals the trans-
mition was very rapid.

Besides the estimates of comparative movement made with the
syphon, experiments in a different manner were resorted to, to more
clearly show the general truth. Thus a piece of the strong intestine
of a goose connected with the oesophagus and gizzard, being par-
tially inflated with common air, and firmly tied, was left in an
atmosphere of carbonic acid, where in less than ten minutes the inflation caused it to burst. On repetition of this experiment and examination before fracture, a very large quantity of carbonic acid gas was discovered to have entered the intestine. Crop, bladder, &c. &c. of recently killed animals produced exactly similar results. Perhaps the following experiment will be esteemed even more satisfactory. Carefully removed from the chest of a snapper, (Testudo serpentina,) its lung was partially inflated with common air, and confined there by a ligature on the tracheal tube. Exposed in this state to an atmosphere of carbonic acid, or nitrous oxide, it became very soon fully inflated by the gas to which exposed, as subsequently proved by chemical examination. Less than half an hour of exposure sufficed for the full inflation of the lung, which was removed only when it threatened to burst. Containing a portion of nitrogen, it was left exposed all night to an atmosphere of oxygen, yet scarcely enough entered to signify its presence; in quantity superior to that which is held in atmospheric air. A taper appeared in it somewhat brighter than before its immersion.

In a subsequent experiment, the two lungs of a snapper having been extracted, were inflated respectively, with common air, and carbonic acid gas. So prepared, each lung was surrounded by a bell-glass containing an atmosphere of the other gas, so that common air surrounded the carbonic acid, et vice versa. That lung which contained common air soon burst by the infiltration of carbonic acid, while the other collapsed by its escape.

In concluding the series of experiments on the question of fact, some were made on living animals. A quantity of solution of acetate of lead having been thrown into the peritoneal cavity of a young cat, sulphuretted hydrogen was discharged from the pipe of the generating retort, directly into the rectum. In four minutes the poisonous gas killed the animal, giving to it, because of enormously dilated pupils, a very wild aspect. Instantly on its death, which was itself an affair of a moment, the peritoneal coat of the intestines, and the walls of the cavity in contact with them, were found lined with a metallic-looking precipitate, adherent to the surface, and susceptible of removal by nitric acid, moderately diluted. It was the characteristic precipitate of sulphuretted hydrogen when acting on lead. When, in another experiment, the abdominal cavity was almost instantly opened, only the intestines and stomach presented the bronzed aspect; the peritoneum of other parts, and the bladder, appeared of their natural colour, thus proving that the gas had infiltrated, and not passed through any rent or fracture, an event
which would have stained the whole of the lining membrane of the cavity, and dyed the bladder. This experiment forcibly reminded us of that where the internal surface of a gum elastic bag holding lead water, was stained black by sulphuretted hydrogen, while the solution continued pellucid.

In another experiment on a cat, a solution of acetate of lead was placed in the thorax, and sulphuretted hydrogen in the abdomen. Almost immediately, on the entrance of the sulphuretted hydrogen into the abdominal cavity, death ensued, with the same dilatation of pupil as before. On inspecting the thoracic side of the diaphragm, which was done as quickly as possible, the tendinous part of it displayed the leaden aspect of the precipitate by sulphuretted hydrogen. Many years ago, in 1823, while engaged in investigating Magendie's theory of venous absorption, I coloured the diaphragm of a living cat blue, by placing a solution of prussiate of potash on one side, and that of sulphate of iron on the other. At that time I supposed the effect to be vascular, but the experiments on membranes of gum elastic afford an explanation which more rationally refers it to organic molecular infiltration; for, in such membranes, vessels cannot possibly exist at all; and as animal membranes act in a manner so perfectly accordant with that of the coagulated vegetable juice, it would be judging against evidence to refer their agency to widely different causes. At the same relative rates, with the same power, and that a great one, they could scarcely act, in obedience to causes so dissimilar as those alluded to.

Every one who has read the beautiful memoir of Dutrochet, on 'L'agent immédiat du Mouvement Vital, &c.,' and who has, as nearly all have, suffered their belief to be swayed by his eloquence of fact, method, and style, will, on a cursory glance at the experiments detailed in this paper, refer them to the 'New power' so ably contended for by the French naturalist. That they depend on the same power cannot be reasonably questioned, whether that power be one long known or recently discovered. In his experiments made exclusively on liquids, and developed with surpassing good fortune and sagacity, he proved the transmission of liquids through animal membranes, and saw them penetrating, too, at different rates, some solutions passing rapidly, some with greater slowness, some in scarcely appreciable quantity, and some never passing at all. Their force, too, he found to be of estimable amount. In fact, every aspect of the two sets of experiments tends, more and more clearly, to induce a reference of them to one and the same cause, whatever that cause may be. Although the facts presented by him demon-
strate all this, yet M. Dutrochet did not perceive it, as is evident from his reference of the phenomena to a source to which, in latter years, the French naturalists and philosophers have been accustomed to look with almost superstitious reverence. Electricity is the great key of scientific explanation; and the theory of Du Fay is relied on, though badly itself sustained, as the point d'appui of almost all other theories. M. Dutrochet has accordingly ascribed the transmissions to that power, and supposed, in the very teeth of some of his most striking facts, that the current was from a less dense to a more dense fluid, or from positive to negative, dependent not on an inherent power of infiltration, and of course for the same membrane always the same, but varied, or even inverted, at pleasure, by arrangements productive of supposed electrical powers. He says, p. 139,

'Ces résultats nous font déjà pressentir que l'impulsion qu'éprouvent les liquides dans ses expériences, dépend d'un courant électrique déterminé par le voisinage de deux fluides de densité ou de nature chimique différentes, fluides que sépare imperfection une membrane perméable. Cette membrane ne joue évidemment aucun rôle propre dans cette circonstance; elle ne fait fonction que de moyen de séparation entre les deux fluides auxquels elle est cependant perméable: les liquides la traversent, soit dans un sens, soit dans l'autre, au gré de l'action réciproque des deux fluides qui baignent ses parois opposées.'

As he used water and solutions in water, by which the former became denser, he found, as might be expected, that it infiltrated the tissue more readily than most of its solutions; hence, in such cases, the water penetrated more quickly than they, and the current usually set most rapidly from less dense to more dense. But when he used essentially different liquids, he yet found the water going through at its high rate, as we perceived to be the case with sulphuretted hydrogen and ammonia. Water traversed the animal membrane rapidly to join alcohol, which, according to his electrical theory, should not have been the case, as the alcohol is less dense than water. For this and some other exceptions Dutrochet attempts to account, by reference to influence derived from chemical qualities.

If, however, as in the case of the gases, two liquids of different rates of penetrativeness be placed on opposite sides of an animal membrane, they will in time present the greater accumulation on the side of the less penetrant liquid, whether more or less dense, but will finally thoroughly and uniformly mix on both sides, and at
length, if any pressure exist on either side, yield to that and pass to the other side.

As some substances have no penetrativeness, such as milk or blood, or at least their solid parts, the water placed on the opposite side of the membrane alone moves, and it is only after the decomposition by putrefaction, and consequent formation of a new fluid having penetrant properties, that any current sets in the direction opposite to that of the water. To prove this, it is only necessary to show that alcohol penetrates gum elastic much more rapidly than water; and that, therefore, when that kind of membrane is interposed between them, the greater current is from alcohol to water, and not from water to alcohol.

A hollow glass cylinder, open at both ends, was closed completely by two membranes of gum elastic, having been previously perfectly filled with alcohol. It was then sunk in the large pneumatic trough of my laboratory, where it remained one week. At that time it presented a concavity at each end, of decided depth, proving the escape of a considerable quantity of alcohol. On the other hand, a similarly prepared vessel filled with water and submerged in alcohol presented at the end of a week well-marked convexities, demonstrating the insinuation of alcohol. If it be contended that the nature of the membrane affects and even reverses the electrical state, it may be well said in reply, that there is no analogy for that, and moreover, the same membrane acts under the movement of gases precisely as an animal membrane. The supposition would invest it with a most Protean character.

In making experiments for the preparation of gum elastic by ether, that liquid was found to readily infiltrate its tissue. Alcohol has been already shown to penetrate it better than water, and water enters its substance so slowly, that a bag of a thinness productive of almost perfect transparency, and containing four ounces, two drams, and fifty-seven grains, lost by evaporation but eight grains in the first period of twenty-four hours, and fifteen grains during the next three days. Viewing these facts, a prediction was founded on them relative to the effect of placing ether in contact with one surface of such a membrane, while alcohol or water occupied the opposite surface. As was expected, the greater quantity accumulated on the side of the less penetrative substance, and the ether always caused, by its transmission, an augmentation of liquid on the side of the alcohol or water. Using animal membranes, facts of a similar kind, previously ascertained, led us to anticipate the opposite result. According to expectation, water being most penetrative, passed through
Mitchell on the Penetrativeness of Fluids.

so much more rapidly than ether or alcohol as to swell the amount of liquid on their side.

When alcohol is largely diluted with water it penetrates an animal membrane more easily itself, and offers to the pure water which reaches it from the opposite side less invitation to infiltrate it, according to a law of progressive diminution, pointed out by our experiments on gases. Such a diluted portion of alcohol placed by M. Dutrochet in his endosmometer, and raised above the level of the pure water on its outside, found, in the force of the higher column, sufficient cause for its escape, which continued until the level was reached, when action apparently ceased. If the level be obtained at the commencement of the experiment, either no appreciable change is observed, or the movement is unquestionably in a direction contrary to that stated by Dutrochet. So, when gases are permeating in opposite directions any interposed membrane, the penetration soon begins to lessen, because there is on either side less porosity unoccupied, and there is also in them the repellent character of their gaseous state. M. Dutrochet reconciles these apparently contradictory facts to his system, by supposing chemical influence to produce the first, and electricity the second. In either case, he does not appear to dream of independent and original powers of penetration, by which the liquid comes through to the opposite side of the membrane, remaining in its tissue, or passing on by a similar power of infiltration into new matter, or, such matter being absent, accumulating on that side by the influence of mechanical power, or electrical excitement, or chemical combination, truths adequately demonstrated by my experiments on gases.

The blinding effect of preconception on the most philosophic and candid mind can perhaps have no better exemplification than is afforded by what M. Dutrochet says relative to the point of accumulation, when a diluted acid and water were placed on opposite sides of an animal membrane. As alkalies produced towards them a current for the support of his electrical theory, acids should be found to set the current towards water, and he found it so. In my experiments, the greater current was always towards the acid, and not from it; and I find that Dr. Weidemeyer (Untersuchungen über der Kreislauf des Bluts, &c.) has made the experiment with a like result. On reference to Dr. Togno's experiments (Amer. Journ. of Med. Sci.), which were chiefly repetitions of those of Dutrochet, we perceive that he does not seem to be satisfied perfectly with the report of Dutrochet on this subject. Let any one desirous of testing this matter, tie a piece of animal membrane over the end of a hollow
glass cylinder, partially fill it with diluted sulphuric acid, and place it in a vessel of clean water, so as to bring the two columns to a level. In a few hours the column holding the acid will rise considerably above that of the clean water, proving the greater current to set from water to acid, and not from acid to water. Tests, however, show that some acid does pass the membrane.

To feel assured of the error of Dutrochet, I repeated the experiment in another form. A tube of five-sixteenths of an inch in diameter, ending in a funnel-like extremity of an inch and a quarter, was covered at its broad end by animal membrane, then partially filled with diluted acid, and placed, membrane downwards, in clean water, so as to bring both columns to a level. Instantly the rise in the narrow tube was perceptible, and amounted to nearly half an inch in half an hour. Reversing the order, by placing the clean water in the tube, and the diluted acid without, as sudden and progressive a descent of the column of clean water was observable. Tests, after a short time, betrayed the percolation of some acid, and, finally, in every case the liquid became uniformly acidulous throughout, and the two columns fell to a common level—an event which may always be expected, unless the combination produced by transmission is not penetrant.

Water may be removed from the surface of a membrane at which it has arrived in many and various methods. Invitation may be given to it by a column of mercury contained in a hollow cylinder closed above by animal membrane. Water readily passes through, may be seen studding in drops the surface of the mercury, gradually covering the under side of the membrane, causing at length the separation and descent of the mercury, and continuing to enter the cylinder until the mercurial column sinks to the level of the general contents of the trough. There the action ceases, but if the water placed above the membrane be now removed, the mercurial column will again rise, and all the water having escaped through the membrane by the process of infiltration into the atmosphere, the mercury will be finally seen in close contact with the membrane from which it had receded. Sometimes before the completion of the process a change takes place in the condition of the animal matter, and some gas being introduced below suspends the ascent of the mercury.

* This fact I demonstrated to Dr. Togno.

† A new hygrometer was suggested by this experiment, of which I purpose giving an account to the Philosophical Society.
A sponge *slightly* moistened, or dry oat-meal, or any other absorbent, placed by means of a moderate weight closely in contact with the membrane, will, by absorbing the water, cause its continued permeation.

Even *vis a tergo*, as in the instance of the gases, will produce infiltration where there exists no other cause of penetration. Over the end of the short limb of an inverted syphon was tied a piece of bladder, and over that, and *in close contact with it*, was also secured a piece of sheet caoutchouc. Water was then placed in the short limb in communication with the bladder, and thus left for a few hours without compression. No appreciable amount of infiltration ensued. But, in a short time after a column of mercury had been placed in the long limb, water was plainly seen to insinuate itself through the bladder, and to raise up and separate from it the more elastic membrane which surmounted it. After all the water had passed into the space between the two membranes, the syphon was placed in its ordinary position, the end of the long limb resting in the mercury of the trough. Soon the water repassed the bladder, ascended through the short column of mercury lying above it, and collected in the curve which then formed the pinnacle of the apparatus.

Another fact, in itself important, bears forcibly against the electrical theory of Dutrochet. To try the absorbent power of the dermoid tissue, pieces of it in a recent state were tied, cuticle outwards, over bottles which contained common air, or carbonic acid gas. Over the bottle which held carbonic acid was inverted a jar of common air, and over that holding air was placed a jar of carbonic acid. The more penetrating gas was, in the first case, in contact with the cuticle, and in the other, with the dissected under surface of the skin. A trial of the contents, after twenty-four hours, showed that much more carbonic acid had penetrated in that apparatus where it was applied to the cuticle, than in the other. As in that case it had gone from the jar into the bottle of common air, while in the other case very little carbonic acid gas had escaped from its receptacle, I filled it again, and tied over it a piece of skin with its cuticle looking inwards. In twenty-four hours the carbonic acid was equally diffused through both bottle and jar. Two similar sections of intestine were slightly inflated with common air, one of them being turned inside out. Both having been carefully tied at the ends, were placed in identically the same carbonic acid in vessels of equal size. It was soon apparent that the one which had been inverted, filled itself
most rapidly, and although rather less than the other, soon greatly exceeded it in size and hardness. After remaining so exposed for eighteen hours, vessels of common air were placed over the distended bags, when a diminution of volume became in time apparent, and was more rapid considerably in the specimen which had not been inverted. It appears, then, that the transmission of a gas is easiest where it is placed on the cuticular or mucous surface of an animal membrane, rather than on its cellular or peritoneal surface,—a fact to be kept in view in rating the transmissibility of the different gases or liquids. The fluids should be compared under exactly similar circumstances, standing in the same relation to the surfaces of the membrane used.

In the following experiment, made with great precaution, we perceive a result distinctly indicative of the superior penetrability of the cuticular surface. Over the mouths of two phials, accurately filled with alcohol, weighing, according to a Pese-Ether, thirty-five and a quarter, were tied two pieces of human skin. In one the raw side presented, in the other the cuticular side. Both were placed mouth downwards in similar specimens of water, with columns of equal altitude. After the lapse of twenty-four hours, the alcohol was examined, and found to weigh more, by at least one degree, in the phial which presented the cuticle to the water. In it the thermometer sunk to thirty-three and a half, while in that which presented the dissected surface to the water it fell only to thirty-four and a half. The one had been reduced by the water one degree and three-fourths, and the other only three-fourths of a degree.

In all these cutaneous experiments, we perceive not only the agency of the membrane itself, but even that of its respective surfaces, so that we are not at liberty to admit the assertion respecting the action of the liquids, as independent of the influence of the intervening membrane.

In truth, it is now manifest that the liquid, if penetrative, permeates a given tissue at a rate dependent on the character of tissue and power of penetration. If on the opposite side there exist a substance or power capable of occupying or removing it as fast as, or faster than the membrane delivers it, the actual rate of transmission will be as high as is possible; but if not so capable, the accumulation will be at a lesser rate, and will represent the degree of permeability of the inviting substance alone. Thus, for illustration, if ether can convey away water as fast as, or faster, than the membrane can transmit it, the rate of penetration will be the greatest possible, and
will represent the full penetrability of that membrane by water. But if ether is less penetrable than that membrane, the rate of accumulation will not represent the power of the animal tissue, but that of the ethereal interstices, which, on the supposition, is less.

The power of this process in liquids, like that of the gases, is not yet measured. It is the power of infiltration in all such cases, and must be eminently great. Like all processes having dependence on molecular action, this one is influenced by electricity, when that is brought to bear on it, but we can scarcely, after a fair estimate of the value of facts, see anything more in the power than that of common interstitial infiltration, a power marvelously great, but insusceptible of demonstrative reference at present to any known cause.

The amount of force having been shown to greatly exceed that of atmospheric pressure, we feel assured that the interstices are penetrated not by any vis a tergo. It must therefore be attributed to some species of attraction, the force of which, as shown by the condensation of some gases by charcoal, sometimes equals a power of forty atmospheres, or nearly six hundred pounds on the square inch, a power amounting nearly to that of steam, at its maximum density*. It is not chemical, because the quantity absorbed bears no relation to known affinities; it is not homogeneous attraction, for it takes place solely among dissimilar substances, and often subverts the condition produced by that power, as in some cases of solution.

After having proceeded thus far with my argument and experiments, I felt as if it were important, if not essential, to my positions, to test the power of gum elastic as an absorber of gases, independently of the artificial arrangements which brought different gases to the opposite sides of it. For that purpose I selected a hollow cylinder of gum elastic, with thick parietes about an inch in length. This specimen was placed in a cylindrical graduated test-glass, filled with carbonic acid gas and placed over mercury. In less than one minute the mercury began to rise, and in eight hours, during which the observer was absent, it had risen to a considerable height. A rough attempt to measure the bulk of mercury raised, and of gum elastic used, showed that nearly an equal volume of

* Found by comparing the experiments of Cagnard de la Tour with those of the Committee of the Institute of France.
carbonic acid had been absorbed by the caoutchouc. A piece of dry bladder was subjected to the same treatment, and produced a similar rise of the column of mercury. Macerated in water for an hour, and then wiped well with a dry towel, so as to obtain dry surfaces, the same piece of bladder was again placed in the gas over mercury, and produced a diminution apparently equal in quantity to that which, when dry, it occasioned.

The bulk of the gum elastic was considerably increased by the infiltration, so that, although easily placed in the glass vessel, it was of difficult removal. This fact, added to that of the thorough penetration by water of an animal membrane macerated in it, shows how much of the phenomena described in this paper is attributable to the organic molecular infiltration. The remainder of the effect is dependent on the moleculo-porous relation of the gas or liquid to the substance beyond, into which infiltration carries the permeating substance. If the recipient beyond the membrane be as active as the membrane, or more so, all that the membrane brings to its surface will be transmitted as fast as it arrives; but if that recipient be of inferior penetrability, less will pass on than the membrane could carry through, and in that case the rate of penetrativeness of the substance relative to the membrane is inappreciable. Any gas penetrates another gas better than it does any solid, hence we obtain for them the true rate. But liquids penetrate each other sometimes less rapidly than at the rate of the transmission through the membrane. Such cases do not show the rate of transmissibility by the membrane, but of reception beyond.

[To be continued.]
Proceedings of the Royal Institution of Great Britain.

FRIDAY EVENING MEETINGS, 1831. (CONTINUED.)

April 15th.—Mr. J. F. Daniell on the Forms and Attractions of the Particles of Crystals.—The subject of this evening forms the matter of the paper at page 30 of the present Number of this Journal.

The recent experiments made in Edinburgh by Mr. A. Trevelyan, on the production of musical sound during the transference of heat, by conduction, from hot pieces of metal to cold masses of lead, were repeated before the members, with apparatus brought from Edinburgh, by Mr. Addams.

April 22d.—Mr. Marshall on the Origin and Utility of Cow-pox; with the Causes of Failure in the practice of Vaccination.—Mr. Marshall introduced the subject by a short account of Dr. Jenner and his exertions. He then proceeded to notice the effects of the practice of vaccination, and the causes of its occasional failure. From tables it appeared that the annual mortality in cases of small-pox was reduced in Copenhagen from 450 to 9; in Prussia the average was as 12 to 1; Berlin, in 1819, only 25 had died, being about 1 in 8000; Bavaria, in 11 years, only 5 had died; Anspach, the disease had been completely exterminated; Norwich, in one year the small-pox cut off more persons than any disease, except the plague; Edinburgh, similar havoc; London, in one year 13,000 died; Russia, from 1804 to 1812 there were upwards of 1,200,000 individuals vaccinated.

Mr. Marshall then stated the various causes of failure, as age of virus, want of care, bad selection, &c. &c.; and the precautions under which vaccination might be considered as a thorough barrier to the small-pox.

April 29th.—Mr. Faraday on Mr. Trevelyan's recent Experiments on the Production of Sound during the Conduction of Heat.

Mr. Trevelyan had remarked that when a heated poker was laid upon a table, so that the knob rested upon the table, but the hot part upon an interposed block of cold lead, regular musical notes were frequently produced. By extending his experiments, he found that a better form than that of a poker might be used for the hot metal: a piece of brass about four inches long, one inch and a quarter broad, and half an inch thick, should have a groove of one-eighth of an inch in width, formed down the middle of one of the broad faces, and then that face bevelled from the edges
of the groove on each side. Being now placed with the groove downwards upon a table, and shaken, it rocks to and fro, and is in right condition for the experiment. It is convenient to fasten a brass wire, terminated by a knob, to one end of this rocker, so as to act as a prolongation of an axis: it renders the whole arrangement steady and regular in its action. When this piece of metal is used instead of the poker, musical sounds are almost always produced. The surface of the lead upon which it rests should be clean.

The peculiar effects exhibited in these experiments depend upon the occurrence of isochronous vibrations performed by the rocker. When by loading the rocker these are rendered slow, they become visible: but when they occur with sufficient rapidity they produce the necessary result, a musical note, of higher or lower pitch, as the vibrations or tappings are more or less numerous. It often happens that other and extraneous sounds, as those due to the ringing of the metal, the vibration of the table, or subdivisions of the whole vibrating system, mingle with the true sound produced by the blows of the rocker; these were referred to and illustrated, and a method shown of easily distinguishing the latter from the former: it consisted in pressing perpendicularly with a small stick or pointed metal rod on the back of the rocker, exactly over the groove, so as to make the vibrations quicker, but not to disturb their regularity; the true sound of the beats of the rocker immediately rises in pitch, and may be sometimes made to pass through an octave or more at pleasure, falling again as the pressure is removed.

As the sound was evidently due to the rapid blows of the rocker, the only difficulty was to discover the true cause of the sustaining power by which the rocker was continued in motion, whilst any considerable difference of temperature existed between it and the block of lead beneath; this Mr. Faraday referred to the ultimate expansion and contraction, as Professor Leslie and Mr. Trevellyan have done generally; but he then gave a minute account of the manner in which, according to his views, such expansion and contraction could produce the effect. When the heated rocker is reposing upon a horizontal ridge of lead, it touches at two points, which are also heated and therefore expanded, and form two hills; when one side of the rocker is raised, the point relieved from its contact is instantly cooled by the neighbouring portions of lead; the expansion ceases, and the hill falls. When the rocker, therefore, is left free, the raised side descends through a greater space than that through which it was lifted; and also to a lower level than the other side: in consequence of which a momentum is given to it, which carries its centre of gravity beyond the point to which it would pass if there had been no alteration in the heights of the sustaining points. It is this additional force which acts as a maintaining power; it recurs twice in each vibration, i.e. once on each side. The force is gained by the whole rocker being lifted bodily by the point on which it is for the time supported, and comes into play by the side
of the rocker which is descending, having a greater space to fall through than that which is passed over by the mere force of its momentum during its previous rise. A curious consequence of this action is, that the force which really lifts the rocker is on one side of the centre of gravity, whilst the rising side of the rocker itself is on the other.

This, however, is not the only maintaining cause or mechanical force generated by the alternate expansion and contraction of the lead. If the vertical direction of the forces be put out of consideration for a time, and the two points of support be examined, it will be found that whilst the rocker is quiescent, both points (with their neighbouring parts) being heated, will expand and compress the lateral portions of the lead, until the tension of the latter is equal to their own. When one side of the rocker is raised, the point that it rested upon is instantaneously cooled, and therefore contracts; but as the neighbouring parts retain their tension, they move towards the contracting part, the other point of support moving with the rest. When the rocker returns in its oscillation, it reheats and re-expands the first point of support, whilst the second, now out of contact, is cooled and contracted, and the first point, therefore, moves towards the second. A necessary consequence of this mutual relation of the points is, that the one under process of heating is always moving towards the other which is under process of cooling; and, consequently, towards a perpendicular from the centre of gravity; but as it is at the same time the supporting point to the rocker, that supporting point is, by irresistible impulse, carried in a direction under and towards the line passing from the centre of gravity towards the earth, at the same instant that the centre of gravity of the rocker is, by the movement of the latter, moving in the opposite direction: hence a very simple maintaining power, sufficient, whenever the rocker continues to vibrate, to compensate for the loss of force in each half of the vibration which would occur if the rocker and lead were of the same temperature. Mr. Faraday illustrated the sustaining force of the lateral motion of the points of support, by placing a rocker on a piece of lead, and the latter on a board. A pair of sugar-tongs was held tightly by the bend against the edge of the board, so that the line from the tongs towards the rocker was perpendicular to the axis of the latter. On making the limbs of the sugar-tongs vibrate in the manner of a tuning-fork, they communicated longitudinal vibrations of equal duration and number to the board, and through it to the lead and points supporting the rocker; which latter itself immediately acquired vibratory motion isochronous with the vibrations of the tongs, and by successive blows upon the lead produced sound: upon removing the rocker, and repeating the other parts of the experiment, no sound was produced.

Experiments with other metals were then made. A piece of curved silver plate being heated and placed on an iron triblet, rocked and sang in the manner of the others; this is an effect which work-
ing silversmiths have long known. The superiority of lead, as the cold metal, was referred to its great expansive force by heat, combined with its deficient conducting power, which is not a fifth of that of copper, silver, or gold; so that the heat accumulates much more at the point of contact in it, than it could do in the latter metals, and produces an expansion in that respect proportionally greater.

Mr. Trelivellyan's paper had been read to the Royal Society of Edinburgh, but is not yet published. Mr. Faraday stated that Mr. Trelivellyan had very liberally allowed him the use of a written copy.

May 6th.—Mr. Lindley on the Pitcher Plant.—On this evening Mr. Lindley brought before the meeting some illustrations of the plants that have those remarkable appendages which botanists call Pitchers or Ascidia.

He remarked that appendages in which water or fluid collects have been noticed in a variety of plants; but that he did not propose, upon the present occasion, to advert to any in which the pitchers do not form a striking and principal feature of the vegetation. These he stated to be the following.

Firstly, all the species of Sarracenia, little North American swamp-plants, in which the pitchers are hollow, green, sessile bodies, arising from the crown of the plant, and surrounding the scape; they are furnished with a projecting membranous wing on their face, are terminated with a green leaf-like lid, and are covered inside with numerous inverted hairs.

The second kind of pitcher plants was said to consist of the various species of Nepenthes, which are found growing in the marshes and ditches of China, and the damper parts of India. In these the pitchers were described as hollow bodies, similar to those of Sarracenia, and, like them, furnished with a sort of lid, but differing in having a long stalk, which in the lower half is leafy and flat, and in the upper, where it joins the pitcher, cylindrical and twisted; and also in proceeding from the stem in the place of leaves, instead of forming a cluster of pitchers, arising from the base of the scape at the surface of the ground.

A third kind, a native of swamps in New Holland, the Cephalotus follicularis, was compared to Sarracenia, in regard to the organization and position of its pitchers, but was stated to be remarkable for the presence of flat leaves of an elliptical figure among them.

All the foregoing were said to be either herbaceous plants, or at least not more than undershrubs; that is in the case of nepenthces, intermediate between herbaceous and shrubby. Trees or climbing plants were next mentioned, as sometimes having appendages to which the name of pitchers might be applied, although destitute of the remarkable lid which exists in all the kinds previously named.

Thus in the Dischidia Rafflesiana and Clavata, two plants found the one in the Indian Archipelago, and the other on the coast of
Martaban, the pitchers are in the form of large yellowish-green bags, hanging in bunches from the slender woody stems by which the species climb to the tops of trees; and in Marograavia umbellata, and in the genus Norantia, the former a West Indian climbing plant, the latter small trees found in the midst of rocks and mountains in Brazil, especially in the Minas Geraes and Serra Dorada, the pitchers are small, coloured, hollow bodies, occupying the place of bractee, and hanging down or standing erect among the flowers.

These forms of pitchers were illustrated by highly magnified drawings, and by beautiful specimens furnished for the occasion by Dr. Wallich.

With regard to the uses for which these curious organs are destined, it was observed that a variety of opinions had been entertained, among which it was difficult to say which was most unsatisfactory. Thus Rumphius supposed that the pitcher of Nepenthes was intended as the nest of a sort of shrimp frequently found in it; Morison considered it in Sarracenia as an "operculum divina providentia ad obtegendum et defendendum plantam a pluviarum injuriis statutum." Linnaeus compared Sarracenia to a water-lily growing in dry ground, and thought its pitcher was a reservoir of rain; and he supposed that in Nepenthes the pitchers were reservoirs of water, to which animals might repair in time of drought, their lid being especially destined to close up the mouth of the vessel, and thus to prevent evaporation. Sir James Smith thought that in Sarracenia the pitcher was intended for an insect-trap, because insects are often found in the water, and because the stiff inverted hairs that line it are peculiarly well adapted to prevent the escape of insects once inclosed in it; and that the putrescence of the insects was converted into the food of the plant.

The objections to these theories were obviously, that they either depend upon data which are actually false, as in the case of Linnaeus, when he fancies that a plant which really grows in marshes is a native of dry situations; and that a lid, which never alters its position when once raised from the pitcher, has a power of contracting and closing up the mouth to prevent evaporation; or else upon unsupported hypothesis, as in the case of Smith's opinion, that the putrescence of insects generates food for the plant; and of others who think that the pitchers are reservoirs of water for the use of animals in dry weather. With regard to this latter supposition, it was remarked that, in Sarracenia, the water could not easily be emptied out of the pitcher except by birds; that in Nepenthes it evaporates, without being renewed, shortly after the elevation of the lid; that in Discidia and Norantia it is not easy to conceive how the pitchers can be emptied at all; and, finally, that it is contrary to reason to suppose that Providence should make provision for an accumulation of water for the use of animals, exclusively in those places where, in consequence of the humidity of the atmosphere, or the nature of the marshy soil, such an accumulation would be of no use.
It was suggested that pitchers have doubtless different uses in different plants. In the Dischidias it is probable, as Dr. Wallich has remarked, that they are reservoirs of nutriment, from which the roots, emitted by the stem, and constantly found ramifying within them, absorb food for the general support of the individual, and that in this case they become necessary in consequence of their long slender twining stem being too narrow a channel of supply from the subterranean roots to the leaves. With regard to Nepenthes, the following idea of the uses of its pitcher was offered for consideration. It has been discovered by Mr. Valentine, that a vast quantity of spiral vessels is found in the stem and petioles, a quantity so considerable that no plant has yet been noticed in which they are equally abundant; now it has been ascertained by Bischoff, that spiral vessels convey air containing about twenty-eight per cent. of oxygen; and as it is well known that an accumulation of, or an excessive supply of, oxygen is destructive to vegetable life, is it not possible that the pitchers are a contrivance to enable the plant to get rid of its oxygen, and may not the water that they contain have been discharged by the spiral vessels themselves? It was submitted that a confirmation of this opinion was apparently afforded by an observation of the late Dr. Jack, that the bottom of the pitchers of the Penang species, in the inside, is beautifully punctured, as if by the mouths of vessels; and also by a remark of Dr. Graham, that the water in the pitchers formed in the Botanic Garden, Edinburgh, was at first subacid, and continued to increase in acidity till the whole evaporated.

It was stated that scarcely anything was, however, known of the exact nature of the water in pitchers; it having been only analyzed in one instance, when Dr. Turner found the contents of Nepenthes to contain minute crystals of superoxalate of potash. It is well known that in Sarracenia the water is putrid; and in Norantia it is described as sweet in one species, and bitter in another.

The last subject of inquiry was the nature of the pitchers, and their analogy to the other and more common organs of vegetation. With reference to this point, some remarks were made upon the doctrines of morphology, a subject first distinctly adverted to in this country, incidentally and in a very concise manner, by Mr. Brown, in 1816, but originally conceived by Jungius in 1678, and subsequently explained in an admirable treatise by the celebrated poet Goethe in 1790. The sum of this doctrine is, that a plant usually consists of only two essentially different parts, viz., the axis (or stem and root) and the appendages of the axis, all of which, under whatever form they may appear, whether of bractæ, calyx, corolla, stamens, or fruit, are mere modifications of leaves. Consequently, it should follow that pitchers are also leaves; an opinion that was supported in Sarracenia by a reference to their evident transition from the form of leaf in Dionæa Muscipula, the pitcher itself being a petiole in a particular state, and the lid the lamina; in Nepenthes and
Cephalotus by their obvious identity of nature with Sarracenia; in Dischidia by their position upon the stem, by the condition of their petiole, and by their relation to the inflorescence being the same as that of leaves; and in Norantia by their position with regard to the flowers, and their gradual transition from leaves to their most perfect state.

May 13th.—Mr. Brockedon on the Passage of the Alps by Hannibal.—Mr. Brockedon, in offering his remarks upon the passage of Hannibal across the Alps, illustrated his observations by drawings and maps.

His object was to show the errors and absurdities into which writers upon this subject had fallen by fire-side conjectures and reliance upon previous authors, who were as ignorant of the Alps as themselves, and whose chief authorities were incorrect maps. His own actual examination of above forty of the passes across the Alps, having traversed them nearly sixty times, has brought to test the impossible and impracticable routes laid down by different authors—impossible under the authority of Polybius—which, contemporary as he was with the event, and consistent as he was with himself throughout his narrative, Mr. Brockedon thought was the only authority to be relied upon. He successively exposed the fallacies of Livy, St. Simon, Folard, Fortia d'Urban, Whittaker, Laranza, and other authors who theorized upon any other pass than that of the Little St. Bernard, where alone, throughout the great chain, Mr. Brockedon observed the coincidences of times, distances, and events, as related by Polybius, could be found to have occurred.

The researches of General Robert Melville, given to the world by M. de Luc of Geneva, were the first to excite attention to the true line of the passage of the Carthaginians; the subsequent examinations of this and other routes in the Alps by Messrs. Wickham and Cramer, of Oxford, confirmed General Melville’s views; and Mr. Brockedon’s repeated journeys over the Little St. Bernard, and every other course across the Alps, which it was possible for Hannibal with his army to have taken, confirmed his belief in the moral certainty that it was by this pass only that the extraordinary entry of the Carthaginians into Italy was effected.

May 20th.—On this evening Mr. Charles John Robertson, of Worton-house, Isleworth, gave an account of his improved method of painting in water colours.

The object which the lecturer proposed to himself in the experiments which led to these improvements, was the uniting the advantages supposed to exist separately and exclusively in oil or in water colours. Oil painting has been supposed to possess greater durability, and the exclusive power of executing pictures upon a grand scale: the oil which is used as a vehicle bringing out the colours to their fullest tone, and producing a richness and mellowness of effect.
of which it has been supposed water colours are incapable, which were considered, till within these few years, to be fit for little else than the slight sketchy works of amateurs; and, indeed, notwithstanding the great and rapid improvements made within the last thirty years, and the beautiful specimens that have been seen in the annual exhibitions of the Society of Painters in water colours, the works painted in water have hardly yet established their just claim to be called paintings, being usually named drawings, to distinguish them from works in oil. Still, every lover of art has felt and appreciated the superior brilliancy and purity of the tints of water-colour pictures. This they owe to their being painted with transparent colours on a white ground, where the rays of light passing through the diaphanous colours to the paper, are reflected back to the spectator, producing a similar effect to the light reflected through a jewel from the foil at the back; while pictures in oil, being painted for the most part with opaque colours, give back the light from the surface, and deprive them of that fulness of tint which is possessed by water colours, in this case resembling the light reflected from the surface of a jewel which, in those of the deepest colour, appears nearly colourless. Now the question of durability is not so easily disposed of as a hasty adopted prejudice would assert. If any collection of oil pictures be carefully examined, those in the highest state of preservation will be found to have suffered considerably, while the majority are obscured from various causes; the oil itself becomes dark and opaque by time, and acts chemically upon many of the colours; which again being mixed upon the painter’s pallet without regard to their chemical nature, also act upon each other: thus the light colours become browner, and the deeper colours mealy; then follows the ruinous practice of cleaning and varnishing, the evils of which our space will not allow us to enumerate, but which are well known to those interested in the subject. On the other hand, pictures in water-colours, by the usual modes, require a glass to preserve them from the injuries of dirt and moisture, and if they become stained or dirty, cannot be cleaned without the greatest risk of destruction.

The risk of breaking, the weight, and the expense of plate glass, would necessarily limit the size, did not the inferior force of water-colours, by the old method, preclude the attempting a large picture. But our limits warn us that we must proceed to describe briefly the new method as proposed by Mr. Robertson, which he illustrated by pictures, one of them containing about forty square feet of surface, with whole-length figures the size of life, the production of which excited the full approbation of the audience. Mr. Robertson attaches the paper upon which he paints, by means of glue and paste, to a strong linen, the back of which he defends by tin foil, also firmly attached by the same means, which secures that part from the effects of damp, a great source of injury to pictures. Beginning with a neutral tint, he washes in all his colours sepa-
rately and unmixed, each separate colour being firmly fixed in the paper, by washing it plentifully with water till the water runs off clear, so that there is no danger of one colour tarnishing another by mixing with it; by this means the colours are so pure that he is not under the necessity of using those that are not permanent, for the sake of their brilliancy, as it is well known that a transparent colour used over another produces a much brighter third colour than when the same two colours are mingled (his colours are prepared in the usual way). When he has got his picture to the utmost degree of force that he is able by these processes, he varnishes it over in the early stages with a solution of gum tragacanth, and in the latter with a solution of isinglass in alcohol, painting over these and again varnishing, till he produces a depth of colour equal to the most powerful oil picture. By these means colours that, although permanent in themselves, may injure other colours (permanent separately) by their chemical agency, become protected from each other, and all are defended from the action of gases and vapours, as well as from injury by smoke, grease, or dirt; for the latter may, with the greatest facility, be removed by spirit of wine, which readily dissolves them, while it cannot injure, even in the slightest degree, the surface of the picture: for since isinglass can only be dissolved in spirit of wine, by being kept at the boiling point for several hours, it is evident that when used in a cold state it cannot affect the varnish. To defend the picture from injury by humidity, it may be varnished by any of the varnishes used for oil pictures, which may all be taken off again readily by alcohol, down to the exact surface of the picture. Nay, if the whole surface were painted over with oil paint and suffered to dry, it might be as easily cleaned. Amongst those present were the president and several members of the Royal Academy, who expressed the greatest interest in Mr. Robertson's principles and details.

May 27th.—Mr. Britton's remarks on, and illustrations of, the Old Domestic Architecture of England.—This subject was illustrated by a variety of curious prints, drawings, and models, tracing the progress of the art, from the Norman style still extant in some of our ancient buildings, down to the period when Roman architecture began to be mingled with our national works, and till it ultimately supplanted them in the buildings destined for domestic use, as well as those intended for divine worship. The new buildings at Windsor Castle, and those in and about London, were very particularly dwelt upon, as indications of the present state of domestic architecture.

Numerous presents were placed on the library table, given by G. Bennet, Esq. They were principally from the South-Sea Islands.
June 3d.—Mr. Ritchie on the relation between Electricity and Magnetism, and on Electricity as the probable origin of all the phenomena of Natural and Artificial Magnetism.—After a few general observations on the nature and laws of action of voltaic electricity, Mr. Ritchie proceeded to illustrate the striking relations between a conductor of voltaic electricity and artificial magnets. As the law according to which the needle places itself across a conductor is easily forgotten, the lecturer recommended the following artificial mode of viewing it, as the best for fixing it securely in the memory. Regarding the sun as the visible cause of terrestrial magnetism, and conceiving a current moving round the earth in the direction of the sun’s apparent motion, from east to west, let a person conceive himself looking towards the east, and then lying down on his back, with his feet to the east, he may consider himself as a portion of a conductor, the current of positive electricity entering at his feet and passing out at his head. If he now conceive a magnetic needle suspended above his chest, the north pole of this needle will arrange itself towards his left hand, which will always be its direction, whether he thinks of terrestrial or artificial electricity.

Mr. Ritchie then described the mutual action of two voltaic conductors, discovered by M. Ampère. If a voltaic conductor act on a magnetic needle, it may follow that two conductors will act on each other, and the manner in which they will act may be imagined by reasoning à priori. If an indefinite number of very short magnets be placed transversely on a flat piece of wood, with their poles of the same name, all placed in the same direction, we shall have, says Mr. Ritchie, something very like the section of a conducting wire.

Let AB be a slip of wood, having a great number of small magnets made of portions of sewing needles, cemented on the slip of wood, with all their south poles above and their north poles below, and let a small magnetic needle be suspended above this compound bar, and the needle will obviously arrange itself as in the annexed figure. If two such bars be placed parallel to each other, as in the annexed cut, with their poles in the same direction, and if one of them be moveable, it will move parallel to itself, till it come in contact with
the other. This is obvious, since dissimilar poles are opposite to each other, attraction must take place. If they be placed, as in the second case, the moveable one will be repelled, and continue to move parallel to itself, till it get without the sensible repulsive action of the other.

Viewing these compound bars as metallic slips conducting voltaic electricity, it is obvious that attraction will take place when the currents move in the same direction, and repulsion when they move in opposite directions.

The same illustration will apply to a terminated current moveable about one of its extremities, when acted on by a straight indefinite current. Let AB be an indefinite straight conductor, and DC a terminated conductor moveable about the point C. It is obvious that in this position repulsion will take place, and the moveable conductor will turn round C, till it arrive at a position at right angles to the straight conductor. At this point the attraction of the other half of the straight conductor will act till it be brought to a parallel position. By viewing the tendency to move in every position, we clearly see that the terminated conductor has a tendency to revolve about the point C, and by a sufficiently powerful battery this revolution may be made to take place.

By substituting a magnet for the conductor the same thing takes place, as in the beautiful experiments of Mr. Faraday and M. Ampère.

A portion of a circular conductor may be viewed as a straight one, and all the phenomena of terrestrial magnetism may thus be satisfactorily explained, by supposing currents of electricity circulating about the earth from east to west. Such currents may be made to act on terminated currents, and produce all the phenomena of attraction, repulsion, and rotatory motion, as has been done by M. Ampère.

By means of an artificial globe, surrounded with coils of copper wire, Mr. Barlow has exhibited all the effects of terrestrial magnetism, on the direction and dip of the needle. Without entering into the scientific details of the theory of terrestrial magnetism, Mr. Ritchie showed, by a very simple experiment, that the old theory, which viewed the earth as a huge magnet having two poles and a middle line, was inconsistent with known facts. If a vessel of water be placed on the middle of a large magnet, and a light magnetic needle floated on the surface of the water, this needle will arrange itself in the direction of the poles of the magnet. So far it agrees

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with what actually takes place on the surface of the earth. But the needle cannot be made to remain in that position. The attraction of one of the poles always overcomes the attraction of the other, and the needle floats towards the nearest pole—a fact quite contrary to what takes place when the earth acts on the same floating needle.

But it may be asked, how are these currents generated? Are they voltaic or thermo-electric? From the constitution of our globe, we can scarcely doubt that they belong to the latter class. The earth abounds with metalliferous veins, and these veins are undoubtedly of different temperatures, and, consequently, thermo-electric effects must take place. The rapid change in the direction of the magnetic equator, when approaching South America, renders this supposition highly probable.

The effect of voltaic electricity in forming temporary magnets was exhibited; but as that was fully described in the last Number of our Journal, we refer to it for further information. Mr. Ritchie remarked, in concluding, that we were fast approaching to the period when all the phenomena of heat, light, electricity, &c., would probably be referred to the same great cause, merely acting in different ways; and concluded by quoting the following prediction of the late Professor Playfair, which we shall give in his own words:—

'If, on the other hand, we consider how many different laws seem to regulate the other phenomena of the material world, as in the action of impulse, cohesion, elasticity, chemical affinity, light, magnetism, galvanism, electricity, the existence of a principle more general than any of these, and connecting all of them with that of gravitation, appears highly probable.

'The discovery of this great principle may be an honour reserved for a future age, and science may again have to record names which are to stand on the same levels with those of Newton and La Place. About such ultimate attainments it were unwise to be sanguine and unphilosophical to despair.'

June 10th.—Mr. Faraday on the Arrangements assumed by particles on the surfaces of vibrating Elastic Bodies.—This was the subject of a paper read before the Royal Society a few weeks ago, of which Mr. Faraday was the author. He stated, that his principal reasons for bringing it forward on the present occasion arose from a desire to illustrate the characteristic differences between the Royal Society and the Royal Institution, in their modes of putting forth scientific truths; and his conviction that every thing, whether small or great, originating in the latter establishment, should be placed, as soon as possible, in the possession of the members at large.

When a plate or pane of glass is held horizontally by a pair of tongs gripping the glass at the centre, and a violin-bow drawn over the edge of the glass, it is made to vibrate; and sand having been previously sprinkled upon the surface of the plate, the particles arrange themselves into regular forms, figuring forth the quiescent parts of
the glass. These are called by Mr. Chladni, their discoverer, nodal lines. When light particles, such as scrapings from the hairs of the bow used, dust, or the powder of lycopodium, happen to be on the plate; instead of proceeding to the same quiescent lines as the sand, they accumulate at the parts in most violent agitation, forming a cloud, and at last settling down into little hemispherical heaps, having a peculiar revolving or involving motion. This determination of light powders has always embarrassed philosophers: and M. Savart has founded a theory of some peculiar modes of vibration upon it. Mr. Faraday's object was to show, that the effect is a very simple and natural one,—consisting of nothing more than currents formed in the air surrounding the plate, which, proceeding from the quiescent to the most agitated parts of the plate, then pass upwards, and in their course carry the light particles with them. Mr. Faraday explained, and demonstrated by numerous experiments, how such a current would necessarily result from the manner in which the mechanical forces of the plate are transmitted to the air. He showed that this current could be interrupted by walls of card, when the light particles took new courses. He stated that the heavy particles went to the lines of rest because the air had not force enough to carry them in its course; but that light particles, being governable by it, were taken in the opposite direction. He confirmed this view by substituting water for air, making the plate vibrate in the former fluid, and showing that the sand was then carried from the quiescent to the agitated parts, exactly as the lighter particles were in air; and further, on vibrating plates in vacuo, he found that even the lightest particles went to the lines of rest, because there was no current of air of sufficient force to sweep them in the opposite direction. Want of time prevented Mr. Faraday from entering upon the explanation of the involving heaps: but this point is fully treated of in his paper read before the Royal Society. He announced that further consideration of the subject induced him to believe he should be able to account, by the same principles, combined with the cohesive power of fluids, for the peculiar and hitherto unexplained crispations which occur on water and other fluids lying upon a vibrating plate.

This being the last evening-meeting of the season, Mr. Faraday, on the part of the committee, took leave of the members, after earnestly exhorting them to use both individual and conjoined exertions to aid the prosperity of future seasons. In the library was placed a beautiful portrait of Sir Humphry Davy, of the full size, copied by W. Pickersgill, jun., from the portrait by Sir Thomas Lawrence.
Proceedings of the Royal Institution.

On the 2d of May the usual election took place, when the following officers were elected:—

President. His Grace the Duke of Somerset.
Treasurer. Sir George Ducket, Bart.
Secretary. Edmund R. Daniell, Esq.

Managers.
Benjamin Bond Cabbell, Esq.
Captain Chapman, R.A.
Davies Gilbert, Esq.
Sir Henry Halford, Bart.
Henry Hallam, Esq.
Edmund Halswell, Esq.
William Hamilton, Esq.
Marquis of Lansdowne.
Geo. Moore, Esq.
Dr. Whitlock Nicholl.
W. H. Pepys, Esq.
Charles Pilgrim, Esq.
William Pole, Esq.
Hon. W. F. Spencer Ponsonby.
Lord John Russell.

Visitors.
Thomas T. Bernard, Esq.
Sir Gilbert Blane, Bart.
Robert G. Clarke, Esq.
Rt. Hon. Reginald P. Carew.
John F. Daniell, Esq.
John Fuller, Esq.
Dr. A. B. Granville.
Henry G. Knight, Esq.
Lieut.-Col. W. M. Leake.
Viscount Palmerston.
Lord Seymour.
R. H. Solly, Esq.
William Sotheby, Esq.
Sir J. T. Stanley, Bart.
N. A. Vigors, Esq.

From the Annual Report presented by the Visitors at the same time, it would appear that the property of the Institution is considered at present, when estimated in the lowest manner, as follows:—

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Funded Property.
In 3 per cent. Consols, Mr. Fuller's donation 1250 0 0
The Sinking Fund 770 15 6
Investment on Laboratory Account 977 4 11

**£2998 0 5**

The bond debts and simple contract debts amounted all together to £2352. 6s. 8d.
There have been fifty-seven new members elected during the year.

BOTANY.

Sterility of Hybrid Plants.—On the 9th of May, M. Dutrochet addressed a letter to the Academy, in which he attributes the sterility of hybrid plants to the imperfection of their sexual organs. In the flowers of some species of cherry-trees (those derived from the union of the prunus cerasus and the prunus avium), the stamina have no pollen; their antherae form a compact mass which does not divide into pollenic or fertile dust, as is the case with fruitful cherry-trees.

Irregularity of the Organs of Vegetables.—On the 6th of June, M. Dutrochet communicated some observations on this subject, which he considers as presenting a phenomenon similar to that which he has observed in some animals, viz. an invariable abortion of some of the parts, so that these plants are in fact consistent or perpetually recurring monstrosities. In an Alpine cytisus, which was terminal, M. Dutrochet observed six petals, four disposed in a cruciform manner, and above them two contiguous petals placed alternately. The manner in which these last were placed prove that there must have been two others which have become abortions, so that the papilionaceous flowers were originally regular flowers, having eight petals disposed in two ranges alternately. Three of these petals constantly become abortions, and the five remaining ones form the standard, the two wings and the two pieces of the keel. Irregular flowers are always lateral; when by chance they become terminal, they resume their original regularity, because they have then equal room for development on every side.

Abortions and Irregularities of Flowers.—On the 13th of May, M. Adrien de Jussieu read a memoir on this subject, in which he entered into a very detailed examination of the structure of flowers, particularly of the family Malpighia. The flowers of this family have been generally described as regular, or, if authors have pointed out some irregularities, they have cited them as exceptions, whereas they are constant and numerous. In fact, the segments of their calyx are similar in very few cases, their petals scarcely ever; their stamina differ both in number and in form and dimension, and the number of the ovaries is never in proper proportion to the other parts of the flower. M. Jussieu’s object is to account for and systematize these irregularities.

A complete flower presents several orders of organs disposed in an equivalent number of concentric circles; in the dicotyledones (to which this memoir principally relates) the number of parts springing from the same circle is generally five, or a multiple of five; these
parts sometimes rise to the same height, in which case they form a verticillus, and sometimes to different heights. Although the latter case is much more frequent than the former, the authors who have written on the subject generally speak of these flowers as composed as verticilli, probably because the difference of height between the various parts of each system is so small as to appear to them unworthy of notice. But M. Jussieu, by taking a minute account of these differences, seeks to establish that the most general law relative to the disposition of the leaves of a branch may be equally applied to the parts of the flower. Let us suppose the parts inserted in different points of a spiral line turning on the conic spindle (noyau) of the flower. Let us divide the surface of the cone into five equal parts, by so many lines let fall from the summit to the base, by which each spiral turn will be cut by these lines at five points. Let us then suppose an insertion at any of the points of intersection, and place them upon these points alternately (de deux): after two spiral turns we shall find the sixth insertion situated directly over the first, and the first five will form what Bonnet calls a quincunx. When the parts are large enough for the borders or edges to pass each other, they will cover or lap over each other in such a manner as to form two exterior, two interior, and one intermediary, that is to say, covering on one side and covered on the other. The two exteriors will be placed first and second, the intermediary third, and the two interiors fourth and fifth. It is necessary to remember these characteristics, because they serve to point out the order of insertion where the difference of height is too small to serve as a guide. When the parts are large enough to lap over each other, not only with their borders or edges, but by the greater part of their surface, one will envelope, or lap over, two, two over three, three over four, and four over five. This is the inflorescence which has been called enveloping, and which differs from the quincuncial only by the enlargement of the parts. The parts of a quincunx in the flower are generally placed alternately with those of the two quincunces which are immediately above and below it. It is difficult to account for this disposition on the hypothesis of a single spiral, but it is perfectly intelligible if we admit the existence of a second spiral entirely similar to the first, and revolving on the same cone, but commencing from the opposite point of the base like the worms of a double screw. It is upon these two spirals that the concentric spirals are inserted alternately. This supposition is justified by observing the flowers on which the parts which are multiples of five, or some other number, alternate among themselves in various rows; it is evident that this alternation results from the parts being inserted at equal intervals upon several spiral parallels. The petals of the cactus, the stamina and fruits of several of the magnolias and ranunculi, furnish examples of this. The disposition of the scales in the common pine cone is also an illustration, if not a positive proof, of this.
From what has been said, the five folioli of the calyx of a flower may be considered as placed on the first spiral. At the point corresponding to that of the sixth insertion, but on the second spiral, the quincunx of the petals will commence, which will also be disposed upon two spiral turns, nearer to the summit than those which bear the folioli. When the system of the petals is finished, that of the stamina will begin, and form a third quincunx, situated on the same spiral as that of folioli, but on the fifth and sixth turns. Lastly comes the quincunx of the ovaries, which will be inserted on the seventh and eighth turns of the second spiral. It has here been supposed that each spiral begins to form the first part of one quincunx at the moment at which the other spiral completes another; but this regularity does not always occur. Thus the second, instead of bearing the first petal at the point corresponding with that which a sixth folioulus would occupy, may bear it at the point corresponding with the seventh, eighth, ninth, or tenth; whence there are four other combinations possible; and although the alteration of the parts in the two successive quincunces, and the corresponding opposition of those of every other quincunx, is apparently unaltered, the regularity is less perfect, because, although a stamen will always be found corresponding to a folioulus of the calyx, and an ovary to a petal, it will not correspond with the folioulus and petal bearing the same number. The nearer the spiral rises towards the summit of the cone, the more its turns contract and approach each other. The lower, therefore, a quincunx is situated in a flower, the more its parts are separated from each other. This remark may give rise to several deductions. In the first place the principles just laid down will be the less easily recognised in proportion as their exemplification is sought for in a higher or more interior quincunx, because the contraction of the spiral turns tends to give it the appearance of a verticillus, and the slightest deviation in the insertion of a part will tend to change its apparent order: it is therefore in the calyx that these principles may most frequently be verified. The inequality of height in the insertion of the petals can rarely be observed, except by their situation being a little more exterior and interior, and even this can rarely be perceived in the flower when open; in the bud it is more perceptible. The laws of the quincunx, being once admitted for the calyx and corolla, should also by analogy be admitted for the stamina and the ovaries, although the latter more generally present the appearance of a perfect verticillus. Several instances, however, may be found in which this appearance does not exist; added to which the developement of the parts of each of these pretended verticilli is much less contemporaneous than is generally supposed. This developement, indeed, would naturally be slower in the highest parts, which must be impeded by want of space; whence it arises that total or partial abortions are more frequent in proportion as the quincunx is in a more elevated situation in the flower. Thus abortions are rare in the calyx, less so in the corolla, still less so in the
stamina, and frequent in the ovaries, the number of which is often less than that of the other parts, and the unequal development of which may be constantly observed both during and after its progress towards maturity. Thus the passage from regularity to irregularity in flowers becomes insensibly established. These irregularities could hardly be explained on the supposition of the parts being exact \textit{verticilli}, and therefore placed in conditions equally favourable to development. In fact, no examples of irregularity are found in flowers of the valvulary or twisted efflorescence, which invariably indicates the disposition of the parts in \textit{verticilli}. In a quincunx, on the contrary, it is clear that the upper parts are placed in a situation more unfavourable to their development. This is especially true with respect to the ovaries, because the action resulting from the relative situation of the parts has fuller force from there being no quincunx above it. This tendency to irregularity in the flowers in which the parts are disposed in quincunxes, is not much observed when the two halves of the conic spindle are placed in equally favourable conditions, which is the case when the axis of this cone has a direction according to the elongation of the peduncle. But when the flower rests on the peduncle by an oblique base, one of its halves is situated higher than the other with relation to the axis of the plant, and is therefore in a situation more likely to produce abortions. This obliquity of the base exists in the majority of irregular flowers, particularly in those in which the inequality of the two halves is very decided, and which are called \textit{didynamiae}. These causes of irregularity will be much greater, if, instead of supposing the spiral constructed on the circle which forms the base of the conic spindle, we suppose it constructed on a section which is oblique to the axis, that is to say, on an ellipsis. In this case, every spiral turn ascending and descending alternately as respects the axis, the series of the insertions will no longer have an exact reference to their heights, and thence will result an order apparently differing from the quincunx. The calyx of the \textit{antirrhinum majus}, the insertion of which is extremely oblique, affords an example of this. M. Jussieu, having thus laid down his position, proceeds to exemplify it by numerous examples, illustrated by plates; but it is unnecessary to cite them here, as the above explanation of his theory will be sufficient to enable our botanical readers to verify or refute his ingenious hypothesis. We shall take care to make known the report which will be made to the Academy on the subject.

\textbf{Chemistry.}

\textit{Perchloric Acid—Perchlorates—a test for Potassa and Soda.}

On the 25th of April, M. Serullas communicated the particulars of some experiments which he had recently made on this acid, considered as a re-agent, by means of which to distinguish and separate soda from potash, either alone or combined with other acids. He was led to make these experiments from observing the great differ-
ence in the degree in which the perchlorate of potash and the perchlorate of soda are soluble. The first, at a temperature of 15° C. (59° F.), requires more than sixty times its weight of water to dissolve it, while the latter is remarkably deliquescent, and consequently easily soluble, not only in water, but even in alcohol. In order, therefore, to distinguish and separate these two alkalis, it occurred to M. Serullas that it would be expedient to ascertain the possibility of producing, in the same liquid, a salt from potash, which would be almost insoluble, and one from soda which would be very soluble. Neither hydrochlorate of platina, tartaric acid, nor hydrofluosilicic acid, all of which may be employed to precipitate potash and soda, afforded sufficiently accurate means of obtaining the desired result; but M. Serullas ascertained that if perchloric acid be poured, drop by drop, into a solution of soda and potash mixed, a perchlorate of potash is instantly precipitated; the perchlorate of soda (or the soda itself, if there be not an excess of acid) remains in the liquid, whence it may be separated by concentrated alcohol, which will precipitate at the same time the small quantity of perchlorate of potash which may remain. A solution of perchlorate of soda, to which potash is carefully added, will instantly precipitate a perchlorate of potash, the soda becomes free, and may be separated by alcohol. Owing to this great difference between the solubility of the perchlorate of potash and that of every other salt having the same base, it is easy to ascertain the existence of potash, either free or combined with other acids, in any saline solution, as the smallest quantity of perchloric acid occasions a precipitation of perchlorate of potash, while the other acids are rendered free, and may be isolated by alcohol. This experiment has been tried with the sulphate, nitrate, chlorate, bromate, muriate, and hydrobromate of potash. By this mode of proceeding the simultaneous existence of soda and potash may always be ascertained, and it will also be easy to examine the nature of the acid primitively combined with the former, as it may always be isolated by concentrated alcohol. There will also be a great advantage in employing the perchlorate of barytes, and that of silver, (both of which are very soluble,) in cases of combinations of soda and potash with sulphuric acid or hydrochloric acid, as, in both cases, by means of alcohol, all the perchlorate of soda may be obtained on the one hand, and on the other, all the perchlorate of potash, with the sulphate of barytes, or the chlorure of silver, from which the perchlorate of potash may easily be removed by washing it in warm water. The general results of the above experiments are—

1. That perchloric acid forms, with potash, a salt soluble with great difficulty, and requiring for its solution sixty times its own weight of water, at a temperature of 59° F.

2. That soda forms, with the same acid, a salt very deliquescent, and consequently easily soluble, either in water or even in the most highly concentrated alcohol. This fact was previously unknown.
3. That these opposite qualities of the two compositions afford the means of separating soda and potash when in solution together; the former generating a perchlorate easily soluble in alcohol, and the latter a perchlorate absolutely insoluble in that liquid.

4. That any acid whatever, which was primitively combined with potash, will always be separated and set at liberty by perchloric acid.

*Production of Perchlorate from Chlorate of Potassa.*—On the 23d of May, M. Serullas communicated the result of his observations on the change of chlorate of potash into oxychlorate (perchlorate) of the same base, by the action of heat, and on a new method of obtaining oxychloric or perchloric acid. The similarity of the phenomena manifested in the production of oxychlorate of potash, with those observed in the production of oxychloric acid, led M. Serullas to suppose that the simple action of a high temperature, maintained within certain limits, would convert chlorate into oxychlorate of potash, by combining part of the oxygen with the chlorate remaining undecomposed. He had long since observed that chlorate of potash, decomposed by fire, left a saline residue not easily soluble, and removable with difficulty from the tubes in which the experiment was made, but he concluded that former chemists, in establishing that this residue consisted only of chlorure, had taken notice of all the effects produced by heat at different periods of the operation; and, therefore, had not directed his attention to that point until accidentally led to do so in the course of his experiments on oxychloric acid. He observed that when chlorate of potash is heated in a crucible, it first melts, and then boils, at which state there is an escape of oxygen. When the heat is regulated sparingly, after a certain duration of the ebullition, this escape of oxygen ceases, unless the temperature be raised; if the operation be then suspended, and the solution be filtered while hot, a considerable quantity of oxychlorate will be obtained, when it cools, in small brilliant crystals: 40 parts of chlorate furnished 17·5 parts of oxychloric acid. M. Serullas has established that Stadion, and all the other chemists, were wrong in supposing that oxychlorate of potash is decomposed at 392° Fahr.: it requires at least 752° Fahr.

In order to obtain oxychloric acid, oxychlorate of potash must be boiled with silicated hydrofluoric acid, in great part evaporated, so as to obtain, when cool, a more abundant deposit of the gelatinous fluosilicate of potash; this must be filtered, again evaporated, left to cool, again filtered, concentrated in a capsule, and then distilled in a small retort. In order to precipitate the small quantity of fluosilicate of potash and of oxychlorate, which may exist in the oxychloric acid, it is only necessary to pour into it a little concentrated alcohol, filter it, dilute it with water, and then let it evaporate. The oxychlorate of potash contains 65·725 parts in 100 of oxychloric acid. This simple means of procuring the acid is highly important, as from M. Serullas' discovery
of its utility in separating soda from potash, as above-mentioned, it will necessarily become in great request.

Action of Vegetable Substances, Gum, Sugar, &c. in contact with Metallic Oxides.—On the 2d May, M. Becquerel communicated to the Academy a very interesting paper on carbonate of lime in crystals, and on the simultaneous action of saccharine and mucilaginous matters upon the oxides of certain metals, obtained through the medium of alkalies and earths. M. Becquerel has, for a considerable period, directed his attention to the means of submitting organic substances to the action of electric currents, with the view of ascertaining the causes of some of the phenomena observable in those substances, particularly that of fermentation. It was already known, from the experiments of Cruikshank and Daniell, that on exposing a solution of sugar and lime in water to the action of the atmosphere, small crystals of carbonated lime are produced on the surface; but the cause of this phenomenon was entirely unknown, although it was supposed that the carbonic acid might perhaps be supplied by the atmosphere. M. Becquerel, however, has, by means of the following experiment, ascertained the real source of the acid. He plunged into a wide-mouthed bottle, filled with barytes water, two tubes, (the lower parts of which were stopped with moistened barytes,) filled, the one with a solution of lime and sugar, and the other with a solution of sulphate of copper. The liquid contained in the first tube was connected with the positive pole of a voltaic pile, by means of a plate of platina, and that in the second tube with the negative pole, by means of a plate of copper. The moment this communication was established, the sulphate of copper was observed to be decomposed, the copper was precipitated in a metallic state on the copper plate, the sulphuric acid was absorbed by the barytes, and the oxygen was transported to the positive pole; where, by a re-action on the carbon of the sugar, it produced carbonic acid, which was immediately combined with the lime. After the lapse of some days, small prismatic crystals of carbonate of lime were observed on the plate of platina, and continued to increase as long as there remained any lime in the solution. Gum, the component parts of which are nearly similar to those of sugar, produced the same effect. In both cases, those portions of the vegetable substance which do not tend to the production of the carbonic acid, or of the water of crystallization of the carbonate, are converted into acetic acid. M. Becquerel was next led to examine the simultaneous action of saccharine and mucilaginous substances upon the metallic oxides, through the medium of the alkalies and the earths. If hydrate of copper be acted on by water and lime, with the aid of heat, it becomes black, and probably passes into an anhydrous state; but if a very small quantity of sugar be added, a portion of the oxide is dissolved, and the liquid assumes a beautiful blue tint, similar to that of a solution of oxide of copper in ammoniac.
Honey and sugar of milk have the same properties, which, however, have never been observed, except in saccharine substances. Potash and soda may be substituted for lime in this experiment with a similar effect, except that their faculty of dissolving is greater, whereas that of barytes and strontia is much less. Gum does not produce the same effect as sugar: that substance, when dissolved by water, is not precipitated by the alkalies and earths which we have just mentioned, but if a deutoxide of copper, in a state of hydrate, be added, a flaky insoluble precipitate of gum and oxide of copper is formed. When there exists in the solution a small quantity of saccharine matter in addition, it re-acts immediately on the excess of oxide, and of copper, which has been added, dissolves it, and gives a blue colour to the solution. In order, therefore, to detect the existence of gum and saccharine matter in any substance which contains both, it is sufficient to add potash and caustic lime to the solution, and then apply hydiate of copper to it. The mucilage found in a decoction of linseed produces the same effects as gum; and as the solution becomes slightly tinged with blue, it is evident that it contains saccharine matter. If the solution be acted on by heat, the effects are different. If a solution of sugar, potash, and deutoxide of copper, in water, be heated to the boiling temperature, the blue colour changes successively to green, yellow, orange, and finally to red, and then all the deutoxide is changed into protoxide. If oxide of copper be then added gradually, until there is no longer any protoxide formed, all the sugar is decomposed, and nothing remains in the solution but carbonate of potash and a small quantity of acetate of the same base. The saccharine matter of milk, which, when cold, acts on copper and potash in the same manner as common sugar, acts differently when heated. The deutoxide of copper passes first to a state of protoxide, and is then reduced to a metallic state. The oxides of gold, silver, and platina, submitted to the same tests as the oxide of copper, are reduced to a metallic state, while the oxides of iron, zinc, and cobalt do not undergo any change. The deutoxide of mercury is reduced to a metallic state by potash and the saccharine matter of milk; it then, in consequence of the water which is interposed between the parts, presents itself under the form of paste. Under this form, the mercury may be applied to glass without the necessity of using tinfoil; it is sufficient to spread the paste in a very thin layer, and heat the glass slightly, to remove the water which is interposed. Lime, barytes, and strontia, when acting by means of heat on the deutoxide of copper and saccharine matter, do not form compositions similar to those of the alkalies. Lime, for instance, does not convert the deutoxide into a protoxide, or a metallic state; it occasions a precipitate of an orange-yellow colour, formed of the protoxide of copper and lime. In the same manner, proto-cuprates of barytes and strontia are precipitated. These are the principal results of M. Becquerel's experiments, which
have considerable importance, as showing the intimate connection between the electric and chemical systems.

_Oxides of Barium._—On the 13th June, M. Despretz stated that the hydrate of barytes, which has been generally said to resist heat, is decomposed at a sustained red heat. This fact, which completes the series of experiments tried with lime, magnesia, and strontium, proves that no oxide of barium will retain water at a high temperature.

_Azote and Iron._—At the same meeting, M. Despretz also stated, that iron, at a red heat, subjected to the action of azote gas, is sensibly increased in weight; it disengages azote when dissolved in the acids.

_Sulphates._—M. Despretz also remarked, that all the sulphates which are not liable to be decomposed by heat alone, disengage sulphur, when acted on by carbon or hydrogen gas at a strong red heat.

**Geodesy.**

_Heights of the Pyrenees._—On the 4th April, M. Puissant presented to the Academy a report on a memoir by M. Corabœuf, entitled ‘Sur les Opérations Géodésiques des Pyrénées et sur la Comparaison du Niveau des deux Mers.’ The object of this paper was to fix with certainty the heights of several of the summits of the Pyrenees, and to determine the long-contested question as to whether the waters of the Mediterranean are precisely on the same level as those of the ocean—both being, of course, considered as in a state of perfect tranquillity. The report stated that the mode of operation, adopted by M. Corabœuf and his associates, was such as almost to preclude the possibility of any important error existing in his conclusions. His trigonometrical observations were always made in the most favourable state of the atmosphere, and the calculations resulting from them worked with scrupulous fidelity. The results obtained by M. Corabœuf generally coincide with those mentioned in the ‘Base du Système métrique décimal’—the greatest difference being 0" 24 on a distance of 9514" 78, and that but in one instance, the distance from Espira to the southern point of the base of Perpignan. In calculating the heights, M. Corabœuf made use of two tables, which he has annexed to his memoir—the one being the height above the Mediterranean, as observed by him on the eastern side, and the other the height above the ocean on the western side. The medium between these two calculations has been adopted as the definitive height. The greatest difference found between the two heights is that of Gardan de Montagu, which varies 2" 64; but this is to be accounted for by the great difference of the level between
Proceedings of the

this point, which is situated very low, and the Crabère, the absolute height of which is 2634 m; because, in such a case, the variability of the refraction sensibly affects the zenithal distances employed in the calculation of the differences of the levels. This has induced M. Corabœuf to adopt the summit of the Crabère as the point of comparison between the levels of the sea and ocean. His admeasurements were made by the southern direction of the intersection of the triangle, by the northern ditto, and by the diagonals; the mean result of these admeasurements gives, for the height of the Crabère above the Mediterranean 2633 m 50 above the Ocean 2632 77

Difference 0 m 73

Another series of calculations proves that the greatest probable error in the admeasurements is, for the southern, 1 m 860; for the northern, 1 m 421; and, for the diagonals, 1 m 416;—so that, even if there be any difference in the level of the Mediterranean and the ocean, it must be less than one of the above errors, since, according to the theory, the probability of that extreme error existing is only 150:000. The principal of the absolute heights determined in metres are the following:

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In a subsequent part of the memoir, M. Corabœuf has rectified an error of Delambre, who had supposed that there existed a difference of 3 toises in the height of the summit of Salces, as taken at Montjoïn and at the marsh of Leucate. The fact is, that the height of the point above the Mediterranean is 362.26 toises and above the Ocean 361.51 toises.

Difference 45

Thus affording an additional proof that the two surfaces may be considered as forming one level. This work will form part of the new geometrical description of France; and from its lucid details, minute calculations, and important geodesical researches, was considered by the Academy as deserving of insertion in the 'Recueil des Mémoires des Savans Étrangers.'

Geology.

Terrains Tertiaires.—On the 26th March, M. Reboul, a corresponding member of the Institute, read a memoir, the object of
which was to prove that the marine deposits of the Mediterranean basins of the departments of the Herault and the Aude correspond, as to their position, with the coarse calcareous (calcaire gros) formation of Paris, and not with the upper marine sands and freestone. In this opinion, which is contrary to that of most geologists, M. Reboul stated that he had been confirmed by a comparison of the fossils in the two basins. The lower marine deposit of the south of France is, like that of the Seine, covered with the gypseous and siliceous calcaire of fresh water (eau douce), and in both basins an upper, or rather mixed, marine deposit succeeds the fresh water deposit. The essential difference between the lower deposits of the south and those of the Seine is, that the former are free from all mixture of fresh water fossils, and at the same time from concretions of silex, of saline deposits, and even from compact, fine-grained rocks. All these various productions are, on the contrary, found in the upper or mixed earth of this basin, which forms a stratum over the fresh-water deposits, or rather which is subordinate to them. In the basin of Paris the same productions are found in abundance in the upper stratum, but they are also found, though in a smaller proportion, in the lower stratum. As the fresh water deposits have, in this basin, preceded, or at least been associated, with the first sediments of the calcaire gros, M. Reboul distinguishes basins of this kind by the name of prolymnéens and by that of metalymnéens—those where the fresh water does not appear to have penetrated until after the completion of the deposits of the first epoch; such are those of the Aude, the Oise, and the Herault. The comparison of the sediments of these two species of basins opens a new field of inquiry for geologists.

On the Chalk Formations of the South of France.—On the 25th of April, M. Brogniart read to the Academy a most interesting report on a memoir by M. Dufresnoy, entitled ‘Des Caractères particuliers que présente le terrain de craie dans le Sud de France et sur les pentes des Pyrénées.’ The generality of the world, and even many eminent geologists, have considered that they were fully acquainted with the nature of chalk, from observing in the quarries its immense masses without any distinct stratification, and its beds interrupted by flint stones; but, having confined their attention to the external and mineralogical characters of chalk, and neglected its geological peculiarities, they have yet to learn that strata, which have not the external character of white chalk with black silex, may and do belong to the same period and the same geological formation, because they possess the same peculiarities which, in the deposits of white chalk, form the true geological characteristics. There are three modes of determining the rank which any deposit occupies in the series of which the shell of the globe is composed.—1st. The nature of the strata which are constantly found above or below it—this is the geological characteristic; 2dly, the nature
of the rock or stratum itself and the minerals which accompany it—this is the mineralogical characteristic; and 3d, the organic remains contained in it—this is the zoological or organic characteristic. Of these, the second is the least certain. By chalk deposits, in a geological sense, therefore, we mean not merely those composed of white chalk, but such as occupy the same position in the beds of the globe as chalk usually occupies; which contain the same species of organic remains; but which may or may not present the same mineralogical characters. Hence chalk formations (terrains crétacés) may be black and compact, yellow and compact, in masses or in strata, with or without silex, and even wholly composed of sand and freestone, without containing any mineralogical chalk, and even scarcely any carbonate of lime. Hence it is that it has been hitherto supposed that there were no chalk formations in the south of France and the Pyrenees.

M. Dufresnoy, by his observations, has established three series of facts:—1st. He has recognized these formations in parts of France and Spain in which their existence was hitherto unknown; 2dly, he has shown that they contain mineral masses which were supposed wholly foreign to them; and, 3dly, he has, on the one hand, augmented the number of their zoological characteristics, and, on the other, has diminished the negative importance hitherto attached to the absence of certain shells. In the south of France these formations have been recognized as forming a subterranean valley, the northern and southern borders of which show themselves by hillocks or mounds of earth, separated from each other, but tracing, by their disposition, two zones from east to west. The northern zone commences in the south of La Vendée, near Rochefort and Royant, and extends to the foot of the maritime Alps. The southern zone rests on the northern declivity of the Pyrenees, commencing from the eastern extremity of the Corbières, and extending as far as Bayonne in a narrow band. At Bayonne it becomes wider, and, entering Spain, extends to Cardonne. The valley inclosed by these zones is almost entirely filled with terrain tertiaire and alluvial earths. M. Dufresnoy remarks that, by a general and almost regular elevation of the granite chain of the Pyrenees, chalk formations have been carried to a great height, acquiring a compact texture and a black colour: Mont Perdu, a summit 3500 metres above the level of the sea, belongs to this class of chalk formations. Another elevation, that of the ophites, has also, though in a much smaller degree, deranged the horizontal position of these deposits; but as the eruptions of ophite, to which this derangement is probably owing, have been much less abundant on the southern than the northern base of the Pyrenean chain, the chalky formations have been much less deranged from their position on the Spanish than on the French side. The proofs which M. Dufresnoy gives of the nature of these formations are quite satisfactory: indeed they have even proved the existence in France of a group of chalk deposits (the Weald group), which had previously been only observed in England. The chalk
deposits of the north of Europe are placed under the terrains tertiaires, and over those known by the name of epiolithe (upper and middle oolite), because they appear to form the upper part of the great oolitic mass of the European Jurassic earth. The parts in the south of France, in which they have appeared covered with the terrains tertiaires, are the Landes, Medoe, the environs of Bordeaux, and St. Paulet, near the Pont St. Esprit: those in which the deposits under the chalk may be referred to the epiolithe group, are much more numerous, chiefly near Rochefort and St. Jean d'Angely.

The principal difference of the physical structure of these chalk formations from those of the north of Europe is, that they are generally in oblique strata, which may be partly occasioned by the elevation of the crystalline rocks, which constitute the Pyrenean chain, lifting up part of the layers of chalk in greater or smaller angles. The mineralogical characters of these deposits in the south of France are, in some respects, similar and in others different from those of the north of Europe; but these differences are of less importance, inasmuch as sedimentary rocks, to which these belong, can never offer those decided and uniform mineralogical characters which distinguish the crystallized rocks: for the latter are formed under the influence of chemical composition, an invariable principle of nature; whereas the former may almost be said to be mechanically constructed, under circumstances perpetually varying and subjected to no certain rule. The chalk formations of the north of Europe are composed (commencing from the surface) of white chalk, a grayish and friable rock called tufous chalk, a sandy rock, filled with green particles, and called glauconie crayeuse, and frequently of a sandy and ferruginous rock. Under this last rock there has been observed in England, and particularly in Sussex, a remarkable deposit of shells and fossil animals of the lake and river species: this is called the groupe Veldien. Sand, tolerably pure freestone, some metallic combinations of hydroxidated iron, and pyrites, are also observed in these earths. M. Dufresnoy has ascertained that the greater part of these substances are found in the chalk deposits of the south of France; and particularly that the groupe Veldien, supposed to be peculiar to Sussex, exists at the base of the Montagne d'Angoulême and at La Grasse. Although this deposit is less distinct in France than in England, it is easily recognized by the argilo-calcareous nature of its rock, by its position, and by its lacustrine shells (melanie, paludinis). The great difference in the mineralogical character of these deposits from those of the north of Europe is the almost total absence of white chalk, which has either never been deposited or else been carried off. The formation generally begins with the tufous chalk. Another very remarkable difference is, that in some cases (particularly at St. Froult, near Rochefort), they contain masses of gypsum, with its accompanying sulphur: by the
derangement of the beds, these masses appear to have introduced themselves from below, and been developed there. Marine salt is also found in these earths; and M. Dufresnoy supposes that the famous bed of sea salt at Cardonne in Catalonia ought to be classed among the chalk beds. But it is in the zoological characteristics of these formations of the south of France that M. Dufresnoy has found the most distinct proofs of his theory, and, at the same time, the most remarkable anomalies. M. Dufresnoy, in addition to the belemnites, ammonites, and other fossils peculiar to chalk, has found in these earths the bulla, the cypræa, the melonía—several kinds of the Venus, the Lucina, the crassatella tumida, the neutina perversa, and other fossils, which had hitherto been only found in the terrains tertiaires. This would, at first sight, appear to materially lessen the reliance to be placed on the zoological characteristics of strata; but it must be considered that, in judging of the character of a stratum from its zoological characteristics, there are four points to be specially examined.—1. The minute difference of the species of fossils. 2. The geographical position of the bed: this is important, because it is to be supposed that a difference of latitude produced the same difference in zoological productions of the ancient as of the modern world. 3. The position of the different species in the strata. 4. The relative number of the species, which are characteristics of the stratum in question, and of those of which the geognostic position appears an anomaly.

In the present case, M. Dufresnoy informs us that the anomalous or littoral fossils (by which we mean those usually found in the terrains tertiaires) are assembled in layers distinct from those which contain the pelagian fossils (as we may call those fossils which are characteristic of chalk), and appear the results of a separate deposit. We might thence be led to suppose that, while a precipitate of chalky limestone, enveloping the belemnites, ammonites, and other pelagian fossils, was forming at the bottom of the deep seas, a calcareous earth, enveloping the cerites, the ampulla, and other mollusca which could inhabit shallow waters, was simultaneously forming itself near the shores, and in the shallow waters; so that the chalk and the terrains tertiaires would, according to this hypothesis, have been formed nearly at the same time, and in the same seas, but at different depths. Thus the fossils similar to, though rarely identical with, those of the terrains tertiaires, would be the littoral fossils of the seas at the epoch of the formation of the chalk. This would throw light on the nature of the chalk of Maestricht, which is so different from all others, and might be supported by the presence, in that bed, of the metosaurus, an animal which, if a marine animal at all, could only have lived near the shores. But the concurring observations of different kinds, establishing that the chalk deposits and the terrains tertiaires belong to two distinct and probably remote epochs, are too numerous to admit of our adopting the above hypothesis. As, however, the co-
existence of the two species of fossils in the chalk formations discovered by M. Dufresnoy in the south of France appears to break down the distinct line of separation which exists between the terrains tertiaires and the chalk in the north of Europe, we must have recourse to the consideration of the relative number of the two species of fossils, in order to see whether the anomaly and confusion are so great as might be feared. The number of species of shells and zoophytes which M. Dufresnoy has distinguished in these strata is 124, of which 110 are determinable as genera. Of these there appear to be only the following five which distinctly belong to the terrains tertiaires, as well as to the chalk strata:—Cardium aviculare, Crassatella tumida, Cerithium diaboli, Nerita perversa, and Turbinallia elliptica. There are about ten others, to which M. Dufresnoy has not been able to assign names, but which he considers to be identical with some of the species belonging to the terrains tertiaires. Thus, at the outside, there are but 15 out of 124 which belong to the two species of deposits; the 209 others have always been recognised as belonging distinctly to chalk formations. M. Brogniart then proceeds to establish, in an elaborate but lucid line of argument, that a slight anomaly of this kind cannot diminish the weight to be attached to zoological characteristics in determining the nature and epoch of a particular deposit. He remarks that it is certain that every new deposit of earth must have been occasioned by some extraordinary convulsion of nature; and that all experience shows us that the animals existing at one epoch differed so materially from those existing at another, as to enable us to distinguish, by their organic remains, the relative epochs of the formation of each deposit; but it does not therefore follow that in each of those convulsions of nature the whole of the then existing species of animals were so completely annihilated as to prevent any of them surviving or re-appearing in the succeeding epoch, in which case the admixture of the different species of fossils will be accounted for; only, as in this case, the number of those belonging to a preceding epoch will bear a very inconsiderable proportion to that of those which properly characterise the epoch under examination. In support of his opinion he cites the chalk formation discovered by M. Merton in 1828, in New Jersey and Maryland, which contains fossil remains similar, though not identical, to those of the chalk of Europe, and also several of those which we have called littoral, and attributed to the terrains tertiaires. Hence he concludes that the zoological characteristics of strata form the surest guides as to their nature and epoch, although their geognostic and mineralogical characteristics may also be taken into consideration as additional evidence. On these grounds, he considers that M. Dufresnoy has fully proved the existence of the chalk formations in the south of France—a discovery which is not only important as a matter of information on the structure of that part of the globe, but as affording a guide for useful mineralogical researches, founded
on the knowledge we possess of what substances are found beneath chalk, though never either above or in them. The Memoir of M. Dufresnoy was ordered to be inserted in the ‘Recueil des Mémoires des Savans Etrangers.’

Boring the Earth.—On the 20th of June a letter was read from M. Jobard, of Brussels, announcing that he had brought to perfection a new machine for boring the earth to any depth, and through any soil. He stated that his plan had been tried with the greatest success in the neighbourhood of Marienburg, where he had rapidly attained a depth of seventy-five feet, through an inclined rock of phylade, mixed with argillaceous flints. By a process something similar, though less perfect, wells have been dug in China to a depth of from 2000 to 2800 feet, through solid rock. M. Jobard anticipates the greatest advantages to geognosy from his discovery; and, with the usual enthusiasm of projectors, looks forward with confidence to the period (not far distant) when we shall be as well acquainted with the centre of the earth as we now are with its surface.

The Lesser Atlas.—At the same meeting M. Cordier communicated some geological observations made by M. Rozet in Africa. M. Rozet is now of opinion that the earths which he had formerly considered as terrains de transition are, in fact, to be classed among those belonging to the epoch of the lias and the calcareous gryphites. The most elevated summit of that part of the lower Atlas visited by M. Rozet, and measured with the assistance of the barometer, was 1399 metres (4590 feet) above the level of the Mediterranean.

Medical Science.

Cure of Fever.—On the 11th of April, M. Rousseau announced to the Academy, that in three distinct cases of recent occurrence, fever had been completely cured by a few doses, of a drachm each, of the powder of holly-leaves, diluted with half a glass of water. The Academy directed the Medical Committee of the Prix Montyon to take cognizance of the cases.

On the 23d of May, M. Deleschamps, a young chemist, announced that he had succeeded in obtaining a new vegetable matter from the bark of holly, to which he had given the name of ilicine, and which may be substituted for quinia in the treatment of intermittent fevers. It will be recollected that an extract of the bark of willow, called salicine, has already been suggested as a substitute for sulphate of quinia; should experience prove that the qualities of these two matters are at all comparable to those of quinia, their low price will render the discoveries highly important to the lower classes.
Lithotrity.—On the 18th of April, Dr. Civiale read a memoir on the diseases of the bladder, in which, after arguing on the general advantages of lithotrity, he expressed his opinion that, in cases where that mode of operation is absolutely impracticable, and it therefore becomes necessary to cut into the bladder, the hypogastric operation, as now simplified, is generally preferable to those by the rectum or the perineum. He detailed at great length the particulars of a case in which he had successfully operated in that manner on a Russian nobleman, who had been suffering the most intense agony for more than eight years. The irritation and inflammation of the parts were so great as to render lithotrity impossible, and even excision quite a forlorn hope. The operation was, however, performed, and the patient perfectly cured in twenty-eight days. From the details of this case he draws the conclusions—that the cystotomie sus-pubienne may be performed in cases which at first sight appear most opposed to it; and that the passage of urine through the wound is no serious obstacle to its cicatrization. He also takes occasion particularly to advise all medical men to carefully examine, in each case submitted to them, whether the inflammation of the urethra be the result of local irritation of the urethra itself, or sympathetic, and arising from the diseased state of the bladder, as in the latter case nothing can be done to relieve it until the primary cause is removed by the abstraction of the stone. At the meeting on the 25th of April, M. Larrey read a report on a memoir sent to the Academy some time since by Dr. Civiale, in which he gave the latter great credit for his perseverance in bringing lithotrity to perfection, but regretted that his anxiety to support his favourite theory had induced him to record only the favourable cases, and remain silent on those in which the operation had terminated unfavourably. The reporter said that the official reports of the hospitals proved that the number of patients who have died after being operated on by lithotrity is, in proportion, as great as that of those who have not survived the operation of cutting out the stone. At the subsequent meeting, Dr. Civiale questioned the correctness of this report, and repeated, that, in 152 cases, lithotrity had been successful. M. Larrey’s documents were, however, official; and the real comparative merit of the operations must still be considered as undecided.

New Surgical Instrument.—On the 2d of May, Dr. Tilhol presented to the Academy a new instrument for the purpose of making injections into the cavities of the mucous membranes, and abstracting the liquid contained in those cavities.

Temperature of the Blood.—On the 9th of May, a memoir on this subject, by M. Collard de Montigny, was read to the Academy. It maintains that the temperature varies in the course of circulation, and that (contrary to the received opinion) it is lower when the blood
leaves the left ventricle of the heart than when it enters the right ventricle. M. Collard supposes the proper temperature of the blood to be eight degrees (?), and that the variation depends on physical causes. The novelty of the theory entitles it to mention, but we shall defer any more detailed account of it until after MM. Dulong, Savart and Flourens have made their report upon it.

Use of Gold in cases of Syphilis.—On the 16th of May, M. Magendie made a very favourable report on a work by M. Legrand on this subject. The author establishes that gold acts favourably on the digestive organs, without weakening the patient, and at the same time produces an exhilaration of spirits. There are four methods in which it may be advantageously administered; 1st. metallic gold reduced to a state of extreme division; 2d. the oxide of gold with potash; 3d. the oxide of gold with tin; 4th. the perchloride of gold and sodium. Of these the last is by far the most powerful. It is applied by mixing three parts of the perchloride of gold and sodium with nine parts of any inert powder, and administered by way of friction on the tongue, in doses, varying according to circumstances, from $\frac{1}{3}$ to $\frac{1}{2}$ of a grain per day. As much as a grain has been given with safety, but this requires care. This is the least expensive of all the preparations of gold. Next to this in strength is the oxide precipitated by tin, then the oxide precipitated by potash, and, lastly, the gold in a state of division, which is the mildest, and, at the same time, the surest form under which it is administered. It is obtained by dissolving one part of perchloride of gold in fifteen parts of distilled water, and then pouring into it little by little a solution of four parts of proto-sulphate of iron, in sixteen parts of distilled water, until there is no longer any precipitate produced. The precipitates are then collected and preserved for use. This is administered by friction on the tongue, in doses from one quarter of a grain to four grains per day. It may also be administered internally in a spoonful of conserve of any kind. The oxides are employed in the same manner, but in doses of $\frac{1}{10}$ of a grain to one and a half or two grains per day. They are more frequently given internally, either in pills of six grains of oxide, with sixty grains of extract of mezereon, or any other extract of a milder character, divided into sixty pills, of which from one to ten are taken fasting in a gradually increasing ratio, or in lozenges made of six grains of the oxides, with one ounce of powdered white sugar, divided into sixty tablets, to be taken in the same manner. The work, which makes a tolerably thick octavo volume, contains very copious illustrations of the subject, and also of the danger of the use of mercury, of which the examples are striking and well reported. M. Magendie, in conclusion, bestowed high praise on the assiduity and research of M. Legrand, and considered that he had established the beneficial nature of his remedy, although
in administering it great attention must be paid to regulate the
doses according to the strength and constitution of the patient.
The work has been published some years, but has only lately
attracted the attention of the Academy.

New Instrument for Lithotrity.—On the 16th of May, M. Leroy
d’Etoles, to whom we are indebted for most of the instruments used
in lithotrity, presented a new curved instrument, which he uses
to break the stone, in cases in which a straight cylinder cannot pos-
sibly be introduced. The Monthyon prize of six thousand francs
was adjudged to M. Leroy for his various instruments.

Anatomical Phenomenon.—M. Combetti, in the sitting of the 23d
of May, read a memoir, containing the particulars of the case of a
young girl, aged ten, who had recently died in the hospital for Orphans,
Alexandrine Labrosse was born at Versailles in 1821; her father
was healthy, but her mother weak, and worn out by excesses of
every description. The child came into the world meagre, but well
formed; it was very weak, and at two years old had not cut its first
teeth. It was not able to articulate a word until after it was three
years old, and could not stand alone until it had completed its fifth
year. To this backwardness of corporeal development was added a
great imbecility of mind. At nine years old she was admitted into
the Orphan hospital, at which time she was labouring under a pa-
ralysis of the abdominal extremities. M. Combetti did not see her
until January, 1831, when she had been three months in bed. Her
face was pale, and her features emaciated and oppressed with stu-
por; she never spoke, and, when addressed, replied in monosyl-
lables, but always to the purpose; she lay constantly on her back,
keeping her head inclined towards the left side; she could scarcely
move her legs, but they retained all their sensibility; her hands were
unaffected. She had long had glandular swellings of the neck; she
afterwards had a mild carbuncle on the buttock, and an ulceration
of the foot. She was ultimately attacked by an intestinal affection,
which carried her off on the 25th of March last. The body was
dissected thirty hours after the death. The lungs were found
crepitans, but full of miliary tubercles. The intestinal surfaces
offered no appearance beyond what was usual in cases of similar
disease. The cranium was of the ordinary thickness; the meninges
offered nothing particular, the brain appeared in its proper state,
except that it was rather large. A small sanguineous effusion of
recent date was observed in the thickness of the left posterior lobe.
The tentorium of the cerebellum being opened, the marrow cut in the
direction of the occipital orifice and the encephallic mass removed and
turned over, there was observed—1. a large quantity of serosity filling
the occipital fossa; 2. in the place of the cerebellum a cellular, ge-
latinous, semi-circular membrane of about an inch and three quarters
diameter transversely, and connected with the *medulla oblongata* by
two gelatinous processes. Near these peduncles were two small white isolated masses about the size of a pea, upon one of which was one of the nerves of the fourth pair; the quadrigeminal tubercles were uninjured; 3. no appearance of the fourth ventricle; 4. the pons varolii entirely wanting, without any appearance of depedition of substance; the anterior pyramids terminated forkwise by the cerebral peduncle. It appears that the unhappy child had, from her earliest infancy, contracted habits of the most vicious self-indulgence; and M. Combetti, in arguing on the foregoing facts, is disposed to attribute the absence of the cerebellum and pons varolii to a gradual destruction of those parts from disease, and not to any inherent defect of organization. At any rate the case presents the extraordinary fact of the child having lived for some time in the possession of all its faculties, and even of a certain degree of intellect, though deprived of cerebellum, posterior peduncle, and cerebral protuberance. MM. Geoffroy St. Hilaire, Blainville, Magendie, Flourens and Serres have been appointed to examine and report on the case.

Preservative against Small-pox and Measles.—On the 13th of June a letter was read from M. Remy, a physician at Chatillon, detailing some experiments which he had recently made on chloride of lime as a preservative against the small-pox. During the last autumn he had observed that, out of some hundred individuals whom he had vaccinated, nearly five-sixths had not taken the infection properly; whereas in the spring, although he had used matter precisely similar, every case succeeded. He then recollected, that during the autumn he had constantly carried in his waistcoat pocket a small packet of chloride of lime, and felt convinced that the non-reception of the virus must have been occasioned by that circumstance. He, therefore, in a village where the small-pox was raging with such violence that there only remained twelve individuals subject to the infection who had not been attacked, caused those twelve to be washed twice a week with a solution of chloride of lime, and gave them at the same time two drops of the solution in a glass of eau sucrée. Two of them had a slight eruption similar to a vaccine, which has not taken well; the other ten remained constantly with patients suffering from the small-pox, without the least symptoms of illness. In another village where the small-pox also raged, there were fifteen individuals liable to take it; ten of them were subjected to a similar treatment, and wholly escaped the malady; two of the other five caught the complaint. The same treatment was tried on four individuals under the influence of the small-pox; the result was an increase of inflammatory symptoms, which were removed by bleeding; the progress of the eruption appeared arrested, the pustules remained in the same state as when first washed with the solution, and then dried away very slowly. This letter was referred to MM. Magendie and Serres. On the 20th of June, M. Chevalier reminded the Academy that he was the first who had suggested the use of
chloride of lime as a preservative against the small-pox, long before the experiments of M. Remy. He also stated that the chloride of lime might likewise be used as a protection against the measles, by keeping in the chamber of the child whom it was desired to preserve from infection a saucer of dry chloride of lime, renewed from time to time, and dipping its shirts in a solution of one ounce of concentrated liquid chloride in twelve quarts of water.

**Cholera Morbus.**—On the 20th of June a letter was read from Dr. Foy, an eminent physician at Warsaw, the contents of which tend to prove that whatever may be the contagious properties of this disorder, their effect mainly depends upon the predisposition produced by the constitution and habits of those exposed to their influence. Habitual intemperance, disorderly living, and want of cleanliness, will generally expose those addicted to them to the immediate attacks of the disease, while the contrary habits will almost invariably be found preservatives. Dr. Foy imagines that the cholera has its seat in the spinal nervous system, and that all the functions of the skin being impeded, their restoration to their natural activity is indispensable to a cure; hence in the Russian soldiers, whose habits are disgustingly dirty, and whose skin Dr. Foy tells us was, in many instances, covered with filth of more than a twelfth of an inch in thickness, the disease generally terminated fatally. Dr. Foy exposed himself in every manner to the infection; he infused into his own veins the blood of an individual at the point of death from cholera; inhaled the breath of patients suffering under the disease; and even tasted the matter ejected from their stomachs, without sustaining any injury from the experiment beyond a slight nausea and head-ache. Dr. Foy's letter was accompanied by official certificates, affording guarantees of his experience and credibility. He also stated that the use of the tincture of *nux vomica* had been unsuccessful, but that some practitioners had lately used, with good effect, chloride diluted with water.

**Lacteal Infection.**—The same day M. Guyon communicated to the Academy the death of an infant and a dog, who had partaken of the milk of a woman suffering from fever. The former died in thirty hours, and the latter in less than four, exhibiting all the usual symptoms of death from poison.

**Zoology.**

**History of Zoology.**—On the 26th of March, M. Geoffroy St. Hilaire read a memoir entitled 'Du degré d'influence du monde ambiant pour modifier les formes animales composant le caractère philosophique des faits différenciels.' It is impossible to follow the learned professor through his discursive, argumentative theories; it will be sufficient to state his general views, which always possess novelty and ingenuity, and often valuable truth. He commences
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by distinguishing the following epochs of the science of zoology:—In the first, man regarded animals merely as he was induced to seek or avoid them; in the second, he was led by curiosity to examine their different forms, independently of their actual utility; in the third, he felt the necessity of characteristic signs and marks of distinction. The fourth was occupied by the details of nomenclature, description, and classification. In the fifth, zoology attained the rank of a science, properly so called; and the era of philosophical inquiry into the natural relation between the various classes commenced. During the sixth, the idea of a primitive identity of organization began to develope itself, though in a somewhat vague and uncertain manner; and in the seventh, at which we are now arrived, science is occupied in developing the external causes in which originate the various modifications which that primitive identity of organization has undergone in various animals. He remarks, that in order to appreciate the action of external circumstances upon an organized being, it is not sufficient to consider that being in its perfect state of developement, we must study it at different periods of its life. Thus we could not find any good reason for the depressed and semi-elliptical form of the head of the frog, if we confined our inquiries to the full-grown animal; but in tracing its origin, we find that the frog, in its tadpole state, partook of the organization of fishes, that is to say, it breathed through the voluminous gills placed under the back cranium. Now, the bones of the auricular region are the parts covering the gills, so that their developement must necessarily be in proportion to the volume of those gills; thus the disposition of the bones of the head of the frog has relation to the aquatic respiration of the tadpole. M. St. Hilaire then traces the changes of organization through the various phenomena of the universe, showing how the organic developement of animals may have been affected, at various periods of their existence, by the external changes in the material world. He particularly dwells on the changes which may have been produced in the organs of respiration, and the organs dependent on them, by the changes which have gradually taken place in the atmosphere and temperature since the primitive ages; by which he endeavours to account for the modifications which, from a comparison of fossil remains with existing animals, appear to have taken place in various species of the animal kingdom. It is certain, he says, that the atmosphere is no longer what it was, either in its chemical or physical properties; and as the atmosphere cannot be modified without the respiration, and consequently the whole animal economy, being modified also, it follows that the existing animals, though descending directly by way of generation from the ante-diluvian animals, differ materially from them in organization. Man undergoes a change something similar; it often happens that, in consequence of a germ being placed in circumstances different from those in which it ought naturally to be placed, the being to which it gives birth does not resemble its
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father; we then call it a monster, but all the individuals which we consider normal, would be monsters if considered in reference to their original progenitors.

Lusus Natureœ.—On the 18th of April, a communication from M. Leon Dufour, Corresponding Member of the Institut, was read to the Academy. It contained a curious account of an anomalous growth of hair in the region of the sacrum of a young man whom he had recently had occasion to examine for the conscription. This mass of hair perfectly resembled that of the head, both in length, colour, and quality. The skin from which it sprung was as white as the surrounding parts, thus preventing the possibility of the phenomenon being referred to the class of defects known by the name of moles, in which the colour of the skin is always dark, and the hair coarse and short. M. Dufour characterises the case as falling under that class of exceptions to the usual laws of organization which are designated as rudiments, and considers it as presenting the character common to several mammiferœ, of having the lower extremity of the vertebral column covered with long hair. The young man in question did not present any extraordinary development of the vertebrae of the coccyx, and may, therefore, be considered as complete an anomaly as the woman with four breasts and a cow's tail, mentioned by Voltaire in his ‘Philosophical Dictionary.’

On the 23d of May, M. Fabre handed to the Academy a foetus, which had come to its full term, and even lived a quarter of an hour, having but one eye placed in the centre of the brow, and appearing to result from the junction of two eyes closely united. There was no external appearance of nose.

Fossil Remains.—At the meetings of the Academy, on the 2nd and 9th of May, M. Geoffroy St. Hilaire communicated various particulars relative to some fossil remains discovered at Caen, which belong to an animal named by him the teleo-saurus. He exhibited several drawings of these fragments, and also the ventral and dorsal carapaces of the animal: the former differs from that of the crocodile, which has no bony scales, whereas that of the fossil animal is composed of strong bony pieces, while a plate, equally hard, and of proportional dimensions, is under the throat, having, however, two sloping cuts, to admit of the lateral movement of the head; the latter is composed of bony scales, placed over each other, nearly in the same manner as those of the crocodile. From the peculiar organization of these animals, the learned professor concludes that they could never have breathed in an atmosphere similar to that in which we now live, but must have existed at a period anterior to the crocodiles and other animals of that species. The teleo-saurus must necessarily have been a marine animal, and must be referred to the same period as the ichthyosauri, the gryphites, the nautili, and other
molluscae, the remains of which form a part of the marine deposits known by the name of terrain secondaire, or Jurassic formations. No feet of this animal have ever been found; but in the cabinet at Caen there is a block containing the imprint of the whole skeleton of a steneo-saurus, in which is observed the form of the first joint of the hind feet, which resembles that of the ikan dugung. It appears that there was but one middle toe, of a length beyond all proportion, accompanied by the rudiments of a lateral joint, thus, in some respects, resembling the horse, but as well adapted for swimming as the horse's hoof is for walking. M. St. Hilaire, however, thinks it probable that the feet of the teleo-saurus were different from those of the steneo-saurus, inasmuch as there certainly existed great difference in the other parts of the organization. Thus the nostrils of the former are entirely terminal, giving the idea that the muzzle terminated in a sort of snout, while those of the latter are open at the top, nearly in the same manner as those of the gavials. The teeth also of the steneo-saurus resemble those of the gavial, while those of the teleo-saurus are thin, and spring laterally; hence it may be supposed that the former preyed on living animals, while the latter lived on submarine vegetables and algae: indeed, from the granite stones found in the midst of the fossil bones, M. St. Hilaire was inclined to believe that the animal swallowed stones, for the purpose of bruising and facilitating the digestion of the herbs and grains. From these examinations M. St. Hilaire deduces a theory, that there have been three epochs of animal creation. In the first, (to which belongs the teleo-saurus,) animals, without lungs, existed alone; in the second, animals with pulmonary organs began to appear; and in the third, which comprehends the present world, the earth was covered with animals of a species of which no analogous fossil remains have been discovered. It results from this theory, that man is of a very modern origin as compared with the age of the globe.

Bicephalous Lizard.—On the 9th of May, M. Beltrami communicated some curious particulars relative to the two-headed lizard mentioned in our last Number (page 570), which lived five months in the possession of M. Rigel, an apothecary at Argelles. It used its two heads simultaneously for eating when it could seize its food as it liked. If a single insect were presented to it, both heads attempted to seize it, and the one which failed endeavoured to snatch it from the other. When, however, one head was satiated, the other refused food, but if water were offered, the head which had not eaten would drink for the other, which then, in its turn, refused to drink when its companion was satisfied. The animal has five feet, four of which, placed in the usual position, served it for locomotion; the fifth is situated at the point of junction of the two necks, at the upper part of the common body. It has nine distinct toes, evidently resulting from the union of the two fore-feet. This foot, or paw, served it to clean itself, and to carry the food alternately to the two mouths; and
it was remarked that it never presented food to the same head twice in succession, and if it had commenced with the right hand one, it invariably finished with the left. The two heads and necks are of equal dimensions, and perfectly well formed. M. Rigal had endeavoured to preserve the animal from the cold of the winter before last, by keeping it in bed during the night, and found it one morning smothered to death. It has been preserved in spirits of wine, and deposited with the Secretary of the Academy.

Collection of Natural History.—At the same meeting, M. Cuvier mentioned in terms of high eulogium the collection brought from India by M. Delamare-Picot, which he characterized as the most extensive ever made by an individual unaided by funds from government. In the zoological department it comprises 53 species of mammifers, 123 of fishes, 52 of crustacea, 150 of insects, 40 of zoophytes, 30 of reptiles, and 75 of birds; there were more than 400 of vegetables. Many of these species were hitherto unknown, and others were wanting in the Museum of the Jardin des Plantes, particularly the rhinoceros without horns, known by the name of the rhinoceros of Java. The mode adopted by M. Picot for the preservation and transport of his vegetables is worthy of observation. After having dried the plants in the ordinary manner, instead of placing them between sheets of paper, he put them all, pressed one immediately over the other, into flat shallow boxes, the interior of which was covered with oil of petroleum, and which were supplied with camphor and pepper pounded together, and carefully closed in all the joints. The vegetables, so packed, were not injured, either by the damp or by insects, under circumstances in which ordinary herbaries were completely destroyed. The adoption of this plan would save botanists a great deal of trouble and anxiety, and relieve them from the masses of paper which they are now obliged to carry.

On the distinguishing Marks of Venomous Serpents.—On the 16th May, M. Cuvier read a report on a very important memoir by M. Duvernoy, Professor of Natural History at Strasbourg, the object of which is to point out the means of distinguishing those serpents whose bite is rendered dangerous from the venom which they instil into the wound, from those whose bite is accompanied by no evil consequences beyond those of the wound itself. The attention of naturalists has long been directed to this subject in vain. It was formerly supposed that the existence of plates or scales on the top of the head was a sufficient criterion; but a further acquaintance with the reptile tribe has proved that the rattlesnakes, the trigonocephalus, the nain, all of which are decidedly venomous, are furnished with these scales, as well as the most harmless snakes. It was afterwards thought that the jaw, remarkably moveable, and furnished with a large hollow fang, was a sign easy to be recognized, and, in fact, all serpents in which that peculiarity is observed are venomous,
but it has been discovered, some years since, that there are serpents, the jaw of which has not that moveable character, and contains as many teeth as the common snake, but which has in front a fang not easily perceived, but hollow, and instilling venom. But even this was not sufficient, as MM. Leschenault, Delalande, and Boyé ascertained that some serpents, which certainly had no hollow fangs in front of the jaws, were unquestionably of a venomous nature; and it therefore became necessary to seek in some other part of the mouth the source of the poison. Accordingly, MM. de Beauvois, Reinward, Boyé, and Cuvier ascertained that the serpents in question have, in the back part of the jaw, some teeth which are longer and stronger than the others, and are sometimes hollowed in a manner which may be supposed as well adapted to convey poison into wounds as the hollow fang of the viper. The important point to ascertain, therefore, was whether these back teeth were, in fact, connected with glands of a venomous character or not. M. Schlegel, in a memoir printed in 1828, in the 14th volume of the 'Memoirs of the Académie des Curieux de la Nature,' had commenced the investigation, and pointed out the particular glands to which these hollowed back teeth can serve as conducting canals, and which glands may be co-existent with the ordinary salivary glands, as he particularly noticed in the homalopsis monilis. M. Duvernoy, who was not acquainted with the memoir of M. Schlegel, has carried his investigations to a much greater extent, and has given a far better account than previously existed of the venomous and salivary glands, and such parts of osteology and myology as relate to them, and which he has illustrated with carefully executed plates. His observations have been principally directed to the following species: non-venomous, the tortrix scytale, the coluber natrix, the coluber quincunciatius, the elaps lemniscatus, the vipera verus, the naia tridipians, and the crotalus durissus; venomous, with numerous maxillary teeth, the baugurus fasciatus and the pelanus bicolor; and finally, among those suspected of venomous properties, on account of the long back teeth, the coluber plumbeus, the dipsas interruptus, and the homalopsis pantherinus. In describing, in a very perfect manner, the general and particular characters of the organs of deglutition and insalivation, M. Duvernoy had added to, and rectified the previous observations of M. Dugez, particularly with respect to the adductor muscle of the jaws, which he considers to be a dismemberment of the mylohyoidean as well as the mylovaginian of M. Dugez, which is attached to the skin above the large scales of the under jaw. M. Duvernoy has also entered into minute inquiries as to the proportions of the lachrymal gland, and the variation of its position within and without the orbit in different genera and species, and also as to the analogy between the development of that gland and the salivary and venomous glands, and the size of the eye, a point which had been left untouched, even in M. Cloquet's work on the lachrymatey organs of serpents. There are also several new
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details on the variations in the size and development of the sub-maxillary or common salivary gland, depending on the existence or non-existence of a venomous gland. All M. Schlegel's observations on the difference and co-existence of the two glands had been previously noticed by M. Duvernoy, who adds to them several new remarks, particularly respecting the muscle of the venomous gland, which appears to be an external temporal muscle, generally attached to the envelope of the gland, and descending to the lower jaw, without being attached to the top of the temporal fossa, but occasionally, as in the naia and the bougares, composed of two distinct portions. His most particular attention, however, was directed to the serpents having the long back teeth, for the purpose of ascertaining in which of them there exists the venomous gland, and in which this elongation of the teeth does not denote any specific secretion. When this gland does exist, it is frequently joined to the sub-maxillary gland by a very thick cellular tissue, and may, therefore, be easily confounded with it. The existence of this gland is certain in the coluber Esculapii of Linnaeus, in the coluber cerberus of Dandin and Cuvier, the homalopsis pantherinus of Boyé, and in a dipsas, the baugarus interruptus of Oppel; all those, therefore, are venomous, and illustrate the observations of M. Boyé, who had ascertained, from experiments made while the reptiles were living, that the dipsas and the homalopsis are venomous. The genera dendrophis, dryenus, and xenodon have also the back teeth large, and even in the dryenus nascetus, the largest tooth is hollowed like a canal; but as M. Duvernoy has not found any specific or venomous gland, he concludes that they are not poisonous. These circumstances explain the contradictory testimony existing respecting the venomous qualities of particular serpents, and at the same time prove that the class in question must be far less dangerous than those in which the fang conducting the poison is in front, because unless the object bitten be sufficiently small to admit of its being taken into the mouth of the serpent, and thus brought into contact with the back fang, no poison will be communicated to it, and only a common wound be produced; so that a person bitten in the leg or arm would suffer no injury beyond the actual bite, while another, whose finger was inserted into the mouth of the reptile, would be poisoned. Hence, M. Duvernoy concludes that the principal use of these posterior fangs is to kill the small animals which the serpents take into their mouths alive, and that they are not of much advantage as a means of attack or defence against external enemies. In the course of his Memoir, M. Duvernoy remarks, that in many serpents the spleen is closely attached to the pancreas, which probably led M. Meckel into the error of doubting its existence. This memoir was ordered to be inserted in the 'Recueil des Savans Etrangers,' and the various preparations presented to illustrate the subject were deposited in the Gallery of Anatomy in the Museum of Natural History.
Organic Symmetry.—On the 23rd of May, M. Dutrochet, a Corresponding Member of the Institute, addressed a letter to the Academy, relative to the want of symmetry observed in the internal organs of a great number of animals arrived at their fullest state of development. He does not agree with Bichat, in supposing this want of symmetry to be an essential character of the organs; but, on the contrary, agrees with M. Cuvier, that in animals with long bodies there is an evident symmetry existing, which is still more striking in the foetus during the first periods of its existence: the alimentary canal is then extended in a right line from the mouth to the anus, and is perfectly symmetrical. In the original plan of organization, the symmetry was as perfect internally as it is externally; and if it be afterwards destroyed, it is by a species of abortion of one of the sides. In the larva of the aquatic salamander, when it first leaves the egg, the alimentary canal is perfectly symmetrical. On its two sides, near the beginning of the intestine, are perceived the liver on the right, and the spleen on the left, forming almost a perfect symmetry, as there is scarcely a perceptible difference of size, and the form, as well as the position, is precisely similar. In process of time, however, the left liver, or spleen, becomes an abortion, and is consequently without functions; so also in the primitive organization of insects, the biliary organs are symmetrical. Sometimes two symmetrical organs will both become abortions: they will then be useless to the body, serving only as indications of the primitive organization. Such M. Dutrochet supposes to be the history of the capsule renales.

Classification of Lusus Nature.—On the 11th of April, M. Geoffroy St. Hilaire communicated the substance of a memoir which he had prepared, on the classification of a particular family of lusus nature, which he considers as forming a regular series of anomalous beings, the fundamental character of which depends on the union of the upper part of the nervous cerebro-spinal system of two individuals in a single system, which is either doubled by the fusion of two complete systems, or single by the combination of two corresponding halves. The encephalus is to be considered as composed of four systems of lobes—the spinal marrow, the cerebellum, the optic or quadrijunal lobes, and the cerebral lobes. In the family in question, the ventral regions remain distinct; the two beings are perfectly separate, and subject to the ordinary rules of organization below the navel, but above it they are united and confounded. The vertebral columns inclined forwards unite beyond the atlas, and each produces half of the cephalic elements which terminate them. The following are the names and characteristics of the four classes into which M. St. Hilaire divides this family:—

1. Deradelphus—Cephalic elements double as far as respects the medulla oblongata and the occipital part of the brain (hypertrophie de l'occipital). The rest of the head single.
2. Synotus.—Cephalic elements double as to the medulla oblongata and cerebellum; the rest of the head single. Ears supernumerary, and united behind the head.

3. Eniops.—Cephalic elements double as to the medulla oblongata, cerebellum, and optic lobes; rest of the head single. Supernumerary ears behind the head, and an additional eye in the sinciput.

4. Janiceps.—The whole encephalus and organs of senses double; the faces opposite each other.

Examples exist of each of these classes, but the first has only recently been met with. The name given by M. St. Hilaire to the family is that of "Monstres bicorps unicephales."

Miscellaneous.

Chronology of the Egyptians.—A considerable portion of the sittings of the 4th and 11th of April was taken up by the communication of a memoir by MM. Biot and Champollion on this interesting subject. It is well known that the Egyptians divided the year into twelve months of thirty days each—which, with five intercalary or supplementary days, completed the number of 365. Twelve great divinities presided over the twelve months of the year, five others over the five intercalary days; thirty genii regulated the thirty days of the month; and the twenty-four hours of the astronomical day were under the protection of twelve gods and the same number of goddesses. This year of 365 days was, however, about a quarter of a day shorter than the solar year, whence the first day of the month of Thoth, which began the year, was perpetually in advance of the sun's progress in the ecliptic: so that if the 1st of the month of Thoth occurred at the vernal equinox, it would, in four years, be one day before it; and so on in progression until the expiration of 1506 years, when it would again occur at the precise period of the equinox. Hence the Egyptian year was termed annus vagus; and so great was the attachment of the country to it, that the kings, on coming to the throne, were compelled to take an oath against allowing any change to be made in the mode of computing the year; and the compulsory correction made in the calendar by Augustus, twenty-four years before Christ, was considered one of the bitterest fruits of the Roman conquest. This attachment was not founded on ignorance; on the contrary, the Egyptians were well aware that the solar year was about a quarter of a day longer than their annus vagus, and were probably even the first to communicate that fact to the Greeks. This strong attachment to the annus vagus is the more remarkable, when we consider that none of their monuments give us any reason to suppose that these years were connected by them in any regular chronological series; on the contrary, all the Egyptian dates which have reached us are reckoned...
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from the commencement of the reign of each king: so that, in order to establish the historical succession of events, it would apparently be necessary to have a chronological canon, indicating the number of years of each reign; and many antiquarians have supposed that this was the case. It is, however, singular, that in the numerous Egyptian monuments with which we are acquainted we find no traces of any such canon—except, indeed, the chronological canon of Ptolemy, and the fragments of the chronicle of Manetho, both of which are of very limited extent. M. Biot seeks to prove, that the attachment of the Egyptians to the annus vagus arose from the fact of its containing a natural cycle, specially adapted to their country: so that, by means of symbolic signs attached to the different days of the annus vagus and to certain epochs of the true solar year, they could, with the utmost facility, connect these two systems of years, and thus fix the dates indicated by the anni vagi, with as much precision as we can do by our present calendar. M. Champollion, in his late researches, has ascertained that the Egyptians divided the year into three equal portions of four months, or 120 days each (the five supplementary days having a separate and distinguishing mark): these were represented by symbols illustrative of the periods of vegetation, harvest, and inundation. The inundation of the Nile commences invariably at the summer solstice: it attains its greatest elevation in 100 days, and then, after remaining stationary a few days, it begins to recede, and the ground is sown while yet moist; so that in 120 or 125 days after the summer solstice, the period of inundation ends, and that of vegetation commences. Four months afterwards the harvest begins, and lasts four months; and thus ends the agricultural year. The Egyptians were well aware, that as the inundation of the Nile invariably commenced at the summer solstice, which would therefore be the first day of the ninth month, or third period of the year, the first day of Thot (the first month) ought to be 125 days after that solstice. They had, therefore, only to observe the degree of variation existing between the time of the solar year at which the 1st of Thot occurred in any given year, and that at which it ought in reality to be found, and a perpetual and unfailing calendar was at once constituted, showing with the utmost precision in what part of the cycle of 1506 years any given year was. It only remained then to ascertain how many of these cycles there had been, or, in other words, when the Egyptians first adopted the mode of computation by the annus vagus, and observed its variation from the true solar year. It is evident that the system must have commenced at a period when the solar year and the annus vagus were in accordance; and as we know that when Augustus altered the mode of computing the year, twenty-four years B.C., the 1st Thot of the annus vagus corresponded with the 29th of August, it is easy, by fixing the date of the summer solstice, to ascertain when the 1st day of Thot did in fact occur 125 days after the summer solstice. The following table shows all the
periods before the Christian era at which that coincidence took place:

<table>
<thead>
<tr>
<th>Julian era</th>
<th>Years before Christ</th>
<th>Date of 1st Thoth</th>
<th>Date of Summer Solstice</th>
</tr>
</thead>
<tbody>
<tr>
<td>76</td>
<td>4790</td>
<td>December 4</td>
<td>August 1</td>
</tr>
<tr>
<td>1429</td>
<td>3285</td>
<td>November 22</td>
<td>July 20</td>
</tr>
<tr>
<td>2934</td>
<td>1750</td>
<td>November 11</td>
<td>July 9</td>
</tr>
<tr>
<td>4439</td>
<td>275</td>
<td>October 31</td>
<td>June 27</td>
</tr>
</tbody>
</table>

It is therefore to one of these four periods that the commencement of the Egyptian mode of calculation must be attributed. The last of the four may be excluded from our consideration, because the researches of M. Champollion have proved incontestably that the *annus vagus* of 365 days was used by the Egyptians previous to the year 1600 before Christ. We have therefore only to examine the probability attaching to the first three; and for this purpose it is important to remark that the Egyptians, in their mythological system, considered the star Sirius as the power influencing the rising of the waters of the Nile; and therefore it must be presumed that their system commenced at a period when the heliacal rising of Sirius coincided with the summer solstice, or rising of the Nile. This occurred for the first time in the year 3285 B.C., previous to which the rising of Sirius was more and more removed from the summer solstice; the date of 4790 B.C. may therefore be also put out of the question, and the doubt only remains between the years 3285 and 1750 B.C. The first, as we have before seen, coincides precisely, and therefore furnishes a fair presumption that the Egyptians at that period formed their year of 365 days; but we cannot be quite certain of the facts, because, supposing the addition of the five days not to have been made until 1750, and consequently calculating backwards by years of 360 days, we should only make a difference of six Julian years, placing the period of coincidence between the summer solstice and the 1st day of Paschous (the ninth month) in the year 3291, when the heliacal rising of Sirius differed only a day and a half from the summer solstice—an error which may easily be committed in determining the heliacal rising of a star from observation only.

From these observations, we arrive at the following important conclusions:—1st. That the Egyptians, knowing that the cycle of variation between the solar year and the *annus vagus* consisted of 1506 years, could always tell, by observing the number of days which the 1st of the 9th month (Paschous) varied from the summer solstice, in what year of the cycle they were; and that at the time of the alteration of the year by Augustus they could not be in more than the third, and perhaps only in the second of such cycles from the time of their first calculating in that manner. 2nd. That as all the data on which this calculation of the year is founded relate to the phases of the Nile, it is evident that it is of Egyptian, and not of Chaldaic origin. 3rd. That as the phenomena relating to the Nile continue at the present moment to occur in precisely the same manner and at the same intervals as at the commencement of the
Egyptian calculations, it is evident, that for 5000 years the distribution of the terrestrial heat on the surface of the earth has remained the same, as any change must have affected the periodical rains of Upper Ethiopia, the rising of the Nile, the duration of the inundation, &c.

Protection of Firemen.—On the 4th of April, M. Gregori communicated some details of the experiments recently made in Italy by the Marquess Origo, the commandant of the firemen at Rome, with a view to guarantee them from the effects of entering houses while a prey to conflagration. Acting on the received opinion that the Romans employed a mixture of clay and vinegar to extinguish flames, he tried that mixture in every manner, but it produced no satisfactory result. He then dipped two complete suits of firemen’s dresses, including boots, gloves, and two cowls, made of the same cloth as the dresses, in a solution of sulphate of alumine and sulphate of lime, and, when dry, saturated them with soap water. Two firemen were clothed in these dresses, and their faces covered with incombustible masks, covered with cloth saturated with a saline solution; the openings for the eyes were covered with a web of amianthus, and small damp sponges were placed in their mouth and ears. Thus protected, they entered a house, 23 feet long and 3 feet wide, filled with burning wood, which they traversed ten times without the slightest injury. Their clothes were not damaged, although they had remained fifteen minutes exposed to the action of the flames. The only effect produced on the men was the increase of the pulsation from 70 to 125. These dresses cost but two pounds sterling each; and are, therefore, in that respect, more eligible than those composed of amianthus, as recommended by the Chevalier Aldini. M. Origo also extinguished flames of considerable violence by playing on them with the solution of sulphate of alumine and clay, by means of a common engine.

Transport of Edifices.—On the 9th of May, M. Gregori alluded to a circumstance mentioned in a late Number of the ‘Journal des Artistes,’ of a rock of granite, 42 feet long and 27 high, having been transported from the bay of Finland to St. Petersburgh, to serve as a pedestal to a statue of Peter the Great. He stated that a much more remarkable fact had occurred at Crescentino in 1776, when a common mason, named Serra, succeeded in transporting a brick belfry (which he had contrived to cut from its base without injuring the walls) from one church to another, at a considerable distance. While it was being moved, a man inside rang the bells. A model of the machine, employed in the transport, was deposited in the library of the Institute.

Travelling in India.—At the same meeting, M. Elie de Beamount read extracts from two letters, which he had received from M. Victor Jacqueminot, a French naturalist, travelling in India. M.
Jacqueminot censures the name of Valley of Dhoon, given by the English to the valley at the entrance of the Himalaya, as being a mere pleonasm, the word Dhoon signifying valley. The Valley of Dheynia is its proper name: it is a longitudinal valley, hollowed between the foot of the Himalaya, properly so called, and the raised diluvial earth. Thence he visited, on foot, the sources of the Jumna. In this expedition he passed over heights of an elevation of 5550 metres (18,208 English feet). He penetrated more than once into the Chinese territories; and, in returning towards Ladack, he slept at a village called Ghjourrneul, situated on an elevation of 5000 metres; on the Indian side of the Cordilleras, he did not observe any village at a greater elevation than 2700 metres. Cultivation, also, on the south side, stops 2000 metres below the level which it attains on the Thibetian sides of the descent. This difference arises not so much from the temperature as from the state of the sky, which is cloudy and rainy on the Indian side, and pure and free from humidity on the other side of the Himalaya. From a variety of geological observations, M. Jacqueminot is induced to think that there exists a difference in the age of the Thibetian and southern chains of the Himalayan mountains; an observation which M. de Beaumont had already made relative to different chains of the Alps. M. Jacqueminot also mentions the uncertainty of correspondence in that part of the country,—as, in addition to the ordinary casualties of letters, the couriers between Benares and Calcutta are occasionally devoured by the tigers en route.

Paganini.—On the 16th of May, Dr. Bennati read a physiological notice of this extraordinary man, in which he gives it as his opinion, that the prodigious talent of this artist is mainly to be attributed to the peculiar conformation which enables him to bring his elbows close together, and place them one over the other, and to the elevation of his left shoulder, which is an inch higher than the right one,—to the slackening of the ligaments of the wrists, and the mobility of his phalanges, which he can move in a lateral direction at pleasure. Dr. Bennati also alluded to the excessive development of Paganini’s cerebellum, as connected with the extraordinary acuteness of his organs of hearing, which enables him to hear conversations carried on in a low tone at a considerable distance. M. Geoffroy St. Hilaire remarked that he had been particularly struck with the prominence of the artist’s forehead, which hangs over his deeply-seated eyes like a pent-house.

Oil Cloths.—On the 23d of May, M. Chevallier pointed out a very simple method of removing the unpleasant smell which has hitherto militated against the use of oiled or varnished cloths and stuffs. It is merely to expose them to the action of a chloric fumigation in a close room.

Prevention of Falsification of Written Instruments.—The attention of the French government has long been directed to the possibility of finding some means of preventing writing being chemically
discharged from papers and other documents, either for the purpose of falsifying the contents, or for making a second and fraudulent use of old stamps. With this view, the Academy of Sciences was directed to take the subject into consideration; and a committee, consisting of MM. Gay Lussac, Dulong, Chaptal, Deyeux, Thenard, D'Arcet, Chevreuil, and Serullas, was appointed for the purpose. The attention of the public was called to the subject, and a great number of specimens of ink, alleged to be indelible, were forwarded to the committee. Numerous experiments were made; and on the 30th of May and 6th of June the report was read to the Academy by M. D'Arcet. It is unnecessary for our purpose to follow the reporter through his elaborate history of the different manufactures of ink in different ages, or the detail of the experiments made with the various samples submitted to the committee: it is sufficient to state the conclusions, which were unanimously adopted as the results of the investigation. These were, that the falsification of written documents will be fully prevented by the use of ink prepared in either of the two following manners. 1. Indian ink (or, in its absence, the imitation of it made in Europe with soot and animal glue or gum), dissolved in a mixture of water and muriatic acid, of the specific gravity of 1010 (1 1/2 degree of Beaumé's instrument.) This ink may be prepared for fourpence English per quart. 2. To a solution of acetate of manganese, of the specific gravity of 1074 (10 degrees of Beaumé), add half its volume of solution of carbonate of soda crystallized, saturating it at about 166 per cent.: dissolve Indian ink in this liquid, and writing traced with it will become perfectly indelible on being exposed to the action of the vapour of liquid ammonia. The committee lay down, as a general principle, that no ink, kept in a liquid state, can be indelible, as the colouring matter, from its excess of density, will necessarily be deposited. Additional security will be obtained by writing on paper so prepared, that even if the ink could be discharged, it would necessarily be seen that it had been so discharged. Thus, M. Coulier proposes a paper, having printed on each sheet, lines and patterns, so complicated, as to defy forgery, and struck off from a steel plate damasked with aqua-fortis. The ink with which this is printed would be discharged by chlorine, so that the superjacent writing cannot be destroyed without also destroying the drawing. This plan is excellent for bills of exchange and other small documents; but from the expense and delay occasioned by the engraving and printing, the designs would be ill adapted for legal proceedings and public documents. M. Chevallier proposes a paper coloured in the pulp with colours liable to be discharged by all the known re-agents, but this might easily be recoloured when the alteration is made. M. Maimu suggests adding to the pulp of the paper filaments of wool, cotton, or hemp, dyed of different colours, some of which will be acted on by the acids, and others by the alkalies, but all liable to be discharged by chlorine. When these colours are discharged, it is almost impossible to restore them; but the writing may, in some cases, be effaced without any
sensible alteration in the colour of the filaments; and on the other hand, that colour will frequently change by simple exposure to the air, without any re-action being used. Mr. Coulier's method is by far the best, but has the disadvantage, that all designs easily dischargeable from the papers may become injured by time or accidental circumstances, a consideration which, in cases of forgery, would tend to render probable the impunity of the guilty by the fear which would be entertained of condemning the innocent. The use of these prepared papers must, therefore, be considered as very secondary, the main security must be found in the indelible inks. The discharge of the writing from old stamped documents, and the consequent fraudulent use of the stamp, may be prevented, 1st. By printing on all stamped paper, by means of a cylindrical press, an engine-turned vignette, placed on the right of the stamp, in the centre and along the whole length of each sheet. 2nd. By employing, in printing these vignettes, a colour having for its base the black precipitate formed in the dyeing coppers of hatters, or ink thickened in the manner adopted in the manufactories of painted cloths; and 3rd. By marking on all stamped papers the date of their fabrication, either by printing it in the pulp, or engraving it on the vignette or the stamp; or, more simply still, by making the dry stamp, impressed on each sheet of paper, revolve, so as to affix a new date each year. This report was ordered to be transmitted to the Minister of Justice.

Gelatine.—The discovery of Mr. D'Arcet, member of the Institute, of the means of preparing the gelatinous matter of bones, so as to form a cheap and wholesome article of food, has excited great attention in Paris. More than two years have elapsed since the discovery, and the system of M. D'Arcet has been adopted in several of the hospitals, and in the Maison de Refuge pour l'Extinction de la Mendicité of M. de Belleyme. The gelatine has also been used in making sea-biscuits, which were used by the troops during the late expedition against Algiers. The mode of preparing both the gelatine and the biscuits is minutely laid down in the pamphlets published by M. D'Arcet. These experiments had invariably been attended with success; but on the 6th of June, M. Donné, a young medical student, communicated to the Academy some remarks tending to throw a doubt on the subject. He stated, that being deeply impressed with the importance (particularly to the lower classes) of ascertaining whether the gelatine did really possess the nutritive qualities attributed to it by M. D'Arcet, he resolved to go through a series of personal experiments on the subject. With this view, recollecting that ten grammes of dry gelatine were stated to be equivalent to half a litre (about two basins) of the best meat broth, he began by taking that quantity every morning with three ounces of bread, and gradually increased the quantity up to fifty grammes, which constituted his sole nourishment up to six o'clock every day;
the gelatine was differently flavoured, so as to prevent its exciting any feeling of nausea or disgust. During the six days which this experiment lasted, M. Donné experienced a constant sensation of sinking and feebleness, and on the sixth day found that he had lost two pounds weight. The next week he substituted ordinary meat broth for the gelatine, taking a litre and a half (about five or six bowls), and from four to five ounces of bread daily; during this week he experienced no sensation of feebleness, and at the end of it had regained a pound and a half of his lost weight. At the same time M. Donné tried similar experiments on two dogs, giving the one, gelatine mixed with a little bread, and offering the other nothing but simple gelatine. The former at first refused it, but at length ate daily as much as was equivalent to twelve or fifteen half litres of good broth. On the sixth day the dog had lost four ounces in weight, and was so voracious that he even greedily devoured some white lead prepared for cleaning plate, and during the second week totally refused gelatine, living only on about an ounce and a half of bread which was given him per day. He ultimately terminated the experiment by climbing to a great height, and taking possession of a quantity of boiled beef which was supposed to be out of his reach. The other dog could not be prevailed on to touch the gelatine, even after being for five days totally without food. M. Donné, therefore, considered it cruel to pursue the experiment further, and gave him his usual food. From these circumstances M. Donné was induced to doubt the nutritive qualities of gelatine, and begged the Academy to appoint a committee to investigate the subject, which was accordingly done. At the succeeding meeting (13th June) M. D'Arcet addressed some observations to the Academy on the subject alluded to by M. Donné; he stated that butchers' meat contained, on the average, in every 100 lbs.—

| Dry meat | 24 lbs. |
| Water   | 61     |
| Bone    | 15     |
|         | = 100  |

Bones contain, on an average,

| Earthy substance | 60 lbs. |
| Gelatine         | 30      |
| Fat              | 10      |
|                  | = 100   |

From this calculation it is evident that the 15 lbs. of bone contained in every 100 lbs. of meat would furnish \( \frac{40}{100} \) of their weight, or 6 lbs. of animal substance, so that 100 lbs. of meat, which now furnish but 24 lbs of dry meat, might, by rendering the gelatine and fat of the bones available, supply thirty, or, in other words, four oxen would furnish as much alimentary substance as is now obtained from five. With respect to the nutritive and salubrious qualities of gelatine, he remarked, that the committee appointed by the Faculty of Medicine, consisting of MM. Le Roux, Dubois, Pelletau, Du-
meril, and Vauquelin, after having given gelatine soup to forty patients and others, during a period of three months, came to the conclusions:—1. That the use of gelatine was both an amelioration and a source of economy in the alimentary system. 2. That gelatine soup is at least as palatable as the ordinary hospital soup; and, 3. That gelatine is nourishing, easy of digestion, and wholesome, and cannot, in any manner, be productive of injurious effects on the animal economy. The apparatus in the hospital of St. Louis is capable of preparing nine hundred soups per day; it has been in use twenty months, and has supplied 550,800 portions of gelatinous food. Numerous reports have been made on the subject to the general administration of the hospitals, all of which agree in stating that the change in the mode of nourishment is a decided improvement; that the convalescent patients acquire strength much more rapidly than before; that it is a source of economy highly important to the poor; that part of the meat formerly employed in making soup may now be given to the patients, either roasted or in other forms, and, finally, they all recommend the adoption of the system of gelatinous nourishment in all similar establishments. At the Hotel-Dieu, 443,630 rations of gelatine have been furnished in fifteen months and a half; and six reports have been made, all of which are equally favourable with those above referred to. They state particularly that since gelatine has been employed, thirty kilogrammes of roast meat may be given to the patients daily, without reducing the quality of the soup at all below its former standard.

When M. D'Arcet had concluded his remarks, M. Gay Lussac animadverted in strong terms on the injustice and insufficiency of the mode of experiments adopted by M. Donne, which he characterised as wholly inconclusive, although calculated to produce a most injurious effect on the public mind, which is always easily impressed with the idea that the poor are neglected, particularly in hospitals. He reminded the Academy that it was well known that no single substance was alone sufficient to support animal nature; that animals fed on sugar alone had died from inanition; yet it would not be pretended that sugar is destitute of nutritive qualities; and though the nutritive qualities of potatoes, taken with other food, are universally known, a dog fed wholly on that vegetable dies in six weeks; whereas M. Donne wishes it to be supposed that because two dogs refused to live upon gelatine, administered alone, we know not how, and because M. Donne himself grew thin on a sudden adoption of simple gelatine diet, the adjunction of gelatine, as an addition to, and taken in conjunction with animal food, is wholly without advantage. On the 20th of June, M. Donne replied to M. Gay Lussac, by saying that his sole object in proposing the question was to have it fully and fairly investigated; since if it can be established that gelatine does possess the nutritive qualities ascribed to it, the advantage to the poorer classes will be immense; whereas, on the other hand, should they be induced to employ the bones as a means
of nutriment, when the fact may turn out to be that the gelatine is not nutritious, their condition is rendered more deplorable than before. In conclusion, he said that he rendered full justice to the active and pure philanthropy of M. D'Arcet, which had induced him to make the greatest sacrifices both of time and money, in order to bring the gelatinous system to perfection.

**Mirage by Suspension.**—On the 20th of June, a letter was read from M. Rozet, stating that he had frequently remarked this atmospheric phenomenon in the neighbourhood of Algiers, and particularly on the 27th of June, 1830, when about ten o'clock in the morning, at which time the sky was perfectly clear, and the thermometer at 21° (Reaumur), he distinctly saw, when looking at the line of battle formed in the camp at Staonelli, two images of all the objects, the mirage being about half as strongly marked as the real image, but still perfectly distinguishable and elevated above the object about one-fourth of its height, deviating a little laterally. On the Algerine tents, surmounted with tin spheres, with a crescent on the top, the image of a second crescent forming a tangent to the first, was distinctly visible, so that, at first sight, it appeared as if there were two crescents to each tent. When the images are reversed, they are rarely clear, and have always a perceptible movement of undulation.

**Climate of Algiers.**—The same letter stated, that whenever the south wind blows in the neighbourhood of Algiers, the temperature is raised from 5° to 10° C. (41° to 50° F.) On the 17th of September the thermometer stood at 39° (108° F.) in the shade. Those who happened to be affected by drinking at that time, suffered severely, falling down insensible. This wind rarely lasts twenty-four hours, and occasions as much inconvenience to the natives as to the French. Storms are not frequent at Algiers, but when they occur they are of great violence. On the 8th of May last, the whole horizon was a sheet of flame; a strong white light rested for half an hour on the extremities of the flag-staffs of the forts of Algiers and its vicinity, and the officers, who were walking bareheaded on the terrace, felt their hair stand on end, and perceived a luminous star at the extremity of each; the same species of star was observed on the ends of the fingers when held upright in the air, but disappeared when held downwards. During these storms every one is affected with great lassitude, particularly in the legs, and experiences strong nervous agitation.

**New Chart.**—At the same meeting M. Coplin presented a topographical chart of the islands of Perouse, in which, by a new plan of drawing, in imitation of relief, he has succeeded in so well availing himself of the process of shading, that not only the geological constitution, and the direction of the declivities, but also the variations in the surface of the different mountains are distinctly exhibited to the eye.
Indian Antiquities.—On the 22d April, MM. Quatremère, Ladjard, and Abel-Rémusat, made a report on the antiquarian part of M. Lamare-Picquot’s collection of curiosities, brought from Hindostan. The zoological part of this splendid collection has formed the subject of a report to the Académie des Sciences (vide p. 157), and the present reporters had, therefore, only to occupy themselves with such parts as tended to throw a light on the civil and religious manners and customs of the Hindoos. The greater part of the articles relating to the Brahmin religion are from Calcutta and its neighbourhood; those which relate to the worship of Buddha are originally from the Burmese empire, whence they were taken during the war with the English in 1825; and a few remarkable curiosities are from the isles of the Ganges. There are about fifty figures representing the divinities of the Brahmins; these are in terra cotta, marble and bronze, and present images (some of them in bas-relief) of Brahma, Vishnou, Sheva and his wife, Parvati, Krishna and his wife Radha, Ganesa, Balarama or Vishnou as a child, Jagher-nout, Dharma-Deva, or the god of the law, under the figure of an ox; Dourga the wife of Sheva; Kali, the same goddess with the attributes of goddess of death, those of protectress of the universe, and those of her combat with Mahichaasoura, the genius of evil, under the form of a buffalo. There are also several mythological subjects, executed on pasteboard, by Hindoo painters; and a large picture representing the combat of Rama against Ravana, the tyrant of the isle of Lauka, a subject taken from the Ramayana or the Baghavata-Pou-rana. M. Lamare-Picquot has also brought over a number of vases, lamps, and other religious and domestic vessels of the Hindoos. He has also succeeded in procuring three or four Bercho-cath, or pieces of carved wood, representing towers with several stories, enriched with a variety of paintings and ornaments. These are carried in the funeral processions of the Hindoos, and then placed near a pagoda on the banks of the Ganges, or some other consecrated river. The collection also includes a variety of exact models of the Hindoo temples, and a sort of fetish, found in an island of the Ganges, representing a head surmounted by a rudely shaped mitre, and coloured equally coarsely. The reporters have not discovered who is represented by this figure. The figures relating to the worship of Buddha are fewer in number, but of considerable dimensions. There are thirty statues of Gaouatama in terra cotta, wood, copper, marble, and alabaster, all exhibiting traces of gilding, and varying in size from one to three feet. This personage is always represented in the act of divination, in a sitting posture at the moment of inspiration, the head surmounted with the characteristic tubercle, the hair in ringlets, half naked, and the right hand pendant. Two only of these statues have inscriptions, one of
Miscellaneous Scientific Proceedings

which is in Burmese, and the other in Bengalese. Smaller figures in bronze and lead represent other and secondary divinities. One of the rarest pieces is a small group representing eight divinities present at the birth of a Shakia. There is also a fine and large bas-relief in terra cotta, of Burmese workmanship, which was intended to be placed over the entrance of a temple; it represents two lions, painted red, in an attitude of repose, and separated by stalks of ananas, reminding us of the celebrated religious monuments of Western Asia. The objects not relating to religious ceremonies are figures of different classes of Hindoos in their proper costume; the bodies are in terra cotta, and the dresses in real stuffs; many of them are executed with great perfection, although the Hindoos of Kishnagore, by whom they are done, have not practised that kind of work more than fifteen years. There are also a great variety of domestic utensils of the Hindoos calculated to throw great light on their habits and manners. The reporters, in conclusion, bestowed the highest praise on the persevering assiduity of M. Lamare-Picquot, and strongly recommend the formation of an Ethnographic Museum, similar to those existing at St. Petersburgh and various towns of Germany, for the preservation of all the objects of every nation, calculated to throw light on the manners and customs of any nation of the globe.

Société d'Encouragement des Arts et de l'Industrie.

Enamel Painting.—On the 1st of June M. Merinnée made a report on a new application of enamel painting, which promises to be of great importance to the arts. This branch of art has hitherto been confined to painting on enamelled metallic plates, or on porcelain: the objection to the former is that, in consequence of the action of the fire on their shape, they can never be used beyond a certain and small size; while the latter, though presenting the advantage of greater dimensions, has the inconvenience of not being susceptible of being passed above three times through the fire, because the enamel of the porcelain not having the same fusibility as the colours, the latter scale off when the action of the fire is prolonged beyond a certain point. The difficulty, therefore, was to find a substance which, while it afforded equal dimensions with the plates of porcelain, would support the action of the fire without breaking or losing its form. This want has been supplied by the discovery of the properties of the lava which is found in great quantities in the mountains of Puy-le-Dôme, and to which the distinguishing name of tephrine has been given: that procured from Volvie is the best. This lava is very porous, and consequently lighter than common stone. It is sawn into plates of moderate thickness (about half an inch); and when these plates have been cut perfectly even, the small cavities of the surface are stopped up with a vitrifiable paste, which, by the action of the fire, forms one substance with the lava, and subsequently unites itself firmly with the layer of enamel which is placed over it. Plates of three and
four feet long are thus prepared without much trouble or expense, and they may be made double the size. The blocks sometimes taken from the quarries have a superficies of ten feet square. The enamelled surface of this lava is not even, like the enamel of porcelain; but it is a little grained, which renders it particularly adapted for pictures on a large scale, as historical pictures, &c. If it were required to use this substance for miniature painting, the layer of enamel must be perfectly smooth; and though this would be difficult to effect, the reporter is of opinion that it would not be impossible. The Count de Chabrol, when prefect of the Seine, first employed this lava for the trottoirs, or foot-pavements, of the streets; and M. Mortelique, being induced from its fusibility, its vitreous qualities, and its porous consistence, to suppose that it was susceptible of being enamelled, made a variety of experiments, and ultimately, in 1827, exhibited a head, painted the natural size, on a plate of this lava, which was considered worthy of a prize. But in order to render this generally useful, it was requisite to make the enamel painting so nearly analogous to oil painting, that historical painters might acquire the art without material loss of time. The great difference was that, on the enamel, as on porcelain, the colour could only be applied by small touches in juxtaposition, and could only be degraded by letting the white ground appear more or less through the transparent tints. This mode of proceeding, which is that of miniature painters, is much too tedious for artists accustomed to lay the colour thickly on the canvass. M. Montelique has therefore applied himself to the discovery of a white which will combine itself with all the colours used in enamel painting, without decomposing them. In this he has fully succeeded, and by this discovery has removed the only difficulty existing in the use of the lava for paintings; so that pictures of the largest size may now be painted in enamel with the same facility as in oil; and with every facility of retouching the picture, when in progress, is combined the advantage of the colours being rendered capable of bidding defiance to the ravages of time, by the unlimited manner in which they may be passed through the fire. Had this discovery been made three centuries earlier, we should not have to deplore the deterioration of the ‘Last Supper’ of Leonardo da Vinci, and the ‘Descent from the Cross’ of Daniel di Volterra.

SOCIÉTÉ DE GÉOGRAPHIE DE PARIS.

Annual Prize.—This prize was proposed for the most important geographical discovery made during the year 1829. The committee, in their report, first mentioned, in terms of praise, Captain King’s attempt to explore part of Patagonia, but added, that as his voyage has not yet been published, no judgment can be formed of the importance of the results at which he has arrived. M. Parchappe, by his discoveries in South America, has thrown new light on the course of the Uraguay, and other rivers of the basin of Parana. This traveller, in the twelve years which he has passed in the province of
Buenos Ayres, and those watered by the Parana and Uruguay, has rectified some remarkable errors, particularly that which assigned to the Lake Ibera, from east to west, four times its real length. He has also ascertained, in a satisfactory manner, the course of a part of the rivers Colorado and Negro. The committee also speak favourably of the voyage of circumnavigation of the Russian ships Moller and Seniavin, commanded by Captains Starikowitch and Lütke. The latter, in particular, has discovered new islands in the archipelago of the Carolinas, particularly the island of Pounipet, inhabited by a race of blacks analogous to that which peoples the coast of New Guinea; whereas all the islands of the archipelago, previously known, are peopled by the copper-coloured race which forms the intermediate link between the Malays and the Polynesians properly so called. The prize, however, (consisting of a gold medal of five hundred francs,) is adjudged to Captain Graah, of the Danish navy, for his exploring-voyage along the eastern coast of Greenland, to which he penetrated by sea, and discovered a people who, from a remote age, have been deprived of all communication with Europe, and whose language was nearly unintelligible to the Greenland interpreters who accompanied him. They retained some vestiges of the Christian religion. The eastern coast of Greenland was previously very little known. Between Cape Farewell 59° 42' latitude, and Cape Barclay 69° lat., very few points were known; the coast was supposed to proceed in a north-easterly direction, but Captain Graah has ascertained that its direction is nearly north. Greenland was discovered about the year 982, by Eric Rauda, and the Norwegians, in the succeeding ages, sent missionaries there, but the colony appears to have entirely dropped into oblivion about the fifteenth century; and though it was vaguely said that a people differing from the Esquimaux in habits and physiognomy existed somewhere, it was reserved for Captain Graah to ascertain their existence with certainty, and make their situation known to Europe. Captain Graah's Journal will shortly be published, and will probably throw much valuable light on the real direction and position of the coast of Greenland.

Academy of Sciences of St. Petersburgh.

Meteorological Phenomena.—On the 9th of February last, a communication was made of a singular phenomenon, observed at Oremburg, on the 1st December. During the whole day a heavy rain fell, although the thermometer remained steady at freezing point: about midnight three loud claps of thunder were heard in a north-westerly direction; the next day there was a fall of snow, accompanied by a multitude of little gnats, the motions of which were similar to those of the flea. The day after the atmosphere cleared up, and the thermometer descended ten degrees below zero. At the same meeting a letter from the Governor of Oremburg was read, stating that on the 7th of January, between six and eight in the evening, the moon, which was nearly new, appeared surrounded
with a large and perfectly regular luminous circle, cut by two diameters equally luminous: the moon occupied the centre of the circle. Two white semicircles were distinctly traced at the extremities of the diameter, which cut the circle from east to west, and their light was reflected almost as far as the extremities of the other diameter which divided the circle with the same regularity from north to south. To the north of this circle was observed a luminous arch of small dimensions. During the whole time of this phenomenon being observed, the atmosphere was pure and tranquil, and the thermometer was not below seventeen degrees (Reaumur); a short time afterwards it fell to twenty-nine degrees below freezing point.

New Mineral.—In the month of August last, the Academy was presented with a new mineral found in some government-lands in the province of Perm. It has received the name of Volkonskoite, in honour of Prince Volkonsky. The spot in which the vein was found is in the mountain called Efmiatskai, in the district of Okhausk. The bed does not consist of regular veins, but in bits of from one to four verschocks thick, by a quarter to three-quarters of an archine long; sometimes ten of those bits or patches are found in the space of a single sogene, and sometimes there are three sagenes without a single one. The mineral, in colour, approaches the grass-green; it divides in longitudinal plates, and breaks on the slightest pressure. When plunged in water it separates with a loud noise into angular pieces, on which, when dried, the water no longer takes any effect. This mineral may be employed as a colouring matter to replace some of the most expensive colours, such as molochite and verdigris. The fine orange colour of chrome may also be chemically obtained from it, as it contains about seven per cent, of extract of chrome. It is easily worked and at a small expense.

Mineralogical Society of Russia.

Native Emeralds.—A very fine native emerald has lately been given to this society by the Emperor. Its form is a regular hexagonal prism; it is of a beautiful green colour: one of the planes, which usually terminate the extremities of these prisms, remains in its natural state. The other plane, or base of the prism, is covered by a gangue of micaceous schistus similar, as respects its composition and black colour, to that in which emeralds are found at Herbachthal, near Binsgau in Salzburg; but the crystals of the emeralds have never been found there of such large dimensions as those recently discovered in Siberia, of which the above-mentioned is a specimen. This new vein of emeralds in Siberia is situated eighty-five versits to the east of Catherineburg, and was discovered in the following manner. In January last, a peasant of the canton of Belosersk, in looking for stumps of trees to extract resin, found, among the roots of a tree which had been blown down, several fragments of emerald which he sold at Catherineburg. This led to further researches, and a most valuable vein has been discovered.
FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—MECHANICAL SCIENCE.

1. PARABOLIC RIDGES FORMED ON MOVING WATER.

M. Poncelet has made some curious observations on the form of the ridges produced when a body is placed in a stream of water flowing with uniform velocity. When a point is placed in the upper surface of water flowing with uniform velocity, a great number of ridges appear of a parabolical form. If a stream of water spout from an orifice in a vessel, these curves have their vertices in a line joining the point and the orifice. The summit of the first parabola is at the point itself, and is the limit of all the others. The number of ridges is indefinite; they are placed at distances which increase with their distance from the point. These ridges become less and less elevated according to their distance from the point, till they vanish altogether. The ridges are perfectly stationary and invariable in their figure, whilst the motion of the fluid remains the same, and they cease to exist the instant the point is removed. If the vein flow in a trough with vertical sides, the same phenomena take place as if it were not so confined; and the ridges are suddenly terminated by the sides without suffering in flexion or reflexion.

From these phenomena one might at first sight suppose that the molecules of the current deviate from their natural course and follow the branches of the curve. This, however, is not the case, as may be proved by throwing fine powder on the liquid vein: the particles of the powder cross these ridges, and follow the same course which they would take if the ridges did not exist.

In plunging several points into the vein at different distances, the same system of ridges is obtained for each point, and the curves cross each other at the points where they meet, without their form being in the least altered. When the velocity of the vein is below twenty-five centimetres (ten inches) per second, the ridges become imperceptible. They become more and more distinct as the velocity increases. The number of ridges also increase with an increase of velocity, the long branches approach more and more to their common axis. The author remarks, that we have here an accurate method of determining the velocity of a current by comparing the form of the exterior ridge with those given by experiment with a current whose velocity is known.

If the point be moved in a straight line along the surface of calm water, we have exactly the same parabolic ridges as we should have with water flowing with the same velocity as the moving point.*

* Annales de Chimie, xlvi. p. 5.
2. NEW THEORY OF CAPILLARY ACTION.

M. Poisson has published, as a paper, the first part of a work which will shortly appear, and in which he gives his views of capillary attraction. After viewing what had been done before, he arrives at the conclusion that the phenomena of capillarity are due to molecular action, modified not only by the curvature of the surfaces, as Laplace has said, but also by the particular state of the liquids at their extremities due to the deficiency on the exterior of that molecular attraction which exists in the interior *.

3. ON THE APPLICABLE FORCE EXER TED BY A HORSE.

M. D'Aubuisson has examined the useful force of a horse by reference to the effects produced at the Freyberg mines, where the ores are raised by this animal power. The horses belong to the neighbouring countrypeople, and are occupied for eight hours in the day; they are small for draught horses, but in excellent condition.

The power of a horse he distinguishes into useful (or applicable) effect, and dynamic effect: the latter being the total force exerted by the animal, and the former that force minus what is consumed by the resistance and friction of the machine, vis inertia, &c. &c. The useful effect is that which it was his object to estimate, and he found it to equal forty kilogrammes raised one metre (or 2.2lbs. raised 39.4 inches) in a second; this being understood of a good ordinary horse working for eight hours, in two portions of four hours each, and in machines of simple construction and properly arranged. From some experiments, &c., of M. Hachette, the dynamic effect would appear to be about sixty kilogrammes raised to the same height in the same time †.

4. BEVAN ON THE RELATIVE HARDNESS OF ROAD MATERIALS.

Mr. Bevan has sent to the Philosophical Magazine a table containing the results of experiments made in 1825, principally upon the hardness of road materials, or their power of resisting the percussion of a given weight of cast-iron falling a few inches upon the several specimens broken to the ordinary size, and resting upon stone or iron. Supposing the weather to have no action, the table would express nearly the relative value of the materials, for the purpose of supporting the wear of a road; and, therefore, those which resist the action of frost and weather, and have the highest numbers, are most valuable.

<table>
<thead>
<tr>
<th>Material</th>
<th>Points</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mount Sorrel sienite</td>
<td>100</td>
</tr>
<tr>
<td>White marble</td>
<td>37.31</td>
</tr>
<tr>
<td>Chert pebble, used much in Mid-</td>
<td>34.27 52.56.55.65</td>
</tr>
<tr>
<td>die sex</td>
<td>65</td>
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Quartz pebble in Bedfordshire gravel 70
Ferruginous sandstone of Bedfordshire 20. 42
Hurlock from lower chalk 10
Chalk 3
Granite, Scotch 110
Flint, yellow 33. 26
Greenstone or basalt, Quittle-Hill near Coventry 110
Sandstone, soft 13. 6
Tile fragment 20
Gritstone, near Brixworth, Northamptonshire 48. 60
Limestone, near Bradwall, Bucks. 5
Dry clay 12
Flint, black 11. 30
Portland stone, hard 14
Quartz, white 56
Blue pebble, like Rowley rag 105. 110
Coarse limestone, near Stilton, Huntingdonshire 60
Gritstone, on road near Leeds 100. 115
Yorkshire paving stone 20
Ketton, hard 20
Tetternhoe 4
Chert? from hills in Devon and Cornwall 57
Gray wether, Hertfordshire and Wiltshire 18
Grit of upper bed, Collymeston, near Stamford, Lincolnshire 40
Second bed do. 100
Slate at do. 50
Stockton limestone, Warwickshire (lias) 45
Newbold, on Avon, do. 36
Limestone of Stoke Cruerne, Northamptonshire 35

The steady pressure, without percussion, required to crush a piece of the marble weighing half an ounce, was 100 lbs.; to crush the grey flint of 1.2 oz. weight, 2000 lbs.; to crush the rolled white quartz pebble of 2 oz. weight, 3400 lbs. *

A specimen of the copper slag, recommended for roads by Mr. Fisher of Newgate-street, was sent by Mr. Taylor to Mr. Bevan, to be tried and compared with the above. Mr. Bevan reported upon it, that it was the hardest material he had met with, its number being 234, or above double the highest in the list. The specific gravity was 4.32. A substance of such hardness, not subject to decomposition by exposure to weather, and of moderate price, is considered by Mr. Bevan as a most valuable material for roads of great traffic and heavy loads †.

* Vol. ix., p. 164.
† Phil. Mag., N. S., ix., 317.
5. ON THE BUR OF PERFORATIONS.
(R. W. Fox, Esq.)

If any slender and sharp-pointed instrument (a common needle for instance) be made to revolve quickly whilst piercing a card, it produces an elevation or bur on each side of it. Hence, may it not be inferred that the same effect, caused by an electrical discharge, is due to the rotation of the electrical current or currents?—for the edges of a hole made by electricity seem to be too regularly and completely elevated on both sides of a card to be reasonably attributed to the simple action of opposite currents not in rotation:— and when several cards are thus perforated together, ought not the outermost cards, on the latter hypothesis, to have the burs on their inner surfaces, and very little, if any, externally?—but it is known that the elevations are generally equal on both sides of the cards: so that the simple mechanical fact above stated appears to strengthen the presumption in favour of the rotatory motion of electrical currents.

6. CONDENSATION OF MERCURY BY PRESSURE.

It having been deemed very interesting to employ the pressure of the water at great depths for ascertaining the condensation of mercury by pressure, the expedition commanded by Captain Kotzebue was furnished with an elaterometer expressly adapted to this purpose. It consisted of a wide thermometer tube, open at one end, and having attached to it at the other a bulb like that of a thermometer. The tube and bulb were filled with mercury, and a drop of oil poured over it. A scale was attached to the tube, whose divisions showed the thousandth parts of the whole volume of the bulb and tube filled with mercury. When the instrument was made to descend to great depths of the ocean, the greatest condensation which the mercury had undergone during the experiment became visible by the oil adhering to the inside of the tube, even after the mercury had returned to its former state of expansion. At the temperature of 19° C (66°.2 Fahr.), the mercury stood at the zero point of the scale. Let these degrees be \( T \), and those at the depth of the sea \( t \); the expansion, for one degree of the thermometer, of mercury \( m \); of glass \( n \); and the volume of mercury at the temperature \( T = V \): then the contraction of the mercury, on account of the temperature, will be \( V \), \( (T-t)(m-n) \), which is to be deduced from the whole condensation observed by the instrument. The following experiment was made in 21° 14' north latitude, and 196° 1' west of Greenwich, at the depth of 914.9 toises (5851 English feet). At the greatest depth the thermometer was at 2°.44 C., and the elaterometer marked 3°.1. The condensation was, therefore, 0.0031. Having \( T=19 \) and \( t=2.44 \), \( n=0.0000274 \), \( m=0.000185 \), we shall find the contraction of the mercury by
temperature \(= 0.0026 V\), and the compression by the weight of the water is consequently \(= 0.0005\). Assuming that the condensation is proportional to the pressure, we shall find the compression by the weight of the atmosphere \(= 0.0000027\), which is nearly three times as much as \(0.000001\), the value assigned by Oersted for this compression *.

Note. The great condensation of mercury following from this experiment makes it doubtful, in our opinion, whether some of the oil may not have insinuated itself between the glass and the mercury, and it would, perhaps, be desirable to supersede the use of the oil by some other contrivance.

7. Instrument for the Condensation of Water by the Pressure Exerted by Water at Great Depths of the Ocean.

(Invented by Professor Parrot of Dorpat.)

This instrument consists of a hollow glass cylinder terminating below in a hemisphere. The upper end is to be closed by a cover which is screwed on. Through this cover a tube, open at the top, passes, fixed in so tightly as to allow no air or water to pass. This tube, when inserted into the glass cylinder, descends nearly to the bottom, and, after a bend, reascends again nearly to the top, where it terminates in a horizontal tapering piece with a small opening at the end. The tube is filled with mercury up to the open point, and the cylinder with water. By a small opening in the cover, which is again closed when the screw of the cover is quite home, the small quantity of superfluous water which may be in the cylinder passes out while the cover is screwing on. It is clear that the pressure of the water in the ocean is exerted on the water in the cylinder entirely through the medium of the mercury in the tube, and that the condensation of the water is exactly measured by the quantity of mercury forced out of the tube through the small opening at its end. The tube is furnished with a scale, on which the proportion which the volume of the mercury wanting in the tube bears to the volume of water in the cylinder is read off †.

8. Comparison of the Prussian Weights and Measures with the New English Weights and Measures.

(By Professor Eytelwein.)

One English inch, at 62° Fahr. = 0.971140 Prussian inch at 61°1/4 Fahr.

An English pound avoirdupois, of 7000 grains, = 31.018012 Prussian loth, 32 of which are a Prussian pound.

An imperial gallon = 253.95383 Prussian cubic inches ‡.

* Petersburgh Transactions for 1830.
† Ibid.
‡ Berlin Acad., year 1827, Berlin, 1830.
§ II.—CHEMICAL SCIENCE.

1. On the Electro-Magnetic Effects of Metallic Plates having various Positions, Intervals, &c. &c.—(By M. Bigiou.)

M. Bigiou has made experiments to determine the relation of electro-magnetic effects, which take place when equal discs of zinc and copper are immersed, under different circumstances, in the same fluid. He obtained the following results:—i. Voltaic electricity is transmitted through a metallic plate, having its surface grooved or roughened with sand-paper, more easily than through a plate of the same metal with a polished surface *. ii. In placing one of the plates inclined to the other, the effect is diminished †. iii. The extent of the surface of copper, as M. Marianini has shown, has a greater influence over the electro-magnetic effect than that of the zinc. The author shows, however, that nothing would be gained in the construction of a voltaic battery, by making the zinc much smaller than the copper, as the greatest effect takes place when they are nearly of the same size. iv. The author remarks that the effect diminishes as the distance between the plates increases ‡. The most valuable part of his paper, is that in which he gives the relative deflecting forces of the same plates with different acid solutions. This was ascertained with the torsion galvanometer.

Water, with \( \frac{1}{30} \) of its bulk of sulph. acid ... 106° of torsion.
Do. \( \frac{4}{10} \) of muriatic acid ... ... 59° do.
Do. \( \frac{1}{4} \) of nitric acid ... ... 106° do.
Do. \( \frac{1}{8} \) of nitric acid, and \( \frac{1}{4} \) muriatic acid, 59° do.
Do. \( \frac{1}{8} \) of nitric, and \( \frac{1}{10} \) sulphuric acid 96° do.
Do. \( \frac{1}{2} \) of nitric, and \( \frac{1}{2} \) sulphuric acid 120° do.

From this it appears, that equal volumes of nitric and sulphuric acids produce the greatest electro-magnetic effects.

2. Hare’s Delicate Galvanometer.

Dr. Hare, of Philadelphia, has constructed a galvanometer, with a riband of tin-foil thirty-four feet long, with a slip of paper intervening, which he says is more sensitive than those made with copper-wire, covered with silk, when the copper-wire was eighty feet long. He finds pure mercury, obtained by precipitating the proto-nitrate by copper, is negative to copper and the other metals, whereas impure mercury is positive, unless the amalgam be formed with the

* According to the experiments of Mr. Leslie, the same thing takes place with heat.
† This is also the case with radiant heat.
‡ He does not seem to be aware of the law, experimentally proved by Mr. Ritchie, (Journal of the Royal Institution, No. 1, page 36,) that the effect diminishes inversely as the square root of the distance of the plates.
precious metals. This is a convenient mode of testing the purity of mercury. Dr. Hare observed, that when the poles of the excited galvanic magnet are brought into contact with the mercury, communicating with one pole of the calorimeter, the vertex of the magnet being in contact with the other pole, a gyratory or whirling motion may be observed in the mercury *. The effect is identical with the vortices of Davy, or the rotation of Faraday.

3. Powerful Electro-Magnet.—(By Professor Henry and Dr. Ten Eyck.)

In the last number of this Journal, page 609, we gave an account of a powerful electro-magnet, constructed in America, by Professor Henry and Dr. Ten Eyck, which was capable of sustaining about 750 lbs. These gentlemen have carried their researches still further, and have actually constructed an electro-magnet for Yale College, which is said to have sustained 2063 lbs., or nearly a ton! It was constructed on the same principle as the former, but a greater number of strands of copper wire were employed. The magnet is wound with 26 strands of copper bell-wire, covered with cotton thread, 31 feet long; about 18 inches of the ends are left projecting, so that only 28 feet of each actually surround the iron; the aggregate length of the coils is, therefore, 728 feet. Each strand is wound on a little less than an inch; in the middle of the horseshoe it forms three thicknesses of wire, and on the ends or near the poles, it is wound so as to form six thicknesses.* With a battery of 4½ square feet, the magnet suspended 2063 lbs. The effects of a larger battery were not tried. It induced magnetism in a piece of soft iron, so energetically, as to raise 155 lbs. When two batteries were employed so that the poles could be rapidly reversed, a curious fact was observed. After one of the batteries had been removed, the armature, with a weight added, in all 89 lbs., remained suspended, and did not fall when the poles were reversed. This effect must have been instantaneous, otherwise the weight must have fallen, as there was an instant when the magnet could have had no power. It was attempted to decompose water by this magnet, but without success †.

4. On Electricity Induced by the Red and Violet Rays of the Solar Spectrum.

Professor Saverio Barlocci, of Rome, states that when two pieces of copper, painted black, and one of them connected with the upper part of a frog, and the other with the hind feet, are placed one of them in the red, and the other in the violet ray of the solar spectrum, and then

* Silliman's Journal, xx., page 143. † Id. xx., page 201.
brought into contact, that contractions took place in the muscles of the frog *.

5. On the Identity of the Nervous and Electric Fluids.

The following experiment is from an inaugural thesis by Dr. David, of Paris:—' The sciatic nerve of a rabbit was insulated and laid bare, and carefully sponged; a piece of glass was gently introduced between the nerves and the muscles, while the leg of the animal was bent. The sensibility of the nerve was shown by the motions of the animal during the introduction of the needles, the one above the other, but not touching each other. They were placed in communication with the galvanometer: the animal was quite tranquil, and the needle of the instrument at rest. By a sudden movement of the rabbit, the apparatus was deranged, but the needle clearly deviated and moved. The needles were again introduced; some muscular contraction succeeded, again the needle oscillated, but so slightly, as not to convince the assistants. The animal, however, soon made some very vigorous and repeated exertions, and there was no longer any doubt of the fact, for the needle now described an arc of more than two lines. The oscillations ceased with the motions of the animal, and again appeared when it moved. The animal was excited to make contractile efforts, by stimulating the nostrils or irritating the nerve, and the needle immediately oscillated; and the arc it described was great in proportion to the energy of the muscular exertions which were provoked. The phenomena could, in fact, be caused at will. With four needles, double the effect could be produced than when two only were employed. In general, the intensity of the phenomena diminished with the vigour of the animal, and they were not observable after death. When two needles were placed in a nerve, and two in a muscle, the oscillations were barely perceptible; when all four were introduced into a muscle, M. David could obtain no deviation of the galvanometric needle.

Other experiments demonstrated why sometimes the phenomena may not arise when needles were placed in a nerve. The causes of the non-occurrence of the phenomena may be either, i. Insensibility of the nerve, from its being strained or pressed upon in sponging it. ii. Its too great tension over the glass placed beneath. iii. Blood may cover both the nerve and needles. iv. The perfect dryness of the nerve, produced by the sponge. It is then necessary to place the nerve for a moment in contact with the muscles, and its power is restored. It is highly important that the needles and the extremities of the threads of the galvanometer should be perfectly clean.

M. David considers these experiments sufficient, i. To prove that organized beings have a special apparatus, which is destined to

* Journal des Progrès des Sciences et Med., tom. ii., 1830
furnish an electric current; and ii. To show the circumstances which are required for its production.*

6. SINGULAR ELECTRICAL EFFECT.

'Whilst the workmen were soldering the iron water pipes in Water Street,' says the Winchester Republican, 'electric shocks were produced to such a degree as to cause them to discontinue their labour for the remainder of the day. Several of our citizens who were standing by, got into the ditch and tried the experiment, when the effect was the same on all. The pipes are united in the following manner:—They are nine feet long, perfect cylinders, with a bore of six inches, and a bowl at one end four inches deep; at the spring is a funnel pipe, which is inserted into the bowl of the succeeding pipe, the spigot end of which is inserted into the bowl of the next, and so on. When fifty or a hundred pipes are laid down in this manner the process of soldering commences. This is done by first ramming into the joint a few strands of rope yarn, and then applying clay around the joint, leaving an aperture at the top into which the molten lead is poured; the clay is then taken off, and the lead driven home with a blunt chisel and hammer. It was in driving home the lead that the shocks were produced. The sun was nearly vertical, and the thermometer at 93°, the ditch somewhat damp, and the pipes warm from the action of the sun upon them. The principle is no doubt that of galvanism; but as the cause is supposed to be entirely new, the plumber (Mr. Johnson, from Philadelphia) having never known anything like it during his long experience in that city, we should be glad to receive the opinion of scientific men upon it. We have since been informed, that after a heavy rain on the ensuing day, and the covering of a few feet of the pipes some distance above with earth, the phenomenon did not occur, nor has it since occurred.'

A correspondent in Silliman's Journal considers the effect as no doubt thermo-electric, and Professor Silliman agrees with him. The voltaic series of iron and lead is supposed to be rendered active by the intense heat of the sun, the black colour of the pipes causing them to rise to a higher degree than the neighbouring atmosphere, and the pipes being themselves unequally acted upon, from lying in a ditch †.

7. EXPLOSION OF PHOSPHORUS AND NITRIC ACID.

Dr. Hare had prepared some very strong nitric acid (having used above one half more of sulphuric acid than the equivalent proportion), which had a specific gravity above 1.5, and used it to illustrate the action upon phosphorus. A tube about seven-eighths of an inch in diameter, closed at one end, was placed within a stout

hollow glass cylinder of about three inches diameter, of which the glass was nearly three-eighths of an inch thick. The whole was situated about four feet in the rear of the table. About five grains of phosphorus, in two or three lumps, was thrown into about as much of the acid as occupied the tube an inch and a half in height. Very soon afterwards, there was a flash, followed by an explosion like that of gunpowder, and the fragments of the glass cylinder, as well as of the tube, were driven in all directions, so as to break many glass articles at the distance of from five to twenty feet, and to wound slightly some of the spectators.

On repeating the experiment on a smaller scale with the same acid, similar effects took place, so that Dr. Hare thinks it necessary to strengthen the ordinary precautions given relative to this action, since it may occasionally rise to such intensity as has just been described.

Professor Silliman adds some statements and remarks, and says, that if the acid be very strong, and especially if warm, the phosphorus burns with a splendid combustion; it is thrown about in jets of fire, and requires great caution. To render it the most beautiful, a tall, narrow, deep vessel should be used, but when the quantity of both substances is considerable, there is sometimes a dangerous explosion. This circumstance has happened so often in my own experience with nitric acid distilled from very pure nitre, and without any water in the receiver, that I cannot but repeat the caution, that the operator should be much on his guard. With a stick of phosphorus dropped into two or three ounces of strong nitrous acid, I have known an explosion like that of a swivel, and the fragments of glass wounded persons at a distance, although the experiment was performed out of doors, and the spectators, formed into a ring, were none of them nearer than fifty feet, and some who were hit were at double that distance.


From the result of numerous experiments made by M. Saladin, he has been induced to conclude that the phosphorescence of many bodies, as dried bones, rotten wood, &c., was caused by the presence of a small proportion of phosphorus evolved by the action of organic matter on the phosphates present, and combining with hydrogen in the nascent state. This action is compared to that upon sulphates in similar circumstances, and has led M. Saladin to several conclusions, and particularly to that which implies that phosphorescence (except in the case of certain insects) diminishes with the proportion of phosphates. Experiments are still wanting to prove this point; but whilst engaged in performing them, M. Saladin is still anxious to secure the credit of priority in taking this view of the effect.

* Silliman's Journal, xvi. 366.  † Journ. de Pharmacie, 1831, 212.
9. On Oxalic Acid.—(By M. Gay Lussac.)

Oxalic acid, when heated, is well known to be partly volatilized and partly decomposed into carbonic acid and a combustible gas. Wishing to know more particularly the nature of these gases, M. Gay Lussac heated some very pure crystals of the acid in a retort: at 209° F. they fused; at 230° water and elastic fluids were disengaged, and the latter increased as the water passed away; at 245°–266° the disengagement was very rapid, and continued until all the acid was decomposed*. The gas was a mixture of six volumes of carbonic acid and five volumes of carbonic oxide, the proportion being nearly the same during the whole decomposition.

This easy decomposition was unexpected; for oxalic acid is considered as a stable compound among vegetable acids. The utility of sulphuric acid in the ordinary mode of decomposition was therefore suspected, and it was found, in fact, that when mixed with that acid, still a temperature of 230°–240° was required. But there is this important difference, that in the latter case the gas evolved consists of equal volumes of carbonic acid and carbonic oxide.

This difference led to the suspicion that some other substance was formed during the decomposition. On examination, it was found that the water which passed over was acid, and that it was so from the presence of formic acid. At first this acid appears in small quantity, but it comes over more and more concentrated, and towards the end of the operation the product has a very penetrating odour and sharp taste. Supposing that the sixth or missing volume of carbonic oxide with water forms this acid, then twelve proportions of oxalic acid should form one of formic acid, and this appeared to be the case.

It is evident that the hydrogen has been given to the formic acid by the water, and not by the oxalic acid, for then the carbonic acid and oxide should have been in equal volumes; besides, it is a necessary consequence of the experiments of MM. Dulong and Dobereiner. If the decomposition is not carried on too rapidly, it is total, no oxalic acid being volatilized.

These effects render it more imperative, M. Gay Lussac thinks, that oxalic acid should not be separated from the two other compounds of carbon and oxygen, i.e. carbonic acid and carbonic oxide. It may be ranged amongst those acids into which two equivalents of the radical enter, and the name which may be applied to it is hypocarbonic acid; but M. Gay Lussac does not press the adoption of this name at present†.

10. Dr. Turner on the Volatility of Oxalic Acid.

Dr. Turner having lately examined the volatility of oxalic acid, finds

* See next page. † Annales de Chimie, xlvi. 218.
the substance to rise at temperature so low as 212°,* without under-
going any chemical change, except that the common crystals lose
two-thirds or two equivalents of their water of crystallization. If
ordinary oxalic acid be placed in a water bath, and heated, it efflo-
resces, losing nearly the proportion of water mentioned; if exposed
to the cold air, it recovers the water; but if continued hot, it sub-
limes, and minute acicular crystals form on the surface. If purified
oxalic acid in crystals be exposed to 350° or 400°, in a deep evap-
orating basin, and, when sublimation begins, the vessel be covered by
a layer of smooth filtering paper, a fold of blotting paper, and a
larger evaporating basin containing cold water, the oxalic acid con-
denses in crystals on the filtering paper, or falls on the side of the
dish, and after an hour may be removed, and quickly secured in a
stoppered bottle. Thus sublimed, the acid is in minute shining
acicular crystals, which, on exposure to air, become dull, and regain
the two equivalents of water.

At higher temperatures the sublimation proceeds more rapidly.
At 300° or 330°, none is decomposed †. At 360° or 400°, the subli-
mation is very free; at 414° it fuses, and boils freely; above 330°,
decomposition to a greater or smaller extent occurs, and is indicated
by the appearance of water. By combining the sublimed acid with
bases, &c. &c., its unchanged nature was ascertained.

Dr. Turner found that a saturated solution of oxalic acid at the
temperature of 50°, contained 1 part crystallized acid, and 15.5
parts water. At 57°, 9.5 parts of water dissolved 1 part crystal-
lized acid. At 212°, the quantity of acid dissolved is almost unli-
mitied; at 220° the crystals fuse in the water of crystallization ‡.


You inquire if we have ever tried whether gunpowder will fire in
the sparks from our polishing wheels.§ We have tried the experi-
ment, and find that when coarse emery is used on the wheels it will
be fired at any distance to which the sparks extend; but when very
fine emery is used, a stream of innumerable sparks may be poured
upon coarse gunpowder without inflaming it. The same powder,
however, on being finely pulverized, will be readily inflamed by the
sparks from the fire wheel. In both cases, the sparks are particles
of ignited iron, and there can be no difference in the two cases.

* It sublimes at common temperatures. See pp. 73, 74, of the last volume of this
Journal.
† See preceding page.
‡ Phil. Mag., N. S. ix. 161.
§ The polishing wheels referred to are of various sizes and kinds, from large
grindstones, on which the gun-barrels are ground, to small wheels covered with
oiled leather and armed with emery powder. All these wheels are moved with
great rapidity by strong water power, and when the steel articles are held upon
them, there is a splendid coruscation of innumerable sparks flying off in tangent
lines, which follow one another with such rapidity, that the wheel is constantly
surrounded with a glory.
except in the magnitude of the particles. It would seem, therefore, that within certain limits gunpowder will not be inflamed by particles of ignited iron, unless they have at least a certain magnitude in relation to the magnitude of the grains of the powder. Your question was probably suggested by the fact, well known to you, that on putting the hand into the stream of sparks, the sensation experienced is rather that of cold than of heat. This is a fact which not a little surprises those of our numerous visitors who have the courage to present their hands to a stream of fire so dense as to have the appearance of one continued flame. The paradox, I apprehend, may be explained in the following manner.

The particles which make up the stream are much smaller in dimensions and fewer in number than they appear to be, each particle, from the extreme rapidity of its motion, appearing to extend several inches, when, in fact, it is little more than a mere point. These particles, being thus minute, do not impart a sufficient quantity of heat to penetrate through the insensible external membrane of the skin, called the cuticle or epidermis, so as to reach the adjacent membrane which alone is the organ of sensation, before it is again withdrawn by the increase of evaporation produced by the current of air which the wheel puts in motion. If the hand is held steadily in the stream until the evaporation is diminished by the gradual desiccation of the skin, we shall perceive a mild sensation of heat. These sensations, first of cold only, and afterwards of mild heat, take place only when we present to the stream the inside of the hand or fingers, where the cuticle is thick. If the back of the hand be presented, a very pungent and pricking sensation of heat is produced at every point where a particle impinges, highly contrasted, at the same time, with a general sensation of cold, produced by the increased evaporation. In the first case, the heat is passing through the thick cuticle of the inside of the hand, extends laterally, and loses its intensity before it reaches the sensible membrane; but the cuticle on the back of the hand being extremely thin is immediately penetrated *.

12. NEW PYROPHORUS.

Dr. Hare recommends as a new pyrophorus, Prussian blue heated to redness, for about a minute, in a glass tube, and then sealed up hermetically. As soon as the tube is fractured, and the contents thrown out upon a table, they take fire †.

13. CRYSTALLIZATION OF OXIDES OF IRON AND ZINC.

(By M. Haldat.)

M. Haldat is in the habit of using a bundle of soft iron wire in his demonstration of the action of steam at high temperatures on iron,

* Silliman's Journ., xvii. p. 114. † Ibid., xix. 173.
and by management has obtained larger crystals of oxide of iron than are usually procured. These crystals are finer the longer the action of the iron and steam has been continued; they have occasionally been obtained one-tenth of an inch in size upon large wire or plates of iron. The crystals are very brilliant, and, under the microscope, perfectly resemble those from Elba or Framont. They are usually rhomboids, covering each other as in the iron ores from those countries, have the same brilliancy and colours, and every other character except size.

M. Haldat then endeavoured to obtain crystallized oxide of zinc by similar means; and by using precautions relative to the application of heat, rendered necessary by the greater fusibility of zinc, succeeded in his object. The oxide had two forms, being sometimes in amorphous globules, and at others in plates covered with rhomboidal transparent crystals, having the colour of honey.

As in volcanoes, most of the circumstances meet necessarily in these experiments to produce crystals, it is probable that all the varieties of crystallized oxide of iron there found, result from a process analogous to the present.*

Mr. Daniell obtained very fine octohedral crystals of oxide of iron upon the bars of iron, used in his pyrometrical experiments, and which had been heated with very imperfect access of air †.

14. DISCOVERY OF VANADIUM IN SCOTLAND.

Mr. James F. W. Johnston has discovered Vanadium in Scotland, in a mineral from Wanlockhead, resembling in appearance an arseniate of lead; and it is a remarkable circumstance, that this new substance has been discovered by three different persons—Professor Del Rio, Professor Sefstrom, and Mr. Johnston—in three different countries, Mexico, Sweden, and Scotland, nearly at the same time, and without any knowledge, on the part of one, of what the others had done.

Mr. Johnston discovered it during the last winter in two minerals, very different in character, but both compounds of lead, probably Vanadates. The first resembles an arseniate, occurs in small mamillae upon the surface of calamine, sometimes passing into a crystalline form. The second mineral can hardly be distinguished from earthy porous peroxide of manganese. It occurs amorphous and in small rounded forms, often powdering the calamine with a thin black coating, and at times scattered in cavities‡.

15. YELLOW DYE FROM SULPHURET OF CADMIUM.

M. Lassaigne proposes to use this substance as a yellow dye on silk. If the silk be immersed in a solution of chloride of cadmium for fif-

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* Annales de Chimie, xlv. 70.
† Phil. Transactions.
teen or twenty minutes, at temperatures between 122° and 140° F.,
then wrung out and immersed at common temperatures, in a weak
solution of hydrosulphuret of potassa, it becomes of a fine gold
yellow colour, or of lighter or deeper tints, according to the strength
of the cadmium solution. This dye is unaltered by sun-light, or
weak acid, or alkaline solutions. Wool could not be dyed by means
of this substance with the same facility as silk.


The separation of these metals is very difficult, because of the simi-
lariry existing between them. M. Gay Lussac has for this purpose
long successfully used tin as a precipitant of the antimony. The
two metals, their weight being known, are supposed to be in solu-
tion in muriatic acid. If alloyed, they would be dissolved in muri-
atic acid to which small quantities of nitric acid had been succes-
vively added. A plate of tin is to be immersed; and there being
muriatic acid in excess, the antimony will soon deposit as a black
powder. The effect will not be perfect at common temperatures;
but by applying the heat of a vapour-bath, it will quickly be con-
cluded, provided that excess of acid is continued in the liquor. The
antimony is then to be well washed and dried on the water-bath.
If the two metals are in solution, their weight not being known,
one portion should be precipitated by zinc, to give the whole of
both metals, and another portion by tin, to give the proportion of
antimony.

17. Compound of Bi-cyanide of Mercury and Iodide of Potas-
sium. (By Dr. Apjohn.)

When solutions of iodide of potassium and bi-cyanide of mercury
are mingled and left, a beautiful pearly substance, in very thin four-
sided prisms, is formed, very soluble in hot water, but scarcely
affected by water at 60°. These undergo no change by ammonia,
potash, or the carbonated alkalies in solution: muriatic acid renders
them bright scarlet, and evolves the odour of prussic acid. When
ignited in a crucible, the residue gave an abundant precipitate with
tartaric acid. These and other experiments proved that the com-
pound contained both the salts added in solution, and upon analysis
they were found in the following proportions:

<table>
<thead>
<tr>
<th>Bi-cyanide of mercury</th>
<th>24.16</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iodide of potassium</td>
<td>15.47</td>
</tr>
</tbody>
</table>

And as these numbers are very nearly in the ratio of the equivalent
numbers, it appears that a proportional of each proximate element
is present. That the substances are not present, as bi-prussiate
and hydriodate, is shown by the correspondence of the absolute
weights with the estimate above given, and by the fact that, when

* Annales de Chimie, xlv., 433. † Ibid., xlvi., 222.
heated in a tube, no water is evolved. Dr. Apjohn proposes to call the substance iodo-bicyanide of potassium and mercury*.

18. Composition of Tartaric Acid.

By differences between his own estimate and that of Prout, Berzelius has been induced to re-examine the composition and number of tartaric acid. He finds, from many experiments, that the tartrate of lead is composed of

<table>
<thead>
<tr>
<th>Substance</th>
<th>Atomic Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tartaric acid</td>
<td>37.2569</td>
</tr>
<tr>
<td>Oxide of lead</td>
<td>62.7431</td>
</tr>
</tbody>
</table>

Its atomic weight, therefore, is 828.05; hydrogen being 12.5, and oxide of lead 1394.5.

Tartaric acid is composed of

<table>
<thead>
<tr>
<th>Element</th>
<th>Atomic Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydrogen</td>
<td>3.0045</td>
</tr>
<tr>
<td>Carbon</td>
<td>36.8060</td>
</tr>
<tr>
<td>Oxygen</td>
<td>60.1895</td>
</tr>
</tbody>
</table>

100

which agrees with 5 atoms oxygen, 4 carbon, and 4 hydrogen (on the assumption that water contains 2 atoms of hydrogen). Calculated in this way, the atomic weight should be 830.709, instead of 828.05, as above. Prout made it 830.709 by his analysis.

Berzelius then examined the equivalent number of lead and its oxide very rigorously; and for this purpose prepared an extremely pure oxide of lead, and reduced it by hydrogen at a sufficiently high temperature. From the mean of six experiments, the results were, per cent.—

<table>
<thead>
<tr>
<th>Element</th>
<th>Atomic Weight</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lead</td>
<td>92.8277</td>
</tr>
<tr>
<td>Oxygen</td>
<td>7.1723</td>
</tr>
</tbody>
</table>

And the atomic weight of lead 1294.29, instead of 1394.5, as above; hydrogen being 12.5.

All the six results were between 1293 and 1296. 'But if the atom of hydrogen weighs 12.5, the atomic weight of lead, to be a multiple of that number, should be either 1287.5 or 1300; and if one of these numbers is the true one, my results ought to oscillate about it, instead of oscillating, as actually happened, between 1293 and 1296.' Other considerations, drawn from the analysis of the tartrate of lead, are by M. Berzelius considered as opposed to the theory of multiple numbers of hydrogen †.

19. Racemic Acid or Para-tartaric Acid.

The following account of this acid is abstracted from Berzelius. A manufacturer of tartaric acid at Thann, on the Upper Rhine, first observed that with the ordinary acid appeared crystals of another

* Phil. Mag., N.S., ix., 401. † Annales de Chimie, xlvi.
and less soluble acid. He mistook it for, and sold it as, oxalic acid. In 1819, Jahn described it in his Dictionary of Chemistry, and distinguished it from both tartaric and oxalic acid. In 1826, Gay Lussac showed that it was not tartaric acid, though its equivalent number was almost the same. Some time after, Walchener also experimented on this acid.

The acid is supposed to be peculiar to the grape of the Upper Rhine; but is probably present in all grapes. It may be obtained by exactly saturating tartar containing racemic acid, with carbonate of soda, and crystallizing most of the double salt formed. The tartrate separates first, the far more soluble racemate remaining dissolved; when it crystallizes, its forms are different to those of the tartrate. The mother liquor is to be evaporated, precipitated by a salt of lead or lime; and the separated precipitate decomposed by sulphuric acid. Racemic acid first precipitates in crystals from the acid solution, and then tartaric acid. Racemic acid requires 5 parts of water for its solution; tartaric acid only 2 parts.

The equivalent number was deduced from the racemate of lead. This compound is much more soluble in excess of acid than tartrate of lead, and usually covers the sides of the glass with a thin crystalline crust. The precipitated salt does not contain water, but the crystallized salt does. The results of analysis were exactly the same, both for the atomic proportion and the ultimate composition, as for tartaric acid, (for which see the preceding page;) so that this substance furnishes a new example of the extraordinary fact, that bodies composed of the same number of simple atoms may, nevertheless, possess different properties.

Racemic acid has a different crystalline form from tartaric acid, and also effloresces by heat, which tartaric acid does not. Pulverised racemic acid, exposed to dry air at 64° F., for twenty-four hours, was then dried in a current of air at 212° F., and lost 10.63 per cent. of water. This loss did not increase, and it was found necessary to use oxide of lead as a base, and apply heat above 212°, before all the water was expelled. The loss ultimately amounted to 21.35 per cent., or two proportions of water. Of these, one may be disengaged by heat alone, and the other by heat and a base. As tartaric acid also contains two proportions of water, the difference in the characters of the crystals is not due to that substance.

With potash, an acid racemate may be formed equally insoluble with cream of tartar, and containing, like it, an atom of water of crystallization. The crystals appeared to differ in form from those of cream of tartar. The racemate of potash and soda, if it exist at all, does not resemble Rochelle salts: a confused mass only is left, and it is uncertain whether it is a double salt or a mixture of salts. A racemate of potash and antimony may be formed analogous to tartar emetic, but its crystals differ, being sometimes rhomboids, sometimes rhomboidal prisms. As two tartrates of potassa and antimony can be formed, one crystallizable, the other not, so also
two similar racemates exist, but both are crystalline, the second occurring in acicular crystals, which in sunshine become as white as milk. The same change happens to the corresponding gummy tartrate, in which also minute crystals may occasionally be seen.

The racemate of lime is much less soluble than the tartrate; both have the same composition, and both contain 4 atoms of water in combination. The tartrate contains 21.765 per cent. of lime, and the dried racemate gave 21.775 per cent. of lime. A solution of sulphate of lime in water is decomposed by the addition of a little racemic acid; and after twenty-four hours, most of the lime is found precipitated as racemate of lime. Tartaric acid does not do this. If two solutions be prepared in muriatic acid, a little diluted,—one of racemate of lime, and the other of tartrate—and then each be saturated with ammonia, the racemate quickly falls as a semi-crystalline, white, opaque powder; but the tartrate does not, unless the liquid be much concentrated. After some time, octohedral crystals form upon the glass. This is a good method of distinguishing the acid, when one of them is in solution. A solution of racemate of lime, evaporated spontaneously, yields crystallized racemic acid; but evaporated by heat, the muriatic acid is volatilized, and the addition of water dissolves no racemate*.

20. On Gallic Acid.—(By M. Braconnot.)

M. Braconnot had recommended the preparation of gallic acid by a process in which the tannin present in the infusion of galls was removed by gelatine; but M. Berzelius thought that such gallic acid was chemically combined with tannin, and that pure gallic acid could be obtained only by sublimation. M. Braconnot has therefore made further experiments, and finds that the two substances differ, and that, not from the presence of tannin in the unsublimed acid. The latter he calls pure gallic acid, and the other pyrogallic acid.

When very white gallic acid, giving no indication of tannin to gelatine, was moderately heated, it became a brown liquid, which crystallized on cooling, and which, dissolved in water, contained still gallic acid and a brown substance which precipitated gelatine abundantly. Thirty parts of dry white gallic acid, being subjected to a higher heat, gave only 3 1/2 parts of sublimed gallic acid: though very white, its solution precipitated gelatine. The residue, when dissolved, gave a brown liquor, which became much deeper with persulphate of iron, and blue-black with protosulphate (these being characters of pyrogallic acid, and not of gallic acid); and it also abundantly precipitated gelatine. Hence, heat appears to rearrange the elements of gallic acid, so as to produce a peculiar variety of tannin and pyro-gallic acid.

Pyrogallic acid reddens litmus paper, though less than gallic acid;

* Annales de Chimie, xlvii., 128.
it has a cool, bitter taste. It dissolves in $\frac{3}{2}$ parts of water, at $55^\circ F.$, whilst gallic acid requires 100 parts at the same temperature. When re-sublimed, pyrogallic acid is decomposed almost entirely, producing tannin and charcoal. It dissolves in ether. Its aqueous solution is colourless, but by exposure to air becomes coloured, and deposits ulmin, being entirely decomposed in a few days if water be added as evaporation proceeds. Persulphate of iron added to it is immediately reduced to protosulphate, and a tanning matter is formed. Protosulphate of iron produces a blue-black liquor. These actions are very different from those of gallic acid, which with protosulphates of iron produce no change, and with persulphates produce a fine blue colour.

Pyrogallic acid slightly heated with strong sulphuric acid does not become coloured, and is not sensibly decomposed. Pure gallic acid treated in a similar way became coloured, but on adding water was found not much altered; no tannin was produced. Strong sulphuric acid and a higher temperature converted the gallic acid into ulmin; no tannin was formed.

Pyrogallate of alumina forms a bitter solution, becoming turbid by heat, and transparent when cold; it powerfully coagulates gelatine; crystallizes; and reddens litmus paper more powerfully than the acid alone; as if alumine itself acted as an acid.

Every endeavour to form gallic acid (upon Berzelius' views), by combining pyrogallic acid and tannin, failed. From all these facts, M. Braconnot concludes, i. That gallic acid procured in the humid way, and cleansed by animal charcoal, is pure; ii. That heat converts it into tannin and pyrogallic acid; iii. That gallic acid cannot be produced from tannin and pyrogallic acid.*

21. SULPHO-SINAPISINE, OR SULPHO-SINAPIC ACID.

MM. Henry and Garot described some years since a curious product derived from white mustard-seed (sinapis alba), which they called sulpho-sinapic acid, to point out at once its source, the presence of sulphur in it, and its acid nature. Some discussion has arisen relative to the subject, M. Pelouze having denied the existence of this acid, and referred the properties observed to the presence of sulpho-cyanic acid or its compounds. This has drawn forth a memoir from the discoverers and from others, in which many curious points relative to this body are established. We do not intend to enter into the controversy, but wish to give an account of the points really established, or, at least, most recently ascertained. The following account is from MM. Henry and Garot's second memoir.

Sulpho-sinapine is prepared by boiling the coarse powder of white mustard-seed, or of the turritæ glabra, for a minute, with five or six times its weight of distilled water, in a copper vessel, the liquid passed through a cloth, and the solid portion pressed. The magma, when exposed to the air, evolves a sulphurous smell. The

* Annales de Chimie, xlvi., 206.
yellow tinted liquid is slightly acid; it is to be quickly evaporated in a tin vessel by a water-bath, until like honey, when it will appear as a yellow, bitterish mass, having an odour like that of osmazome. Six or eight times its weight of strong alcohol is immediately to be added, and a tincture will be obtained yellow, slightly acid, and producing a strong red colour with persalts of iron. This tincture being distilled, the product has no power of affecting persalts of iron. The red-brown, syrupy, transparent matter left in the retort, crystallizes by slow cooling, or becomes a granulated mass if agitated: being allowed to drain on a cloth, then pressed, then purified two or three times in very strong alcohol, its bulk increases much, and it ultimately appears as a white crystalline substance, and is sulpho-sinapisine. Further portions may be obtained by evaporating the mother liquor: the crystalline masses obtained require to be digested in ether to separate a volatile red body soluble in that fluid.

Sulpho-sinapisine is white, inodorous, bitter, very light, soluble in water and alcohol, and more so when hot than when cold. When hot saturated solutions are cooled, acicular crystals in rounded groups are produced; they are most distinctly formed in acidulated water. When heated, it fuses, and is decomposed, yielding fetid products, amongst which are carbonate and hydro-sulphuret of ammonia. The substance is not naturally acid, but yields an acid by various modes of treatment.

Being analysed, its ultimate composition appeared to be per cent.

<table>
<thead>
<tr>
<th>Element</th>
<th>% by Weight</th>
<th>% by Theory</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>50.504</td>
<td>50.504</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>7.795</td>
<td>7.795</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>4.940</td>
<td>5.020</td>
</tr>
<tr>
<td>Sulphur</td>
<td>9.657</td>
<td>9.657</td>
</tr>
<tr>
<td>Oxygen</td>
<td>27.104</td>
<td>27.024</td>
</tr>
</tbody>
</table>

As the nitrogen and sulphur are nearly in the proportions belonging to sulpho-cyanogen, we may consider sulpho-sinapisine as represented by sulpho-cyanogen, and an organic matter not azoted, which may perhaps be competent to form the volatile oil of mustard.

**Sulpho-Sinapisine**, with nitric acid, is instantly altered; a bright red colour is produced, much orange-coloured gas formed, and sulphuric acid appears. With muriatic acid it is dissolved, and becomes green; heat then produces a strong odour of prussic acid. Distilled with sulphuric or phosphoric acid, sulpho-cyanic acid is obtained, and, with the first, sulphuretted hydrogen also. With chlorine, prussic acid and sulphuric acid were formed.

When treated with alkalies the following were the principal results. Ammonia dissolved it, but effected little change. Potassa and soda, with a little water, changed the colour to red and green; being evaporated and dried, much volatile oil of mustard rose. Then the substance fused, and was found to be sulpho-cyanide of potassium; further heat destroyed this state of things, leaving
charcoal and sulphates. Lime and baryta produced similar effect, for heat enabled the mixture to produce volatile oil of mustard and sulpho-cyanogen.

As to the action of salts, persalts of iron are strongly reddened by sulpho-sinapisine. The persulphate of copper is precipitated white; the proto-nitrate of mercury is precipitated white; the acid nitrate of silver is precipitated as a dense white substance, the weight of which, as sulpho-cyanide, is equivalent nearly to the sulpho-cyanogen which the substance could form. From all these circumstances it appears that sulpho-sinapisine is a curious body, not acid, containing sulphur, and containing no sulpho-cyanogen, but competent to form it by various modes of treatment.*

In an after Number † is a highly interesting memoir on the same subject by MM. Boutron and Robiquet. Suspecting that, in consequence of the presence of water, certain substances were formed during the process of extraction, which did not pre-exist in the mustard-seed, they devised various processes dependent upon the action of ether and strong alcohol, (and which involved much trouble) to obtain the substances in the seed in a state as near their original condition as possible, gathering up as they proceeded numerous observations on the changes, &c. &c. of the substances during the successive actions. By the action of ether, in the first place, they discovered an acrid fixed principle, which had not before been observed. On carrying on the operations they obtained the substance corresponding to the sulpho-sinapisine already described; but it had a very different composition to that given. In the following columns, the second gives the proportions of the new sulpho-sinapisine, the third that of the substance before described, the numbers being corrected by M. Pelouze.

<table>
<thead>
<tr>
<th></th>
<th>Carbon</th>
<th></th>
<th>Hydrogen</th>
<th></th>
<th>Nitrogen</th>
<th></th>
<th>Sulphur</th>
<th></th>
<th>Oxygen</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>54.0000</td>
<td></td>
<td>10.6512</td>
<td></td>
<td>2.8392</td>
<td></td>
<td>9.3670</td>
<td></td>
<td>23.1426</td>
<td></td>
</tr>
<tr>
<td></td>
<td>57.920</td>
<td></td>
<td>7.795</td>
<td></td>
<td>4.940</td>
<td></td>
<td>9.657</td>
<td></td>
<td>19.688</td>
<td></td>
</tr>
</tbody>
</table>

From these and other anomalies they conclude that it is impossible to state any thing correctly respecting the true nature of organic substances, and also that we cannot be certain of their pre-existence, until they have been obtained identical in their nature by several different processes.

On mixing black mustard powder with its weight of pure water, or with water containing a little acetic or sulphuric acid, or sub-carbonate of potassa, the two first mixtures evolved their well known powerful odour, but the two latter were inodorous. In the two latter cases the effect is due probably to some kind of combination.

The conclusions at which these authors arrive are, i. That the chemical composition of the seeds of white and black mustard is essentially different. ii. That the active principle of white mustard

* Journal de Pharmacie, 1831, 1.
† Ibid. p. 279.
is a substance not volatile, not pre-existing in the seed, and which may be due to sinapisine, combined with some other product; for when once the latter is separated, the former is not produced. Both contain sulphur. iii. The active principle of black pepper is a volatile oil, having no pre-existence, and not capable of development without the contact of water. iv. There is probably a principle in this seed from whence the sulphur found in the volatile oil is derived. v. Sinapisine, extracted by alcohol, without the intervening action of water, has not the property of reddening the persalts of iron, nor of evolving odour by the action of caustic alkalies. It is less soluble in alcohol, and contains less nitrogen than the sulpho-sinapisine of M. Henry and Garot, but it contains sulphur essentially.

In the same Number is a memoir by M. Fauré on the same subject, in which he also shows the important part performed by water, in contributing to the formation of those substances which characterise black pepper, and make it valuable in medicine. In every pharmaceutical preparation into which mustard powder enters, he considers it essential that as much taste and odour should be developed as possible, and that the powder should first be mingled with water, and then the other substances added. Acids and alkalies do not add to its irritating effects, except perhaps by those which they themselves possess. He arrives at the following chemical conclusions:—i. The volatile oil of mustard does not pre-exist in the seeds or powder; water is indispensable to its formation. ii. Besides the well known principles in black mustard, it appears to contain a particular green substance, which appears to aid in forming the volatile oil. iii. Sulpho-sinapisine is one of the principles of black mustard, and accompanies the green matter in almost every operation to which the seed is subject. iv. Ether has no marked action on the constituent elements of the volatile oil. v. Rectified alcohol, and weak acids and alkalies, when added to mustard powder, oppose the formation of the volatile oil.

22. Salicine.

MM. Herberger and Buchner have found salicine to be a sub-salt, containing a true vegeto-alkali. The saliccia, or base, has all the properties of those bodies, except that it dissolves with facility in water. When burnt it left no residue. It differs from salicine by its alkaline action on litmus paper; by its crystals, which are nevertheless prismatic; by its solubility in water, and by its action on concentrated and dilute acid. It is not so soluble in alcohol as salicine. The sulphate, nitrate, phosphate, acetate, tartrate, oxalate, and muriate of salicia, have been made; ether does not dissolve them, alcohol does, and upon evaporation leaves them (if there be not enough water of crystallization) in a pulverulent or flocculent form. When heated the salts fuse; then become dry; and when more highly heated, fuse and are decomposed with the odour of quinia in

* Journal de Pharmacie, 1831, p. 299.
combustion, leaving a bulky charcoal, which ultimately burns away.

The saturating power of salicia is very small.

All the salts, except the acetate, crystallize in needles, frequently representing vegetation. The acetate assumes the granular state. Many of them effloresce; their taste is generally bitter.

The salicia was obtained by dissolving salicine in a solution of oxalic acid; this acid was then separated by lime, &c. &c.

The acid part of ordinary salicina was then separated by treating it with phosphoric acid at a moderate temperature. This acid body is volatile, and may be obtained by distillation. It has all the properties of a sub-acid, and is the cause of the aroma of salicina, for pure salicina has no odour of the kind. Pure salicina has no advantages over ordinary salicina*.

23. On Dragonine.

M. Melandri announced the existence of a vegeto-alkali in dragon's blood, which he called draconine. M. Herberger has obtained this substance in a state of purity, and states that it has no claim to be thus ranked, but that it possesses the properties of a sub-acid, and should be classed with tannin, &c. &c.

**Dragon's blood consists of fatty matter. . . . . . . . . . . . . . . . . . . . 2.0**

*oxalate of lime... 1.6

phosphate of lime. 3.7

benzoic acid..... 3.0

draconine......... 90.7†

24. Precipitation of Morphia from Laudanum. Dr. Hare.

I believe it is not generally known that the addition of ammoniated alcohol to common laudanum will cause a crystalline precipitate of morphia in the course of a few hours. If the precipitate thus obtained be dissolved in acetic acid, again precipitated by ammonia, and afterwards collected and dried upon a filter, the morphia will be obtained nearly white, and may be rendered perfectly so by repeating the solution by acetic acid and precipitation by ammonia. I have by these means obtained thirty grains of morphia from an ounce of opium.

Instead of alcohol impregnated with ammonical gas, a mixture in equal parts of strong aqua ammonia and common alcohol will answer.

Narcotin is, I find, sometimes spontaneously precipitated in a crystalline form, from a solution of opium in proof spirit. The circumstances under which I procured it are nearly these:—a quarter of a pound of opium was boiled in a quart of proof spirit, and strained, while warm, through a coarse cotton cloth; the solution thus obtained being allowed to stand for about twenty-four hours, crystals were observed to be spontaneously deposited on the sides of the containing glass jar. These being dissolved in acetic acid, on the ad-

* Journ. de Pharmacie, 1831, 225.
† Ibid.
dition of ammonia, a precipitate took place, which was collected by a filter and dried. Narcotin was thus obtained in the form of white and beautiful silky crystals, which were readily soluble in sulphuric ether.

‘When we consider how often opium has been dissolved in proof spirits by chemists and pharmacists, it is surprising that crystalline principles, so easily evolved, as are morphia and narcotin, by the process above described, should have escaped observation until lately, when Sertuerner, by a much less obvious route, had the honour of discovering them.’

25. COLOURATION OF AZOTED BODIES BY NITRATES OF MERCURY.

M. Lebaillif remarked that when certain vegetable and animal substances were moistened with a solution of mercury in nitric acid, they became of a red or amethystine colour, but the effect did not take place with the protonitrate or pernitrate of mercury separately. He and M. Lassaigne investigated this action, and found that a solution containing both prot and per nitrate always produced the effect. Such a solution is always produced when mercury is dissolved by moderate heat in nitric acid; the production of colour is so ready, that if a piece of animal matter, as dried white of egg, caseum, horn, &c., be moistened with the mercurial solution, it will be reddened in eight or ten seconds, and if warmed (as upon platina foil, six or eight inches above a candle), will take a rich purple red colour. The effect may be produced even with milk, mucus or dissolved gelatine.

Organic substances containing azote were such as, in this way, became coloured. Thus starch was not coloured. Gluten was readily coloured, and it was found easy to find out very small quantities of gluten in starch by the rose tint then acquired: but still all organic azoted substances were not coloured. Fibrin and the varieties of albumen, including caseum, horn, wool, milk, membranes, &c., gelatine, silk, vegetable albumen, wheat flour, sweet almond, and some other substances, became coloured. Urea, uric acid, osmazome, picromel, sugar of milk, sugar, starch, lignine quinia, morphia, and the vegetable acids, were either not coloured at all, or only pale yellow.

A solution made of one part of mercury in two parts of nitric acid was boiled for four or five minutes, to convert part into pernitrate, then diluted with its bulk of water, and silk or wool immersed for ten or fifteen minutes, at temperatures from 113° to 122° Fah.: it was thus dyed of an amaranth colour, more or less deep, and which, upon the silk, withstood the action of light, and of weak sulphuric acid or alkaline solution. The colour appears due to a combination of the mercurial salt, for the silk becomes brown by a solution of hydrosulphuret; and 100 parts of white silk increased by 17 or 18½ parts during dyeing.\[1\]

* Silliman’s Journal, xvi. 365.  † Annales de Chimie, xlv. 435.
26. TANNING OF LEATHER BY GRAPE MARC.

A medical man of the neighbourhood of Narbonne has announced that the marc of grapes, after being distilled for the purpose of separating the alcohol, is an important assistant to oak bark, in the tanning process. After preparing skins in the usual manner, he placed them in the pits with the marc, in the place of bark. In thirty-five or forty days the tanning was finished. The expected advantages are, i. shorter time; ii. reduction of the price of oak bark; iii. a more agreeable odour of the leather than that given by oak bark; iv. greater strength in the leather.*

27. ANALYSIS OF A SALIVARY CONCRETION, BY PROFESSOR GOEBEL, OF DORPAT.

The calculus in question was one inch and a half in length, and twenty-eight grains in weight, and consisted of a great number of concentric layers; it was examined in the following manner. The digestion of fifteen grains of the powdered substance with sulphuric ether yielded a colourless extract, by the evaporation of which one-eighth of a grain of yellowish white fatty matter was obtained, which had no taste or smell, was insoluble in water and alcohol, and on being incinerated exhibited a slight trace of iron. The residuum of the powder from the digestion with ether was boiled with alcohol, the liquid evaporated, and half a grain of a yellowish substance obtained, half of which was soluble in water; the solution was of a weak saline taste, and had the odour of osmazome; it was precipitated by the oxalate of ammonia and nitrate of silver. The other half, which was not dissolved by the water, was soluble in ether, and exhibited all the properties of the ethereal extract. The remainder of the powder being boiled with water, a colourless solution was obtained, which, during evaporation, became of a red colour, and yielded one grain and a half of a dry brownish residuum, which was perfectly soluble in water, but not soluble in, nor discoloured by, ether or alcohol. The red colour of the solution, which very much resembled that of the sulpho-cyanate of iron, was entirely destroyed by adding a few drops of liquid ammonia or muriatic acid; and as the incinerated mass of the brown residuum, after having been heated with a few drops of nitro-muriatic acid, and dissolved in water, was precipitated by the tincture of galls, of a bluish black, by the ferro-cyanate of potash, of a blue, and by the sulpho-cyanate of potash, of the original red colour, there can scarcely be any doubt that it proceeded from the presence of sulpho-cyanate of iron. In order to ascertain this still more clearly, nine grains of the substance were boiled with distilled water, and on adding a few drops of the solution of chloride of iron, the liquid was found immediately to become of an intense red colour. No trace of potash could be

* Recueil Industrielle, xvi. 85.
found, but there was a considerable portion of soda, and it is accordingly reasonable to suppose that the sulpho-cyanogen had originally been combined with sodium, and only during the evaporation become united to the iron. The residuum from the powder which had been successively submitted to the action of ether, alcohol, and water, was now heated with muriatic acid, which formed a transparent solution, and an insoluble gelatinous substance of one-sixth of a grain in weight, after having been dried. The solution was saturated with ammonia, and yielded twelve grains and a quarter of phosphate of lime, with a small quantity of animal substance, as appeared from the smell during combustion. The remainder of the solution being mixed with oxalate of ammonia, formed a precipitate of oxalate of lime, which, on being ignited, was found to be one-eighth of a grain; besides there were some traces of carbonate of magnesia. Fifteen grains of the salivary concretion would accordingly consist of—

<table>
<thead>
<tr>
<th>Substance</th>
<th>Quantity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fatty substance</td>
<td>0.375</td>
</tr>
<tr>
<td>Osmazome</td>
<td>0.250</td>
</tr>
<tr>
<td>Sulphates, iron, chloride of calcium, sulpho-cyanide of sodium</td>
<td>1.500</td>
</tr>
<tr>
<td>Animal substance (mucus?)</td>
<td>0.166</td>
</tr>
<tr>
<td>Phosphate of lime</td>
<td>12.250</td>
</tr>
<tr>
<td>Carbonate of lime</td>
<td>0.212</td>
</tr>
<tr>
<td>Carbonate of magnesia</td>
<td>0.247</td>
</tr>
<tr>
<td>Water, and loss</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>15.000*</td>
</tr>
</tbody>
</table>

§ III. NATURAL HISTORY.

1. Power of Carbonic Acid on the Lungs.

When M. D'Arcet went to visit the very abundant and curious source of carbonic acid, existing at Montpensier, in the department of Puy de Dome, he endeavoured to ascertain personally the effect of the gas when respired. He kneeled down, therefore, near the larger source, supporting himself on his hands, and advanced his head slowly downward, intending to raise himself the moment he felt any indication of risk: but on commencing the respiration of the gas, the effect of feebleness and extinction of power was so sudden, that he fell down flat, with the face entirely immersed in the current of carbonic acid, and would have lost his life, but that the guide whom he had forewarned, raised and carried him away to the fresh air.

M. D'Arcet proposes two curious uses of the place. The nature

* Schweigger-Seidel's Jahrb. 1830, iv. 403.
of the ground, assisted by certain protecting hedges, will enable the
carbonic acid to collect in large quantities. A cistern is to be
formed at the lowest level, and then when animals come to drink the
water, or are tempted by the green shade, they will be killed, and
thus much game is calculated upon for the advantage of the village.
Then a house is to be built with an inclined floor, a pulley, a double
rope, &c., so that a dog may be tied to the rope, led into the car-
bonic acid atmosphere in the house, rendered insensible, hauled up
again and revived by the fresh air; and thus by making the celebrated
experiment of the Grotto del Cane in a scientific way, much com-
pany, it is expected, will be drawn to the place*

2. On the Phenomenon of Blushing.
M. E. A. Lauth observes, that he is not aware that any precise in-
formation has been afforded as to the kind of vessels which produce
the colour of the face. Most physiologists merely say that it depends
upon the capillaries. M. Lauth states, that if the arteries are suc-
cessfully injected, the whole of the face becomes of an uniform red
tint. It cannot, therefore, be these vessels which produce the phe-
nomenon of blushing. He has derived the following results from a
perfect injection of the facial veins: the cheeks were deeply
coloured, the chin, the tip of the nose and the forehead obtained a
slighter tint, and the other parts of the face were still less coloured.
This kind of colouration resembles that which is produced by men-
tal emotions during life, and we may therefore conclude that blushing
depends in part upon venous congestion †.

3. General Emphysema formed by a Combustible Gas.
This singular case was described by M. Bally, at the Académie
Royale de Médecine. A man twenty-five years of age, who had been
ill fifteen days, was admitted into L'Hôpital Cochin, with symp-
toms of typhus fever. He also complained of severe pain in the
left thigh, and whilst he was in a state of delirium, he said he had
been bitten on the knee by a dog. The limb was most attentively
examined, but not the slightest trace of such an accident could be
discovered. The thigh and scrotum were much swollen. He died
the following day. Dissection eight hours after death. The surface
of the body was soiled by blood, which had also transuded through
the integuments of the thighs: some blood had also been dis-
charged from the nose. The whole body was emphysematous, but
the left inferior extremity was in this state to a very high degree;
it was double its natural size, of a brown colour, and covered with
numerous phlyctæna, some black, of great extent and collected in
clusters, from which escaped a reddish serous fluid, mingled with a
quantity of gas; others were white, and from these nothing but air
escaped.

* Recueil Industriel, xvi. 220. † Mem. de la Société, &c. de Strasbourg.
The limb sounded upon percussion, and when it was pressed with the hand crepitation was distinctly heard; the abdomen was much distended with gas: the face and temples were deeply injected with blood, and were of a violet colour. Upon dividing the scalp a large quantity of dark red blood escaped. The nerves and lungs presented no remarkable appearances, heart pale, and void of blood. In the intestines were observed those alterations which are so commonly detected in cases of typhus fever. Bubbles of air filled the vessels of the pia mater and the left vena saphena. The lymphatic ganglions of the mesentery were enlarged, and contained gas, which took fire from the flame of a taper, and produced an explosion. The same phenomena also followed the exit of air which was contained in the legs, thighs, and scrotum, where incisions had been made into these parts. A puncture was made in the abdomen, and the gas which escaped also took fire and burned for some time; the flame was blue at its base and white at its summit. The combustion extended to the puncture which had been made with a trocar. The edges of this aperture became black, and were consumed, and the aperture itself was enlarged to double the size it had originally been made. The gas which was contained in the subcutaneous cellular tissue of the thorax was equally inflammable *

4. Poisoning by Mouldy Bread.

Dr. Westerhoff attended, in 1826, upon two children of a labourer, who had been simultaneously attacked with the following symptoms. The eldest, ten years of age, had his face red and swollen, his countenance was animated and bewildered, tongue dry, pulse feeble and quickened, head-ache, giddiness, unextinguishable thirst, violent cholic, desire to sleep, and alternate unsuccessful attempts to vomit; subsequently sudden vomiting and very abundant alvine evacuations, after which very great faintness, indifference to everything, and sleep only for a few minutes at a time. The younger, eight years of age, was even more violently attacked. Having understood that they had eaten the preceding day only a piece of old mouldy rye bread, Dr. Westerhoff prescribed a demulcent treatment, and they soon recovered.

Some time afterward, several boatmen having eaten some mouldy rye bread were attacked with similar symptoms, but they were quickly relieved by vomiting, which came on spontaneously. The question suggested by these cases is, whether this kind of poisoning arises from an alteration in the quality of the bread, or from the vegetation which constitutes mouldiness (mucor mucedo) †.

5. Cure of Scrofula by Iodine.

A report, expressing the utmost approbation, has been made by MM. Dumeril and Majendie to the Académie des Sciences at Paris, upon M. Lugol's application of iodine, in scrofulous cases, at the

hospital of St. Louis. They state that that very common and
dreadful disease, which was formerly considered as incurable, and,
by a rigorous rule, subjected its sufferers to exclusion from the hos-
pitals, may now be cured; that it is not merely cases in their first
stage, but those that were exceedingly advanced (scrofulous con-
sumptions), that were cured. Where the glands, organs, articula-
tions, and bones had suffered greatly, still a month sufficed to cure
the patients. M. Lugol has operated only in the worst cases; such
as, having no hope, came to the establishments to die. ‘M. Lugol
does not claim the discovery of the utility of iodine in scrofula, but,
by the great number of cures which he has effected by his zeal and
perseverance, and by the light he has thrown upon the internal and
external application of iodine, in various states of preparation, M.
Lugol has advanced medical science an important step.’ The Aca-
demy approved the report*.

In 17 months, 109 scrofulous patients had been treated with
iodine only. At the end of 1828, 39 were still under the physicians’
hands, 30 had left the hospital much improved: with 4 the appli-
cation had been useless; 36 were perfectly cured†.

6. ON THE USE OF THE SECALE CORNUTUM.

The following general results, obtained by Dr. Villeneuve in 720
cases, are quoted by Dr. Armour in a paper upon this subject:—
i. In 600 the success was complete in cases of labour, properly so
called, i. e. for the expulsion of the fetus alone, living or dead, at
the term or otherwise, the pregnancy being simple or of twins. ii.
Five cases of success of expulsion of the placenta. iii. Five cases of
success in flooding after delivery. iv. Sixteen cases of incomplete
success, which consisted of cases in which the ergot excited the ex-
pulsive powers for a certain time only, the delivery not being termi-
nated naturally till several hours after the employment of the
medicine, or of cases in which, after having advanced the labour to
a certain degree, the application of instruments became possible,
and was made. v. Eighty-two cases of complete failure, in which
the ergot had no sensible effect, producing no return of uterine
action, whatever doses were given. vi. Twelve disagreeable or fatal
results, either for the mother or child, attributed by different authors
to the immediate action, or to the secondary effects of the ergot.
This proportion of seven and a half of success to one of failure, is
seldom furnished by other therapeutical agents employed to combat
any morbid state‡.

7. ON THE HOLOTURES AND PARTICULARLY OF THE HOLOTHURIA
PHYSALIS.—(Linn.)

This species of molluscae, vulgarly galère in French, and in Eng-

* Revue Ency. xlix. 239. † Recueil Industrielle, xv. 229.
lish, 'Portuguese man-of-war,' so rare in collections, so difficult to preserve, so incompletely described by naturalists, and, it must be owned, so little worthy of observation when deprived of life, is, perhaps, one of the most curious inhabitants of the equatorial seas. There are few navigators who have not sought to ascertain some of the habits of life of these singular animals, whose extraordinary form, brilliant colours, and habit of remaining floating on the surface of the water during calm weather, has attracted the attention of all navigators. These habits are the origin of the vulgar names given to them by the sailors. The body of the smallest of these creatures which we have been able to observe, was about 2 centimetres (0.8 of inch) long, and that of the largest was 17 centimetres (6.7 inches). Their form, which it is impossible to compare to that of any other animal living, rather resembles a small bladder stretched and filled with air, of an azure blue, slightly streaked with deeper tints and green; their body, almost cylindrical, is surmounted by a crest, which is in plaits, very moveable, and edged by the most lively tints of purple and rose-colour. This little crest serves the animal for a sail, and by the disposition which it gives it, regulates its movement in nearly the same manner as a ship. According to the strength of the wind, it spreads, rests, or compresses its sail, and in heavy weather, it allows itself to float, by means of a respiratory apparatus of a peculiar construction. The lightness of its body is such, that it appears resting on the water, and when plunged in alcohol it floats again to the surface of that fluid. The lower and middle part of the animal is armed, at different lengths, with tubes, papillae, and retractile feelers, some of which are from sixteen to eighteen feet long, disposed spirally or in chaplets of the most beautiful blue, and most delicate rose-colour, and serve at once as organs of absorption, defence, and locomotion. These tubes, papillae, and fibres, contain a viscous matter, which produces pustules on the human skin, and occasions a pain similar to that of a large but superficial burn. This property is not easily got rid of; vessels in which one of these animals has been plunged, must be washed several times in water, and carefully scoured before they can be used without inconvenience; and linen, which had merely been rinsed in soap and water, had this quality of irritation fifteen days after it had been used in making observations on these animals. Cutting these feelers does not produce death, at least for a considerable time; and incisions made transversely in the body with scissors do not deprive the animal of life. The membranous crest appears to have more irritability than the other appendages, and the animal appears to contract itself, and to suffer more when tormented there than in any other part. Naturalists suppose that the holothurus feeds on animals of all kinds, occasionally on some of a very considerable relative size, and that they have a very strong and active digestion. They, in their turn, serve as food to species of the scombri and medusae, against which their weapons of defence are unavailing.
8. Defence against Flies, used by the Butchers of Geneva.

It is said that the butchers of Geneva have for a long time used the oil of laurel as a substance which prevents the flies from approaching their meat. The odour of this oil, though strong, is not very disagreeable, and the flies will not approach the walls or parts which have been rubbed with it. The person who describes these effects says, that he has, in this way, guarded the gilt frames of mirrors and pictures most perfectly from flies*.

9. The Palm of Chile.

It is chiefly in the middle province that the palm of Chile (Micrococcus) is found. It is not a common tree, being very partially distributed, but several of the estates owe much of their value to the number of palms upon them; and, although the stem is useless, the leaves, sap, and fruit, yield a large income to the proprietor. For thatching houses, the leaves are considered better and more durable than any other material; the sap, boiled down to a syrup, is used as a substitute for honey, and has a very agreeable flavour; and the small cocoa-nuts, about an inch in diameter, of which every tree produces a great number, are highly esteemed, and form a considerable article of export to Peru. A curious method is employed to free the nut from the green husk in which it is enveloped, a process that was formerly attended with a great loss of time and labour. A number of cows and oxen are driven into an enclosure, where a quantity of the fruit is spread, and being very fond of its husk, they immediately begin to feed on the fruit, only slightly masticating it in the first instance, and swallowing the whole; afterwards, while chewing the cud, the nuts are rejected; and when the meal is finished, a heap of them is found before each of the animals, perfectly free from the husk, the cattle being thus supplied with food at a season when little grass remains on the hills, at the same time that they effectually perform a very useful operation†.


The growth of weeds between the stones of a pavement is often very injurious as well as unsightly. The following method is adopted at the Mint at Paris and elsewhere with good effect. One hundred pounds of water, twenty pounds of quick lime, and two pounds of flowers of sulphur, are to be boiled in an iron vessel; the liquor is to be allowed to settle, the clear part drawn off, and being more or less diluted, according to circumstances, is to be used for watering the alleys and pavements. The weeds will not appear for several years‡.


Eye-witnesses assert that in Russia the inhabitants usually preserve

‡ Recueil Industrielle, xv. 246,
hay with all its natural verdure. To obtain this effect, the grass, as soon as cut, is (without being allowed to fade) instantly stacked. A kind of chimney, made with four rough boards, is constructed in the middle of the stack, and it appears that this channel prevents the accumulation of heat from fermentation; and that the herb thus treated retains all its leaves, its colour, and its primitive taste. The size of the stacks is not mentioned.*

12. Remarkable Propagation of Wind.

Whilst the bells were ringing to church, at Albany, on the 12th of July, 1829, a very violent gust of wind from the south-east passed over the town. This gust passed over New York, which is to the south of Albany, when the service had proceeded for some time: so that this south wind was rendered evident in the northern town an hour nearly before it was felt at the southern position, and it had been propagated from north to south in the direction exactly contrary to that in which it blew.

Franklin remarked that violent north-west winds in the United States frequently had their origin in the quarter towards which they passed, and was inclined to attribute them to great and sudden alterations in the atmosphere of the Gulf of Mexico. To explain the present instance in the same manner, a diminution in the atmospheric pressure to the north of Albany must be considered as having occurred†.

13. Thoughts on North and South Winds.—(By M. Alphonse Blanc.)

It has been generally admitted that winds are mostly, if not always, caused by dilatations or condensations of the air due to changes of temperature. In fine, quiet weather, the wind from the east in the morning, often becomes south at midday, and west in the evening, and may, with great appearance of reason, be attributed to the dilatation of the air by the sun in the east, south, and west in succession, of the place of observation. It is also known that a wind often blows in one place before it blows in another place to windward of the first; and in such cases it has been supposed that the effect is due to some great condensation in the air at some place to leeward of the place where the wind is felt.

Now the barometer should be affected differently according to the nature of the cause of wind. If condensation occurred about the pole, the air of surrounding places should flow towards it, a partial vacuum should occur in those places, forming a south wind gradually extending towards the south, and the others to which the wind should reach, and the barometer should fall at those places. But if expansion of the northern air occurred, a north wind should commence at the north, and be propagated to the south, driving or accumulating the air before it, and the barometer should rise in all those places to which it reached.

The effects due to air passing from the north southwards should be greater, because, arriving at warmer regions, it would become heated, and tend to dilate; the contrary effect would occur with air passing from the south to the north. But these effects would be smaller, if the changes of tension which act as causes of the wind, occurred to the south instead of in the north, because the air, driven forward by expansion, would contract as it travelled northwards, and vice versa.

The variations of the barometer between the tropics is scarcely any thing; the winds are very regular, the cause is permanent, and the equilibrium appears constant. From this regularity, it may be concluded, that the north and south winds which we have, originate in the north; for how can it be imagined that a south wind should generally come to us from a place where the contrary wind is constant? On this view, the south wind would generally be caused by a condensation, and the north wind by an expansion of the polar atmosphere, and the barometer ought to fall with south winds and rise with north winds. This effect is usually observed.

Though the south wind, in passing northward, is cooled, and deposits part of its vapour in clouds and rain, it would appear, from the above reasoning, that such deposition is not the cause of the descent of the barometer. If vapour were the cause, the effect ought to occur in every place where there is rain; but this does not happen. It may easily be conceived, that although the south wind may ordinarily be caused by condensation of air in the north, and be accompanied by depression of the barometer, it may sometimes be due to dilatation in the south, and then the barometer would rise.


Tirhoot is one of the principal districts in India for the manufacture of saltpetre: the soil is every where abundantly impregnated with this substance, and it floats in the atmosphere in such quantities, that during the rains and cold weather it is attracted from thence by the lime on the damp walls of houses, and fixes there in shape of long downy crystals of exceeding delicacy. From damp spots it may be brushed off every two or three days almost in basketsful. In consequence of all this, the ground, even in hot weather, is so damp that it is extremely difficult either to get earth of sufficient tenacity to make bricks, (the country being quite destitute of stones,) or, when made, to find a spot sufficiently solid to sustain the weight of a house. Even with the greatest care the ground at last yields, and the saltpetre corrodes the best of the bricks to such a degree, that the whole house gradually sinks several inches below its original level. Houses built of inferior materials of course suffer much more; one, of which the inner foundations were of unburnt bricks, absolutely fell down whilst I was at Mullye, and the family in it escaped almost by miracle. My own house, which was not much better, sank so much,

* Annales de Chimie, xlv. 421.
and the walls at bottom were so evidently giving way, that I was compelled, with extreme expense and inconvenience, to pull down the whole inner walls and build them afresh, in a more secure manner. From the same cause a new magazine, which government directed to be built with an arched roof of brick-work, was, when complete, found so very unsafe, that it was necessary to demolish it entirely and rebuild it on a new plan, with a roof of tiles*.

15. Progressive Motion of the Glaciers.

The ladder which M. de Saussure used in crossing the crevices in the ice during his first visit to the Col du Géant, and which he left on the upper part of the glacier, has lately been discovered imbedded in the Mer de Glace, in a situation nearly opposite to the aiguille called Le Moine. This ladder, moving on with the body of ice, will thus appear to have advanced three leagues since the year 1787.

Captain Sherwill, in his relation of his ascent of Mont Blanc, speaking of the glaciers, says, 'In traversing these stagnated oceans, very large blocks of granite, of many tons weight, may be seen riding on the surface of the ice. These blocks have afforded the means of ascertaining a fact of importance. The experiment I am about to relate to you was made last year by some of the guides of Chamouni. Two poles were erected, one on each side of the glacier, out of reach of its movement, and so placed as to be in a direct line with a block of granite. In the course of twelve months, this block had entirely changed its position, as respecting the two poles, and had advanced about one hundred yards on its march towards the valley, a clear proof that the glaciers do move on, and are continually diminishing at their lower extremity, by the melting of the ice, and increasing at the upper end by the constant snows.'

If the progress made by the ladder of M. de Saussure taken for one year, and the result of the experiment made at the instigation of Captain Sherwill, should not appear to agree, it must be recollected that from the Col du Géant to the spot where the ladder is, is a very rapid descent, and of course the march of the glacier would be rapid in proportion; whereas the experiment of Captain Sherwill was made on a level part of the same glacier, the Mer de Glace, where the ice is of a more compact texture than that at an elevation of above ten thousand feet, and consequently its progress towards its final issue would be somewhat slower †.

16. On the Comparative Quantity of Salt Contained in the Waters of the Ocean.—(By M. E. Lenz.)

The following experiments were made on the expedition of Captain Kotzebue:—Those with water at seven different places of the ocean, and at various depths, tended to prove, that from the equator to

* Calcutta Transactions. † Philosophical Magazine, N. S. ix. 32.
Vol. II. Aug. 1831. P
45° north latitude, the ocean contains, at the same place, the same quantity of salt, as low at least as 1000 toises. For ascertaining the quantity of salt contained in the water on the surface of the ocean, the observers used hydrometers which, by laying weights on them, were made to sink into the water to a mark on the thin neck of the instrument. It is well known that Dr. Erman has ascertained the contraction of salt water of the specific gravity 1·027 for every degree of the thermometer, from 12 R. (59 F.) to —3° R. (25½ F.), and has proved that such water has no maximum of density, before freezing, like common water. With a view to the investigation here under consideration, the table of Erman was extended, by experiments made for the purpose, as far as to 24° R. (86 F.) It was then ascertained that a small difference in the specific gravity of the water had no great influence on the law of contraction by change of temperature, since experiments instituted with this view proved that two portions of water, whose specific gravity was 1·02 and 1·03, (between which limits undoubtedly all waters of the ocean are comprehended,) observed nearly the same law, and perfectly so at those temperatures at which observations are made on the ocean. The experiments could be very approximately represented by this expression for the density (d) of water whose specific gravity = 1·027, and whose temperature is (t) degrees of Reaumur's thermometer:

\[ d = 1 - 0.0002053 \cdot t + 0.0000003723 \cdot t^2 - 0.000000188086 \cdot t^3. \]

and by this formula the numerous observations of specific gravity of water, in different parts of the ocean, were reduced to the same temperature, viz. 14° R. (68½ F.)

The general result of all the observations is as follows:—

i. The Atlantic Ocean contains more salt than the Pacific Ocean, and the Indian Ocean is, therefore, more salt near the Atlantic than near the Pacific.

ii. In each of these great oceans there is a northern and a southern maximum of salt; the northern is farther from the equator than the southern. The minimum is, in the Atlantic, a few degrees north of the equator; the same is probably the case in the Pacific: there were, however, not sufficient observations to prove it.

iii. In the Atlantic Ocean the water is more salt to the westward; there seems to be no such difference in the Pacific.

iv. The greatest specific gravity of the water in the Atlantic Ocean, at the northern maximum, in lat. 20½° and long. 40° W. of Greenwich, was found to be = 1·02856; that of the Pacific, at the southern maximum, in lat. 17° and long. 119° W. = 1·028084.

v. To the northward of the northern maximum, and to the southward of the southern maximum, the specific gravity of the water continually decreases with the increase of latitude.

It is clear that the quantity of salt in the water on the surface of the ocean must mainly depend on the evaporation; and as this de-
pends again on the temperature of the air and on the wind, it will be found that the different degrees in which these causes operate together, will afford a satisfactory explanation of the circumstances here described *.


These observations were made by the scientific men attached to Captain Kotzebue's expedition in the year 1823—1826, on the Island of Luzon (14° 34' north lat., and 239° 9' west of Greenwich) for ascertaining the periods of its regular falling and rising during twenty-four hours. The barometer was kept in a room six toises above the level of the sea, in which the temperature was nearly the same day and night, (about 25° Centigrade, or 77 Fahr.) and the observations were made on eleven different days between the 12th and 26th of December. The following are the general results deduced from the whole of the observations. The barometer has a maximum of height at . . . 9h 1 1/4 A. M.

it then falls till . . . 4 28 6 P. M. on an average 1.04 line;

it rises again till . . . 9 58 3 P. M. " 0.687 ",

and falls again till . . . 4 30 0 A. M. —-

lastly it rises again till 9 1 1 A. M. on an average 0.445——

18. Comparison of the Mean Temperature of the Air and the Water on the Surface of the Ocean within Twenty-Four Hours.

These observations were made on the expedition commanded by Captain Kotzebue during the years 1823—1826. Four observations were made within twenty-four hours; one half an hour before sunrise, the second an hour and a half after noon, and the two others at equal intervals between the afternoon's and next morning's observations. The result which is obtained from all the observations is this: in the zone from 45° N. lat., to 30° S. lat., the mean temperature of the water on the surface of the ocean, whether taken for the whole year, for single months, or for single days, exceeds the corresponding mean temperature of the air with which it is in contact. Beyond these latitudes, the means taken for twenty-four hours, during the summer months, (there being no observations during the winter months,) varied, sometimes the one, sometimes the other being higher.

The following were the highest mean temperatures for twenty-four hours:—

In the northern hemisphere, of the air = 27°.225 C (81°.0 F.) ; of the water 27°.55 (81°.6 F.)

In the southern hemisphere, of the air = 26°.975 C (80°.53 F.) ; of the water 28°.30 (82°.9 F.) †.

* Petersburgh Transactions, 1830. † Ibid.
19. Professor Oltmanns on the Geography of South America.*

It is well known that the results of the expedition of Malespina, which was most richly fitted out for scientific investigations, and most ably conducted, have never been communicated to the public. M. Bauza, a skilful officer attached to the expedition, communicated to Professor Oltmanns the astronomical observations made on that expedition, which he has saved from the fate of the other fruits of that ill-starred expedition, on condition that he would recalculate them. Professor Oltmanns has redeemed his pledge, and in laying before the world the results of his calculations of the observations of Malespina, together with those of other observers, he confirms the opinion expressed by Bauza, that the former calculations were very inaccurately executed. He likewise agrees with M. Bauza respecting the inaccuracy of the observations made on board the Conway, some of which, when recalculated by Professor Oltmanns, gave results differing more than twenty minutes in arc from what is most probably true. From the numerous list given by Professor Oltmanns, we extract the following positions of some remarkable places on the coast of South America, as corrected by Professor Oltmanns; among which the longitude of Rio Janeiro depends on a great number of occultations of Jupiter's satellites.

<table>
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<tr>
<th>Places</th>
<th>Latitude</th>
<th>Longitude west of Paris</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rio di Janeiro</td>
<td>22° 54'</td>
<td>45° 36' 13''</td>
</tr>
<tr>
<td>Buenos Ayres</td>
<td>34 36 40'</td>
<td>60 47 0</td>
</tr>
<tr>
<td>Monte Video (Observatory)</td>
<td>34 54 40</td>
<td>58 36 50</td>
</tr>
<tr>
<td>San Carlos de Chiloe</td>
<td>41 52 0</td>
<td>76 11 20</td>
</tr>
<tr>
<td>La Concepcion</td>
<td>36 49 30</td>
<td>75 25 55</td>
</tr>
<tr>
<td>Valparaiso</td>
<td>33 2 0</td>
<td>74 2 10</td>
</tr>
<tr>
<td>San Jago de Chili</td>
<td>33 26 15</td>
<td>73 17 17</td>
</tr>
<tr>
<td>Coquimbo</td>
<td>29 56 40</td>
<td>73 43 48</td>
</tr>
<tr>
<td>Callao (Casillo del Callao)</td>
<td>12 3 40</td>
<td>79 34 31</td>
</tr>
<tr>
<td>Guayaquil</td>
<td>2 12 0</td>
<td>82 18 10</td>
</tr>
</tbody>
</table>

20. On Beauchamp's Geographical Positions in the East. (By Professor Oltmanns.)†

It is well known that the geography of the interior of Asia was perfectly unknown till within about half a century, and even at this moment there are, in the interior of Persia, and other central regions of Asia, large tracts without a single place whose position is determined by astronomical observations. We are indebted to two travellers, Niebuhr and Beauchamp, for the first advances made in the geography of the south-western parts of Asia. Niebuhr's ob-

* Berlin Academy, Year 1827: Berlin, 1830, 2d Part, p. 37.
† Mem. of Berlin Academy, Year 1827: Berlin, 1830, p. 139.
observations were in that part, however, confined to latitudes, while
the lunar distances observed by him in Egypt have, after a lapse of
fifty years, been calculated by Bürg, and received by the learned
with the praise which they deserve. Beauchamp made two scientific
journeys to the East, but was much better provided for making
observations on the second, than on the first. The published observa-
tions of this rather capricious traveller did not answer to the high
expectations that had been formed. The numerous errors, espe-
cially in writing, have baffled the exertions of some of the ablest
and most indefatigable calculators; and the suspicion that some of
his observations have been subsequently corrected by himself, as
well as the circumstance that his observations at Casvin of a lunar
eclipse did not give the position of the Caspian Sea, near which
that place is situated, have lowered the estimation of Beauchamp's
labours. Professor Oltmanns obtained a manuscript of some of
Beauchamp's observations which was supposed to be lost, entitled
'Relation Historique et Géographique d'un Voyage de Constantin-
ople par mer, l'an V de la République. Par le Chevalier Beau-
champ. 38 pp., fol.' In this he was likewise much disappointed, as,
of many observations, results only were given; while in others it
appeared that Beauchamp always neglected the aberration and nu-
tation. Notwithstanding this, Professor Oltmanns has thought it
worth while to recalculate the observations by the latest tables, and
he has thus determined the following geographical positions:
Trapezunt.—Lat. (mean of two observations) 41° 2' 18''.
  Long. (by chronometers, lunar distances and eclipses
  of Jupiter's satellites, all agreeing tolerably well),
  mean 37° 11' 18'' E. of Paris.
Sinope.—Lat. (mean of seven observations of the sun and stars)
  42° 1' 42''.
  Long. (mean of chronometers and eclipses of Jupiter's
  satellites) 33° 45' 33'' E. of Paris.
Gydrón.—Lat. (one star observed) 41° 54' 6''.
  Long. 3° 37' E. of Constantinople.
Jeniki.—Lat. by three observations of the sun, mean 41° 59' 49''.
  Long. by chronometers, 31° 16' 15''.
Amasra.—Lat. (according to Beauchamp's own calculation),
  41° 46' 8''.
  Long. 29° 50' 30''.
Neracle du Pont or Eregri.—Lat. by four stars, mean 41° 17' 6''.
  Long. by two eclipses of Jupiter's sat-
ellites, mean 29° 9' 56'' E. of Paris.
Bagdad.—Here Beauchamp had an observatory on his first expedi-
tion: lat. then found, 33° 19' 50''.
  Long. by eclipses of Jupiter's satellites, a solar eclipse,
  and an occultation of Jupiter by the moon, all nearly
  agreeing, mean, 42° 2' 5'' E. of Paris.

A discovery of great historical importance has lately been made at Autun, a city about 160 miles to the south-east of Paris. It was well known that Eumenes had placed in the schools which bore his name a marble tablet representing the itinerary of the Roman roads leading from the territory of the Ædui in Celtic Gaul, to Italy. This marble, having been accidentally broken, was employed, in common with a number of other ruins of ancient monuments, in forming the foundation of the Abbey of St. Jean-le-Grand, founded at Autun at the close of the sixth century by Bruneau. This precious marble was considered as irretrievably lost; but M. Martigny, of Autun, has undertaken an excavation, by means of which he has already recovered a fragment of the itinerary, a marble ewer, a capital, &c. Should M. Martigny succeed in recovering the whole of the marble, it will probably be of great use in correcting or completing the Itinerary of Antoninus, the table of Peutinger, and the Arundel marbles. The Academy of Sciences of Dijon has the fragment which has been found, and is preparing to have it engraved and published.

22. On the Illumination of Theatres.—(Additional Remarks, by Mr. Ainger.)

Since the paper printed at page 45 was written, I accidentally learned that an essay on the same subject had been published by M. Lavoisin, in the Memoirs of the French Academy for 1781. I have examined the essay in question, and I find, as might have been expected, that the objections to the existing mode of illumination had been fully felt and expressed half a century since. The plan suggested by M. Lavoisin is, however, very different from that which I have submitted. In the first place, M. Lavoisin retains the foot-lights, in which are, I think, comprised nine-tenths of the objections to the present system; preserving these, it seems scarcely worth while to incur any considerable expense to remove the other comparatively trifling evils. M. Lavoisin, in addition to the foot-lights, suggests the use of certain powerful lamps with reflectors above the stage for the purpose of illuminating the centre scene when it is so far back as to be ineffectually lighted by the foot-lamps. He proposes to light the audience by nine burners, with ellipsoidal reflectors, placed above the ceiling, which is to be perforated for the purpose; but as this would leave the ceiling itself quite dark, he adds other lamps round the walls, with a view to remove that defect.

It is gratifying to find that the subject has been thought sufficiently important to merit the attention of so distinguished a philosopher as M. Lavoisin; and on his authority I venture to hope that it will be taken up by those who alone have the means of making an effectual experiment.
On the Illumination of Theatres

Fig. 1

Fig. 2

London: Published by John Murray 1831.
A Drawing of

THE HEAD OF BOLA.

A NEW ZEALAND CHIEF.

To illustrate "An Account of the Mode of preparing Human Heads in New Zealand, &c."

By George Bennett, Esq. M.R.C.S. &c.

Drawn from the original Head by M A H

On Stone by M Gaun

London: Published by John Murray, August 1, 1834.
The mode of preparing human heads among the New Zealanders, with some observations on cannibalism*.

By George Bennett,
Member of the Royal College of Surgeons in London, &c. &c.

It is now fully ascertained, that the natives of the New Hebrides, the Marquesas, New Zealand, and other Polynesian islands, are cannibals; yet it is among the New Zealanders only that the custom obtains of preserving the heads of their enemies as tokens of victory, and as objects of contempt. There is something analogous to this among certain tribes of Africans, who preserve the skulls of their enemies for purposes similar to those of the New Zealanders. Captain Tuckey observes, respecting this custom among the natives on the river Zaire or Congo, 'the first objects that called our attention were four human skulls, hung to the tree, which we were told were those of enemies' chiefs taken in battle, whose heads it was the custom to preserve as trophies; these victims, however, seemed to have received the coup de grace previous to the separation of the head, all the skulls presenting compound fractures.'—page 101. The New Zealanders sometimes, however, preserve the heads of their friends, but for very different purposes—those of paying respect to the memory of the deceased; to show to their relations who have been

* The plate was given in the last Number.

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absent at the time of their death; and that lamentations may be renewed over them at certain periods.

The preparation of the Moko, or head, among the New Zealanders, most effectually prevents decay, and is at the same time compatible with the perfect preservation of the features. The process, as practised among them, is as follows:—After the head has been severed from the body, the base of the skull is broken by a stick or stone, and the brain removed, &c.; the cavity of the cranium is then frequently washed out until it is well cleansed. The head is then dipped for a few minutes in hot water, which causes the cuticle to peel off; they are careful at this time not to touch the hair, as by so doing it would readily come off, but it afterwards, when cool, becomes more firmly fixed than before, as is well exemplified in the specimens brought to this country. A piece of thin stick is next placed up the septum narium, for the purpose of keeping the nose in shape, aided also by another piece placed internally to bear against the nose as a prevention against its sinking; the nostrils are also stuffed with muka or flax, or pieces of soft wood, for a similar purpose; the eyes are taken out (if those of a chief they are eaten, if otherwise they are thrown away), and the eyelids are stitched up, as well as the mouth, for the purpose of preserving their shape. A pit having been previously dug, hot stones are brought and placed in it: this pit, which is indeed the usual native oven, is so constructed, that it is covered over, with the exception of an aperture above of sufficient size to permit the base of the head to rest upon it. Water having been previously thrown over the hot stones, and repeated as often as considered necessary, a steam is produced, which is also increased by the addition of leaves moistened with water; the base of the head is placed over this aperture, and the heat and steam ascend into it; the hot stones, water, &c., to keep up the requisite heat and steam during the process, being renewed as often as required by the person in attendance, until the preparation is completed. The person whose duty it is to attend to the preparation of the head is careful, when any of the skin of the face appears to wrinkle, to smooth down and preserve it in shape. The time occupied in this process previous to its completion is from twenty-four
to thirty hours. After the head is prepared, it is removed from the oven, placed upon a stick in the sun, and is frequently oiled, but this latter process is not considered necessary for its preservation, but is intended only to give it a more finished appearance. The adoption of this simple, but at the same time excellent method of preserving the human heads, would enable a valuable series to be formed, illustrative of the varieties of the human race.

The object of the natives in thus preparing the heads of their enemies, is to preserve them as trophies, as well as for the gratification of their revengeful feelings; they exhibit them in their war-dances, holding them up as objects of contempt and ridicule; and when advancing to battle, they display them before the hostile party, and accompany the exhibition by arrogant and insulting speeches. They generally consider these heads as tokens of victory; they are brought home by the conquerors to their wives and children, that they also may have an opportunity of rejoicing with them over their fallen enemies, and they offer them to the spirits as a thanksgiving for victory. At the Bay of Islands, Hookianja, North Cape, &c., the chiefs who die (with but few exceptions) are buried unmutilated; but at the Thames, East Cape, &c., the heads of the chiefs are generally preserved, both out of respect for the deceased, and to show to those of his relations who are absent at the time of his death. These heads are never sold, the heads of their enemies only being thus disposed of as a mark of contempt towards them.

One of the heads in my possession (and of which a drawing is annexed) I purchased at the river Thames, and, what is rather unusual, was able in this instance to procure the name, rank, character, &c. of the individual, and it was procured from the chief by whom he had been slain. He was named Bola; his father's name was Tumau, and he was a young chief of the district of Wigato, at the river Thames. His age was supposed to be about eighteen years, and he had not long been tattooed, the whole of which process was not completed at the time of his death. He was described as being a great warrior for his age, and a very enterprising character, always endeavouring to be the first in battle, and to kill the first man,
which exploits are considered among them as highly honourable. During an engagement, he was first shot in the abdomen by a chief named Warrinhu Eringa (who related to me some of this account), and, on falling, was finally dispatched by a blow on the head with a tomahawk; on an examination of the side of the head, the fracture is very visible, and is of some extent.

The New Zealanders care not to conceal that they are cannibals; they relate the atrocities connected with the practice without any appearance of shame or remorse; they only eat, however, of the bodies of their enemies. If an enemy of rank is slain, the eyes, hands, and feet are presented to the highest chief of the conquering party, as they observe, that 'with the eyes their enemy saw his adversaries, with his hands he fought, and with his feet he invaded their territory—they bore him to the combat.' There was a chief of a district in the vicinity of the river Thames, who was pointed out to me as the one who killed the noted chief named Atoi, or Pomari, and who was said, in their style of expression, to have 'eaten of his eyes, and drank of his blood.' Respecting the eating of the eyes, there formerly existed a somewhat similar custom at the island of Tahiti, from which it has been inferred, that the natives of that island were formerly addicted to the horrible custom of cannibalism: the coincidence is curious, and Captain Cook makes the following observations respecting it. 'We have great reason to believe,' he observes, 'that there was a time when they were cannibals. We were told (and, indeed, partly saw it) that it is a necessary ceremony when a poor wretch is sacrificed, for the priest to take out the left eye; this he presents to the king, holding it to his mouth, which he desires him to open; but instead of putting it in, immediately withdraws it. This they call "eating the man," or "food for the chief;" and, perhaps, we may observe here some traces of former times, when the dead body was really feasted on.' Ellis observes, however, in a more recent publication (Polynesian Researches, vol. i. pp. 35, 357,8), that 'It has been supposed that the circumstance of the priests offering the eye, the most precious part of the victim, to the king, who appeared to eat it, indicated their having formerly devoured the men they
had sacrificed.' 'I do not,' he further observes, 'regard this fact as affording any very strong evidence, although I have not the least doubt that the inhabitants of several of the South-Sea Islands have eaten human flesh. From the many favourable traits in their character, we have been unwilling to believe they had ever been cannibals; the conviction of our mistake has, however, been impressed by evidence so various and multiplied as to preclude uncertainty. Their mythology led them to suppose that the spirits of the dead are eaten by the gods or demons, and that the spiritual part of their sacrifices is eaten by the spirit of the idol before whom it is presented. Birds resorting to the temple were said to feed upon the bodies of the human sacrifices, and it was imagined the god approached the temple in the bird, and thus devoured the victims placed upon the altar. In some of the islands, "man-eater" was an epithet of the principal deities; and it was probable, in connexion with this, that the king, who often personated the god, appeared to eat the human eye.' Notwithstanding these judicious remarks, the coincidence is very extraordinary with the custom of New Zealand, where the eye is actually devoured, and where the natives are well known to be cannibals; and what further corroborates the supposition that the Tahitans were anciently anthropophagi is, that, as the author before quoted observes, (Polynesian Researches, vol. i., p. 310,) 'the Tahitans were not altogether free from cannibalism; and occasionally a warrior, out of bravado or revenge, has been known to eat two or three mouthfuls of a vanquished foe, generally the fat from the inner side of the ribs.' From this it would appear, that the exciting cause of cannibalism is, both with this people and the New Zealanders, revenge: for cannibalism, the New Zealanders informed me, arises from this feeling, not from hunger; and from believing that, by eating of the bodies of the valiant, as all those are considered who die fighting on the field of battle, they become inheritors of their courage and valour. The horrible practice of cannibalism having been found existing in the most fertile countries, we must seek for some other motives for the custom than mere hunger; and the above causes, as asserted by the natives, seem to be the most probable. To
eat the bodies, however, hunger would be requisite, combined with the revengeful feeling: all provision being sent away from the field of battle, with the women and children, hunger becomes a concomitant with the revengeful feeling, but not the sole exciting cause. After a battle, it is customary to collect all the dead bodies of the enemy together, and the heads of those intended for preparation having been detached, are delivered to the persons who are habituated to it; the bodies are then cut open, the viscera, &c. extracted, and the remainder cut into pieces; they then proceed to cook and prepare the banquet—in what manner they are not particular: some make an oven and steam it, others roast on the fire, but they seldom or never eat it in a raw state; but it is a common and general custom, on an enemy falling in battle, for his adversary, excited by the demoniacal spirit of revenge, to rush immediately towards him for the purpose of sucking the blood from his throat, before the vital spark has fled. They also dry the hands of their enemies, and fasten them near their huts, the fingers having been previously dried in a contracted form, so as to be used as hooks, on which to hang their baskets, &c. They also preserve the fat from the buttocks, and the internal fat or fare; they melt it down by the aid of hot stones, keep it in calabashes, and eat it with their potatoes: this is more generally done when the person is a powerful chief, and they always express it as being a mark of great contempt towards him. On my asking some of those who gave me information respecting this horrible custom, how they would like to be eaten, the reply was, 'that it was no matter what was done with them after death.' On my inquiring what was done with the bones of the human bodies that were eaten, I was informed that, if those of a chief, they were preserved; those of the arms, legs, &c. being used for making the flutes named Lehu or Bulrua, others as ear ornaments, &c.; but the bones of a common individual were thrown away. With respect to the taste of this food, they describe it as being superior to pork. Vessels are occasionally destroyed by the natives, and the crews massacred; at one time several heads of unfortunate Europeans, who had thus been murdered, preserved in a similar manner as among themselves, were pur-
chased and brought by a colonial vessel to Sydney, New South Wales. One of the chiefs at the river Thames, from whom I made the inquiry, whether he had ever eaten of the flesh of white men, and whether it was better tasted than that of a New Zealander, replied, that 'he had tasted of the flesh of Europeans: sometimes he found it good, sometimes bad, but generally very salt.' It is a curious circumstance that the natives of New Zealand always express a dislike to salt. It is customary, if a chief is ill, for a slave to be killed, as an offering to the spirits, but the body is not eaten; but if a chief is slain, or deeply offended by the chief of any particular district, and his relations should have any slaves in their possession belonging to that district, they are killed and eaten from revenge. During my visit to New Zealand, in June 1829, I was rambling on shore at Wyshaki Cove, River Thames, on a botanical excursion, when, among some rushes which grew on the borders of a rivulet, I observed some bones protruding, and, on a closer examination, found a heap of human bones, apparently belonging to one person. I thought there had been a cannibal banquet at this place, and I brought away several of them with me; but on showing them to a chief, he said they were those of a person who died a natural death; had they been those of a person who had been killed and eaten, they would not be in so perfect a state; and on mentioning that I had found them collected together in a heap, confirmed him in his opinion. He also said of the lower jaw, that if it had been that of an enemy, it would have been cut down, and used as a fish-hook (matau).

At the village of Kororadeka, Bay of Islands, which is much frequented by whalers and other vessels for refreshments, and which is situated opposite to the missionary station of Paihia, several cannibal banquets have taken place on the beach.

Some of the notions which persons in this country entertain on the subject of cannibalism are very erroneous; since my arrival in England, I have had several curious questions asked me: among numerous others this was one—Whether a child, which I brought from Erromanga, one of the New Hebrides group (where they are cannibals), could eat our food? Surprised at the question, I asked why not? 'Because,' was the
reply, 'I thought that, after having been accustomed to devour human flesh, she would not be able to relish any other kind!'

It is supposed that, by purchasing the preserved heads from the New Zealanders, an encouragement is held out for them to engage in war, or to murder their slaves. This I consider erroneous. They have preserved them, from time immemorial, as trophies, and whether they are or are not purchased by Europeans, the custom will continue, until civilization has extended among these noble, but savage people. During a long stay at New Zealand (and that principally at the river Thames, where it is generally considered they might be purchased in some quantity), not more than six were procured; the reason assigned for their scarcity by the natives being, that there had been lately no wars among them.

In conclusion, we may observe, with Dr. Good, that 'one common character runs through savages of every kind. The empire of the heart is divided between two rival deities, or rather demons—selfishness and terror. The chief ministers of the first, are lust, hatred, and revenge—the chief ministers of the second, are cruelty, credulity, and superstition. Look through the world, and you will find this description apply to barbarians of every age and country. It is equally the history of Europeans and Africans; of the Pelasgi, who were the progenitors of the Greeks; and of the Celts and Scythians, the successive progenitors of the English. All the discoveries of modern circumnavigators confirm the assertion; and though the captivating names of Friendly and Society Islands have been given to two distinct groups in the vast bosom of the Pacific Ocean, and the inhabitants in several of them have made some progress in the first rudiments of civilization and government, there is not a people or a tribe to be met with, who are yet in a savage state, that are not still slaves to their debasing and tyrannical passions.'

ON THE TRANSMISSION OF MUSICAL SOUNDS THROUGH SOLID LINEAR CONDUCTORS, AND ON THEIR SUBSEQUENT RECIPROCATION.

By CHARLES WHEATSTONE.

§ 1.

THE fact of the transmission of sound through solid bodies, as when a stick or a metal rod is placed at one extremity to the ear, and is struck or scratched at the other end, did not escape the observation of the ancient philosophers: but it was for a long time erroneously supposed, that an aëriform medium was alone capable of receiving sonorous impressions; and in conformity with this opinion, Lord Bacon, when noticing this experiment, assumes that the sound is propagated by spirits contained within the pores of the body*. The first correct observations on this subject appear to have been made by Dr. Hooke in 1667; who made an experiment with a distended wire of sufficient length to observe that the same sound was propagated far swifter through the wire than through the air †. Professor Wunsch, of Berlin, made, in 1788, a similar experiment, substituting 1728 feet of connected wooden paths for the wire, and confirmed Dr. Hooke's results ‡.

* 'If a rod of iron or brass be held with one end to the ear, and the other be struck upon, it makes a much greater sound than the same stroke upon the rod, when not so contiguous to the ear. By which, and other instances, it should seem that sounds do not only slide upon the surface of a smooth body, but also communicate with the spirits in the pores of the body.'—Sylva Sylvarum, Phonics, § 3.

† 'The pneumatical part, which is in all tangible bodies, and has some affinity with air, performs, after a sort, the office of the air. Thus the sound of an empty barrel is in part created by the air on the outside, and in part by that in the inside; for the sound will be less or greater, as the barrel is more or less empty; though it communicates also with the spirit in the wood, through which it passes from the outside to the inside.'—Sylva Sylvarum, Phonics, § 2.

‡ 'And though some famous authors have affirmed it impossible to hear through the thinnest plate of Moscovy glass, yet I know a way by which 'tis easy to hear one speak through a wall a yard thick. It has not yet been thoroughly examined, how far otacoustics may be improved, nor what other ways there may be of quickening our hearing; or conveying sounds through other bodies than the air; for that that is not the only medium I can assure the reader, that I have, by the help of a distended wire, propagated the sound to a very considerable distance in an instant, or with as seemingly quick a motion as that of light; at least, incomparably swifter than that which at the same time was propagated through the air; and this not only in a straight line, or direct, but in one bended in many angles.'—Preface to Hooke's 'Micrographia.'

Other experiments of a similar nature were subsequently made by Herhold and Rafn*, Hassenfratz and Gay Lussac †, &c.; but the first direct observations of the actual velocity of sound through solid conductors were made by Biot, assisted at different times by Bouvard and Martin. These experiments were made on the sides of the iron conduit-pipes of Paris, through the length of 951m. 25; and the mean result of two observations made in different ways gave 3459 metres, or 11,090 feet per second, for the velocity of sound in cast iron‡.

Previously to these last-mentioned experiments, Chladni had, in an ingenious manner, inferred the velocity of sound in different solid substances; and his results are fully confirmed by calculations from other grounds. His method was founded on Newton's demonstration, that sound travels through a space of a given length, filled with air, in the same time that a column of air of the same length, contained in a tube open at both ends, makes a single vibration||. His own discovery of the longitudinal vibrations of solid bodies, which are exactly analogous to the ordinary vibrations of columns of air, enabled him to apply this proposition to solid bodies, and to establish the general law, that sound is propagated through any elastic substance in the same time in which this substance makes one longitudinal vibration. In this manner he ascertained the velocities of sound in the following substances, among others: tin 7,800, silver 9,300, copper 12,500, glass and iron 17,500, and various woods from 11,000 to 18,000 feet in a second.

From the experiments of M. Perolle§, it would appear that the intensity with which sound is communicated through solid matters is nearly in proportion to the velocity of its transmission.

† Annales de Chimie, tome iii., p. 64.
‡ Mémoires de la Société d'Arcueil, tome ii., p. 403.
|| A single vibration is here considered as the motion of the vibrating body between the two opposite limits of its excursion, and with this signification the expression is adopted by Chladni. Other authors, however, regard this, with Newton and Sauveur, as a semi-vibration, and call an entire vibration the motion of the vibrating body from one limit of its excursion until it again arrives at the same limit. This difference of meaning attached to the same term has given rise to several mistakes.
§ Journal de Physique, tome xlix., p. 382.
$\S$ 2.

In all the experiments above alluded to, the sounds transmitted were either mere noises, such as the blow of a hammer, or, as in Herhold and Rafn's experiment, a single musical sound, produced by striking a silver spoon attached to one end of the conducting wire; and in no case were any means employed for the subsequent augmentation of the transmitted sound. I believe that, previous to the experiments which I commenced in 1820, none had been made on the transmission of the modulated sounds of musical instruments; nor had it been shown that sonorous undulations, propagated through solid linear conductors of considerable length, were capable of exciting, in surfaces with which they were in connexion, a quantity of vibratory motion, sufficient to be powerfully audible when communicated through the air. The first experiments of this kind which I made were publicly exhibited in 1821, and notices of them are to be found in the Literary Gazette, Ackerman's Repository, and other periodicals of that year. On June 30, 1823, a paper of mine was read by M. Arago, at the Academy of Sciences in Paris, in which I mentioned these experiments, and a variety of others relating to the passage of sound through rectilinear and bent conductors*. I propose, in the present instance, to give a more complete detail of these experiments than I have yet published; and at the same time to add what additional facts my subsequent experience has furnished me with on the same subject.

$\S$ 3.

Before proceeding any further, it will be necessary to make a few observations on the augmentation of sound which results from the connexion of a vibrating body with other bodies capable of entering into simultaneous vibration with it. This participation of the vibrations of an original sounding body is called resonance, or reciprocation of sound.

Sonorous bodies are audible (the extent of their excursions being supposed equal) in proportion to the quantity of their vibrating surfaces. Thus, a plate of glass or metal is capable

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of producing powerful sounds without accessory means; but
the sound of vibrating bodies of smaller dimensions, such as
insulated strings, or tuning-forks, are scarcely audible at a
moderate distance from the ear; but the sounds of the latter
are capable of considerable augmentation when communicated
to surfaces, as when they are placed to a table, or to the
sounding-board of a musical instrument.

There are several circumstances which influence the inten-
sity of the resonance of a sounding-board. The principal of
these is the plane in which the vibrations of the sounding body
are made with respect to the reciprocating surface. Thus, its
vibrations may be so communicated as to be perpendicular, or
normal to the surface, in which case the sound is the most
greatly augmented; or they may be tangential to, or in the
same plane with the surface, when the sound is the most feeble.
The first of these cases may be illustrated by placing a vibrat-
ing tuning-fork perpendicularly to the surface of a flat board;
and the second, by placing it perpendicularly to one of the edges
of the board. In intermediate positions—viz. when the vibra-
tions are communicated obliquely to the surface—the sound
will be found to have intermediate degrees of intensity.

These facts, which the extensive investigations of Savart
place in full evidence, being understood, the peculiarities of
the sounding-boards of various musical instruments admit of
easy explanation.

The sounding-board of the piano-forte is better disposed
than that of any other stringed instrument, as the planes of the
vibrations of the strings are, on account of the direction in
which they are struck by the hammers, always perpendicular
to its surface. The difference of intensity when a string
vibrates in this way, and when it vibrates parallel to the sur-
face, is very obvious, and may be easily tried by striking it
with the finger in these two directions*. There is no other in-
strument now in use, in which the strings make their vibrations
perpendicular to the sounding-board.

* It sometimes happens, when the impulse is oblique to the direction in which
the string presses on the bridge, that its plane of vibration assumes a rotatory
motion; the periodical changes of intensity thus occasioned, produce an effect
similar to that of the beating of imperfect unisons. This phenomenon is generally
erroneously attributed by tuners to a faulty string.
In the guitar, lute, &c., the strings are also parallel to the sounding-board, but the vibrations must, for the convenience of performance, be made obliquely to it. If the sides of the instrument be of inconsiderable depth, and the back be flat, the difference of intensity between the perpendicular and oblique vibrations will be very sensible. But if the sides be deep, very little difference will be perceived, as the vibrations which are tangential to the front sounding-board are perpendicular to the sides, which thus enter readily into normal vibrations; this fact may be proved by placing the ear to the side of a guitar while a string is made to sound with its plane of vibration successively parallel and perpendicular to it. In some instruments, as the lute, mandoline, &c. the back is polygonal or curved; by this construction a considerable portion of the resonant surface enters into normal or nearly normal vibrations when the strings are struck obliquely to the principal sounding-board.

These laws are not so immediately applicable to the violin, and other instruments of the same class; an extensive series of experiments will yet be necessary to enable us to account for the peculiarities of their forms, their various curvatures, and the functions of that irregular conductor, resting on the sounding-board at two points only, which in these instruments is called the bridge. The investigations of Savart still leave much to be desired on this head.

In no instrument are the strings perpendicular to the sounding-board; for in such case, however a string were made to vibrate, its communicated vibrations would be tangential. But they are sometimes placed obliquely, as in the harp, and then the same changes of intensity may be observed as when the strings are parallel to the board; for if the plane of their vibrations coincide with that of the inclination of the board, the communicated vibrations of the board will be oblique to its surface, and the intensity will be at its maximum; but if they be perpendicular to this plane, the communicated vibrations must be tangential to this surface, and consequently the intensity will be at its minimum.

Besides the proper adaptation of sounding-boards, there are other circumstances on which the tones of a stringed instrument
materially depend; one of the most important of these is, the proper dimensions of the volume of air contained within the sides; the laws of these resonant cavities have occupied the attention of Savart, but the obvious use of the bars placed within these cavities to divide the mass of air, and thus to enable it to vibrate more readily in separate portions, seems to have escaped his notice.

§ 4.

In the piano-forte, the guitar, &c., the ends of the strings are not in immediate contact with the sounding-board, but they rest on bars of wood, which are called bridges, through which the vibrations are communicated to the board. In these instruments the bridge is usually about half an inch in height, and in the violoncello does not exceed three inches. To ascertain how far the distance might be extended between the string and the sounding-board of a piano-forte without injury to the tone, I substituted a glass rod five feet in length for the bridge, and by placing at its end a string stretched on a steel bow, I found that the sound of the string was as distinctly audible as when it was immediately in contact with the board; a tuning-fork placed at the end of the rod gave the same result. These experiments, which were the first I made on the subject, and which suggested all the subsequent ones, have been repeated in the theatre of the Royal Institution on a larger scale. A series of connected deal rods, forty feet in length, was suspended so as to extend, in a straight line, obliquely from an open window of the cupola, to within a short distance of the floor of the room; on the upper end of this conductor, an assistant placed the stem of a vibrating tuning fork; when no sounding-board was placed at the lower extremity of the conductor, no sound was heard, but it became powerfully audible the instant the communication was made: this experiment was repeated with different acute and grave toned tuning-forks, employed both in combination and in succession.

Tuning-forks are the most convenient instruments for making experiments on the transmission of sound, because their vibra-
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tions are almost inaudible by themselves, and only become strongly audible when augmented by resonant surfaces.

§ 5.

The vibrations of the sounding-board of any stringed instrument may be communicated in the same manner as those of a string, or of a tuning fork, to a distant sounding-board by means of a metallic, glass, or wooden conductor; but in this case it is necessary to prevent the original sounds from being heard through the air, otherwise the communicated sounds will not be distinguishable from them. This may be effected by placing the originally vibrating, and the reciprocating instruments in different rooms, and allowing the conductors to pass through the floor or wall separating the two rooms.

In the passage of the conducting-rod or wire through these partitions, care must be taken to prevent its touching their sides; for this purpose, a tin tube, covered at its two ends with leather, or India rubber, may be inserted in the partition, and the conductor be made to pass through holes in these coverings, so as not to touch the side of the tube.

A square piano-forte is a very convenient instrument to employ in these experiments. If the sound is to be transmitted upwards, nothing more is requisite than to open or remove the lid of the instrument, and to allow the conductor to rest upon the sounding-board. A metallic wire is not sufficiently rigid to support itself thus without bending; a rod of some straight-fibred wood, as lancewood or deal, is therefore better adapted for this form of the experiment; the lower end of the rod must be reduced in thickness, so as to allow it to pass between two adjacent strings; and the best place to make the contact will be found to be about a quarter of an inch from the bridge, among the middle notes, and on the side occupied by the unvibrating portions of the strings. The reciprocating instrument in the room above, may be a guitar or any other similar instrument, or a harp; in which latter case, the rod may be brought in contact with the inner surface of the belly of the instrument, through one of the apertures of the swell. These were the forms under which the experiments have been repeated at the Royal Institution.
If the sounds of the piano-forte are to be transmitted downwards, a brass wire, about the thickness of a goose-quill, will suffice for the communication, as the weight of a reciprocating instrument suspended from it below will keep it sufficiently straight. To bring the conducting-wire into contact with the under surface of the sounding-board of the piano-forte, an aperture must be made in the bottom of the instrument immediately below the intended point of contact; and to ensure a perfect connexion with the sounding-board, it is advisable to furnish the wire with a shoulder just below its entrance into the aperture, and to occasion an upward pressure on this shoulder, by a piece of leather stretched on a ring (as in the insulating-tube above described) and placed at the end of a strong steel spring; the other end of which is screwed firmly to the bottom of the instrument. To assist in supporting the wire, another shoulder may be made on it, so as to rest upon the upper covering of the insulating-tube which passes through the floor; and the reciprocating instrument may be suspended by inserting the end of the wire into the sounding-board, and then securing it by a nut and screw on the opposite side. The form of the resounding instrument is a matter of choice; but, in order to obtain the freest and loudest tones, it is requisite to have the principal vibrating surface perpendicular to the conducting-wire. It is instructive to observe the gradual changes in the intensity and the quality of the transmitted sounds, when the sounding-board is made to pass through the various degrees of obliquity from a perpendicular direction to the conductor, until it is in the same plane with it; or, to employ Savart's language, as the communicated vibrations change from normal to tangential; in the latter case, the sounds have a subdued, and what is ordinarily called a metallic quality. In the first public experiments I made in 1821, the reciprocating instrument, which was the representation of an ancient lyre, was so constructed as to produce tangential vibration; the tones were consequently far inferior to what I have since been able to produce. The transmitted sounds are not sensibly impaired when the wire is separated at several places, and the disunited parts fastened together by mechanical contact; the annexed wood-cut represents the divisions of the conducting-
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wire, which I found it convenient to make in the original form of the experiment, for the sake of portability and facility of removal; but, if the apparatus be intended as a fixture, it will be easier and better to employ but one length of wire. The wire consisted of four portions: the first part touched the sounding-board of the piano-forte, and reached half-way to the floor; the second passed through the insulating-tube in the floor, and terminated when it reached the ceiling of the
room below in a hook; a third part was suspended from this hook by a loop; and the fourth, after identifying itself with one of the apparent wires of the lyre, passed within the instrument, and was ultimately fixed, at its lower end, to the point marked at the end of the dotted line on the sounding-board; each of the disunited parts were allowed to overlap each other at a and b, and were fastened together by means of a clamp with a screw-nut. The whole apparatus thus prepared may be easily removed; the clamps being unscrewed and the resounding instrument removed, the lower wire must be unhooked from the ceiling, the hook unscrewed, and the middle wire withdrawn from the insulating-tube: the time for fixing or removing the apparatus need not exceed a few minutes.

From what has preceded, it will be obvious in what manner two square piano-fortes or two harps may be so connected as mutually to reciprocate each other's sounds; by such an arrangement, two performers in different rooms may play a duet together to two distinct audiences, or one may echo the performance of the other. If the transmission is required to be horizontal, i.e., between two rooms on the same floor, cabinet piano-fortes must be employed.

The sounds of an instrument may be at the same time transmitted to more than one place; for instance, communications may be made from a square piano-forte to a resounding instrument above, and to another below; and the communication may be even continued through a series of reciprocating instruments. If the instruments be not in adjacent rooms, but be further removed from each other, a person in the intermediate room, through which the conductor passes, will hear no sound but what is communicated by the ordinary means. Hence it would be possible to extend a horizontal conductor through a series of rooms belonging to different houses, and (provided the instrument connected with one of its extremities be constantly played upon) to hear at pleasure the performance in any of these rooms, by merely attaching a reciprocating instrument to the conductor; on removing this instrument, the sonorous undulations would pass inaudibly
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to the next apartment. These observations will equally apply to the transmission of other musical sounds, which will be hereafter noticed (§ 6, 7).

§ 6.

The transmission of the sounds of those stringed instruments which produce sustained sounds, as the violin, violoncello, &c., is equally effective. The conducting-rod may be applied either to the back or the front of the instrument; no precise directions can be given with respect to the points at which the contact should be made; but, in general, the effect has appeared to me better when the end of the conductor has not been too far removed from the situation of the sound-post.

§ 7.

I have been able to effect the transmission of the sounds of reed wind-instruments through solid conductors as perfectly as that of instruments dependent on the vibrations of sounding-boards. In the clarionet, or any other reed instrument, the column of air and the vibrating tongue (or reed) mutually influence each other in such a manner, that whether the sounds be communicated to the atmosphere from the column of air, or to a solid conductor from the vibrating tongue, the quality (timbre) of the sound undergoes no change.

To connect the conducting wire, which may be of brass, and about a tenth of an inch in diameter, with the tongue of the clarionet, the end of the wire must be bent for about a quarter of an inch, and then filed flat on both sides. This flattened end must be fastened to the fixed end of the tongue by the silk wrapping which usually fastens the tongue only, and the angle of the bend be adjusted so as to suit the position of the performer. If the sound is to be transmitted downwards, the embouchure of the clarionet must be placed in the performer's mouth in the usual way, viz. the tongue of the reed resting on the under lip; but if the sound is to be transmitted upwards, the performer must play, as some eminent masters of this instrument do, with the tongue applied to the upper lip. For the bassoon or the hautbois, it is equally convenient to the
Mr. Wheatstone on the performer, whether the wire be applied to the reed above or below.

The resounding instrument may, as in the experiments above detailed, be either a harp, a piano-forte, or a guitar. It is a singular effect to hear the sounds of a wind-instrument thus reproduced by a sounding-board.

§ 8.

The experiments I have made with respect to other classes of wind-instruments have not been equally successful. It is not possible to communicate the vibrations of the air to a solid conductor without an enormous loss of intensity: if, however, the intermediation of other bodies which enter readily into vibration, from the agitations of the air, be employed, the transmission may in some measure be effected. Thus, if the end of the conducting-wire be placed in the most strongly vibrating part of the column of air in a flute, there is but little perceptible transmission of sound; but if the wire touch the side of the instrument, it will more readily transmit the sounds, as the side is susceptible of entering into vibration. Even in this latter case, the sounds are scarcely audible, unless the ear be held close to the resounding instrument.

In a similar manner, the sounds of an entire orchestra may be transmitted, viz. by connecting the end of the conductor with a properly constructed sounding-board, so placed as to resound to all the instruments. The effect of an experiment of this kind is very pleasing; the sounds, indeed, have so little intensity as scarcely to be heard at a distance from the reciprocating instrument; but on placing the ear close to it, a diminutive band is heard, in which all the instruments preserve their distinctive qualities; and the pianos and fortés, the crescendos and diminuendos, their relative contrasts. Compared with an ordinary band, heard at a distance through the air, the effect is as a landscape seen in miniature beauty through a concave lens, as compared with the same scene viewed by the ordinary vision through a murky atmosphere.
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§ 9.

In the preceding experiments on the transmission of sound through solid bodies, the conductors have been represented as straight; but, though sound is transmitted the more readily through straight conductors, it will yet pass, though with diminished intensity, through rods with angular and curved bendings. If a vibrating tuning-fork be placed at one end of a straight brass rod, the other end of which rests perpendicularly upon a sounding-board, the vibrations will, in accordance with what has been above stated, be powerfully transmitted; on gradually bending the rod at any part of its length, while the vibrations of the tuning-fork are kept in the same plane with the angle of the bent rod, the transmitted sound will progressively decrease in intensity, and will be very feeble when the angle has become a right one: as the bending is continued so as to make the angle between the two parts of the rod more acute, the intensity of the sound will increase in the same order in which it had before diminished; and when the two parts of the rod are nearly parallel, the sound will be nearly as loud as when the transmission was rectilineal. If, during the gradual bending of the rod, the plane of the vibrations of the tuning-fork be perpendicular to the plane of the angle made by the two parts of the rod, the same changes will be observed, but in a more obvious manner, than in the former case; and when the angle becomes a right one, the sound will be scarcely perceptible. At intermediate inclinations of the two planes, the gradations of intensity, occasioned by the bending of the rod, will be found to be intermediate.

The changes of intensity dependent on the variation of the angle of the two planes may be instructively shewn by bending the rod permanently to a right angle, and placing, as before, the stem of a tuning-fork so as to form the prolongation of one of the parts of the rod, the other part of the rod resting on the sounding-board. On gradually turning the tuning-fork round the axis of its stem, without inclining it to the rod, the plane of the vibrations will assume every angle with respect to the plane in which the two parts of the rod is bent. During the revolution it will be observed, that when the planes coincide
the intensity will be at its maximum, and when they are perpendicular to each other, at its minimum; thus, supposing the sound to commence when the two planes are parallel, it will gradually diminish until they make an angle of $90^\circ$; it will then increase through the same changes of intensity, in an inverted order, until it acquires its maximum at $180^\circ$; it will again decrease between this and $270^\circ$, and increase until it arrives at its first position at $0^\circ$. If the stem of the tuning-fork be placed perpendicularly on the side of a conducting-rod resting on a sounding-board, the same phenomena may be observed; the stem of the tuning-fork is, in fact, a short conductor, forming a right angle with the rod.

Were it necessary for the transmission of sound that the undulations should propagate themselves only rectilinearly, it is obvious that they would not pass through a bent rod; and, on the other hand, had they the property of diffusing themselves equally in all directions, we should not observe any differences of intensity in the experiments above noticed. These experiments lead us to conclude, that sound diffuses itself in all directions, though unequally; that it is communicated more readily in the plane in which the original vibrations are made, and with the greatest degree of intensity in the direction of these vibrations.

§ 10.

In most of the experiments relating to the transmission of the sounds of musical instruments, which I have in the preceding paragraphs detailed, the conductor has been represented as receiving its impulses from a surface vibrating normally, to which it was perpendicularly attached; the communication was consequently effected by longitudinal undulations in the conducting wire. But if, while the conductor retains its position, the surface were to vibrate tangentially or obliquely, the communication would be effected by transversal or oblique undulations.

In practice it is preferable to employ the longitudinal undulations for the purpose of transmitting musical sounds to a distance; for, firstly, the transmission is more efficacious; and, secondly, the transverse undulations have a great ten
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dency to communicate themselves laterally from the conductor to the surrounding medium, and thereby to become audible without the assistance of a reciprocating instrument. This lateral dispersion is scarcely observable with small conductors but is very obvious when a rod of considerable diameter is employed.

I had an opportunity of observing this fact while repeating some of the preceding experiments at the Royal Institution. A square piano-forte was placed in the apartment beneath the lecture-room; and a conductor, placed perpendicularly to its sounding-board, passed through the floor separating the two rooms, but no reciprocating sounding-board was placed at its upper end. By this arrangement, longitudinal undulations were communicated to the conductor; and, whether this was a brass wire one-tenth of an inch in diameter, or a square deal rod half an inch thick, the insulation appeared to be equally perfect. But it was not so when the conductor, instead of being placed on the sounding-board of a piano-forte, was made to rest on the top of the bridge of a violin, and the strings, put into vibration by drawing a bow across them, communicated transverse vibrations to the conductor; it was now observed, that the metal wire insulated the sound tolerably well, but that when the wooden rod was employed, the sound communicated to the air from the entire surface of the portion of the rod above the floor, was nearly as loud as if a sounding-board were placed at its extremity.

§ 11.

I have in this paper given the general results of a variety of experiments made at different and distant periods during the last ten years; but they are far from forming so complete a course as I have been desirous of making. To extend these experiments much farther would be attended with some difficulties:, but as the velocity of sound is much greater in solid substances than in air, it is not improbable that the transmission of sound through solid conductors, and its subsequent reciprocation, may hereafter be applied to many useful purposes. Sound travels through the air at the rate of 1142 feet in a second of time; but it is communicated through iron wire,
glass, cane, or deal-wood rods, with the velocity of about 18,000 feet per second, so that it would travel the distance of 200 miles in less than a minute.

When sound is allowed to diffuse itself in all directions as from a centre, its intensity, according to theory, decreases as the square of the distance increases; but if it be confined to one rectilinear direction, no diminution of intensity ought to take place. But this is on the supposition that the conducting body possesses perfect homogeneity, and is uniform in its structure,—conditions which never obtain in our actual experiments. Could any conducting substance be rendered perfectly equal in density and elasticity, so as to allow the undulations to proceed with a uniform velocity without any reflections or interferences, it would be as easy to transmit sounds through such conductors from Aberdeen to London, as it is now to establish a communication from one chamber to another. Whether any substance can be rendered thus homogeneous and uniform remains for future philosophers to determine.

The transmission to distant places, and the multiplication of musical performances, are objects of far less importance than the conveyance of the articulations of speech. I have found by experiment that all these articulations, as well as the musical inflexions of the voice, may be perfectly, though feebly, transmitted to any of the previously described reciprocating instruments by connecting the conductor, either immediately with some part of the neck or head contiguous to the larynx, or with a sounding-board, to which the mouth of the speaker or singer is closely applied. The almost hopeless difficulty of communicating sounds produced in air with sufficient intensity to solid bodies, might induce us to despair of further success; but could articulations similar to those enounced by the human organs of speech be produced immediately in solid bodies, their transmission might be effected with any required degree of intensity. Some recent investigations lead us to hope that we are not far from effecting these desiderata; and if all the articulations were once thus obtained, the construction of a machine for the arrangement of them into syllables, words, and sentences, would demand no knowledge beyond that we already possess.
ON THE INFLUENCE OF THE 'SENSE' OF MUSCULAR
ACTION IN CONNEXION WITH VISION.

BY ALEXANDER SHAW, Esq.

To those philosophers who have studied the subject of
sensation, the organ of vision has ever been the most
attractive; and yet the opinions of men, whose attainments
we must respect, differ very widely as to the mode of opera-
tion of this external organ of sense.

Dr. Brewster, in his 'Treatise on Optics,' which is the
latest publication on this subject, has introduced an explana-
tion of the problem—Why an inverted image on the retina
should give us the idea of an erect object. This question, as
he himself has remarked, has been a frequent cause of per-
plexity to the learned; but I am of opinion that no theory of
vision can be admitted to be correct, which does not afford a
satisfactory explanation of it.

This problem involves certain physiological principles,
which are not generally understood, but upon which the doc-
trines of perception through the organs of the senses, espe-
cially the eye, have the most intimate dependence. As the
solution, which Dr. Brewster has adopted, is unconnected,
and, indeed, altogether at variance with these, it becomes ne-
cessary to examine it with care; and I think it can be shown,
notwithstanding the confident tone in which he has expressed
himself, and the high authority which he deservedly holds in
questions of optics, that the explanation which he has offered
is liable to many powerful objections. It is the more im-
portant to undertake this examination, since the view which
he has presented may be considered as that which generally
prevails. It is the same explanation which was originally pro-
posed by Dr. Porterfield in the 'Medical Essays,' and Dr.
Reid, when treating of the mode of operation of the organ of
vision in his 'Inquiry into the Human Mind,' has explained
it upon the same grounds.

These writers have all agreed in representing that the idea
of the 'direction' in which objects are seen is obtained imme-
diately from the retina or nerve of vision. They suppose that
this nerve can convey to the mind a sensation of the course in
which the rays have proceeded from the object to impinge upon it; or, in other words, that the retina can receive an impression, not only of the object, but of the direction in which the object is presented to it. Founding upon this as a true position, they find it easy to frame what they consider to be a just explanation of the problem. In an inverted image, they assert, the retina does not convey the impression of its particular parts being inverted; but each point in the image is judged to be in the direction in which the rays have proceeded in falling upon it; the uppermost pencil of rays from the object, falling upon the lowest part of the image, gives the sensation of its proceeding from the highest part, and consequently makes that part appear to be at the top, instead of at the bottom; and so, they say, it holds with regard to the lowest rays and all the others. To use Dr. Brewster's words, the retina 'sees along the lines of visible direction;' that is, the lines which lead from the image in the direction of the object.

Such is the leading proposition on which the whole theory is rested. But it is surely an error to assume that the retina possesses such a power as is here attributed to it. If we ask—What is the meaning conveyed by the words 'visible direction'?—it will be seen, on reflection, that they include something more than a simple sensation obtained through an organ of sense. To acquire the idea conveyed by the term 'direction' alone, it is necessary that there should be a comparison; that is to say, an operation of the mind itself. We can only form the idea of the particular quarter or situation in which a body is placed, by informing ourselves of its position in regard to another, which has been previously fixed upon as the standard of our comparison. To say, then, that our knowledge of 'direction' can be obtained at once, and can be conveyed to the sensorium like an impression through the optic nerve, is to employ the term in a vague and loose manner, which must necessarily lead into error.

It is contrary to all analogy to attribute to a single nerve, as the optic, the possession of such incongruous powers as this theory assumes. Allowing that the idea of 'direction' could be conveyed to the mind through the medium of a nerve, it would follow, if this theory were correct, that the optic nerve was not only sensible of the relative position of an object, but
that it had, at the same time, the sensation of the variations of light which distinguish that object from others placed around it. The usual expression of Dr. Brewster, that the image on the retina is 'seen along' a certain line, implies this: for these two words signify that the retina, besides discovering that the object is red, blue, or yellow, determines that it is placed to the right or to the left, or above or below, and also the exact line or degree in which it is so placed.

The main error, which has misled the numerous writers who have treated of this question, and which has created the degree of puzzle that seems always to have been attached to it, may be traced to this—that they have invariably sought to solve the problem by a reference to the functions of the optic nerve alone; they have looked upon the globe of the eye and this nerve as constituting the entire instrument of vision; without taking into consideration the apparatus of muscles, and their nerves, which, under the guidance of the will, move and direct the eyeball from point to point. It can only have been from taking this partial view of the organ of vision that they could attribute to the optic nerve such complicated and inconsistent functions as those which have been bestowed upon it.

The explanation of the problem—Why an inverted image should give the idea of an erect object, which has been adopted by Dr. Brewster, is founded upon the law which has been called the 'law of visible direction.' This supposed law, as the words themselves imply, includes the opinion that seeing and the power of distinguishing the direction of objects are possessed by the retina together; and the following passage, taken from the 'Treatise,' will serve to exhibit the nature of the proofs upon which it rests.

'On the Law of Visible Direction.—When a ray of light falls upon the retina, and gives us vision of the point of an object from which it proceeds, it becomes an interesting question to determine in what direction the object will be seen, reckoning from the point where it falls upon the retina. In fig. 142, let F be a point of the retina, on which the image of a point of a distant object is formed by means of the crystal-line lens, supposed to be at L'L. Now the rays which formed
the image of the point at F, fall upon the retina in all possible directions from L' F to LF; and we know that the point F is seen in the direction FCR. In the same manner the points ff' are seen somewhere in the directions f' S, f T. These lines, FR, f' S, f T, which may be called the lines of visible direction, may either be those which pass through the centre C of the lens L' L, or, in the case of the eye, through the centre of the lens, equivalent to all the refractions employed in producing the image; or it may be the resultant of all the directions within the angles L'FL, L'f'L; or it may be a line perpendicular to the retina at Ff'f. In order to determine this point, let us look over the top of the card at the point of the object whose image is at F, till the edge of the card is just about to hide it; or, what is the same thing, let us obstruct all the rays that pass through the pupil excepting the uppermost RL'; we shall then find that the point whose image is at F, is seen in the same direction as it was seen by all the rays L' F, C F, LF. If we look beneath the card in a similar manner, so as to see the object by the lowermost ray RL F, we shall see it in the same direction.

It is remarkable that Dr. Brewster, in conducting an argument of this nature, should have fallen into two such errors as are here exhibited.

1. He endeavours to demonstrate how the retina can discover the relative direction of an object, and yet he omits altogether to present any second object or image to our notice. In each of the three instances which he has given, he has confined his attention solely to the object whose direction forms the question at issue, or to the rays which come from it, or to
the image formed by it upon the retina. But it is obvious that from such a mode of proceeding no conclusion could ever be drawn. There ought to have been some standard, or some fixed point introduced, by which to calculate the comparative direction of the object; or to enable us to estimate the truth of the demonstration, by contrasting one line of visible direction with another.

2. But it is in his experiment that the most remarkable error is found: indeed, the conclusion which he has drawn from it must have somewhat startled the reader. He affirms that if the rays RC RL, proceeding from a distant point, be cut off by a card (which may be represented by AB), and the ray RL', alone enters within the eye, the image F will be seen along the line FCR. But how can this, by any possibility, be the case? Is it not obvious that the card AB totally obstructs vision in that direction? if we 'see along' the line, it can only be by seeing through the card! An explanation seems to be required how Dr. Brewster could have been brought to make so extraordinary a statement. The error can only have been occasioned by his proceeding on a wrong path in making this inquiry. Unexpected conclusions are often forced upon us in the study of mathematics; and even when a strict course of inductive reasoning is pursued in physical subjects, the results are often strange, and apparently paradoxical: so that we need not be surprised that a learned philosopher should occasionally yield his assent to a proposition which an uneducated person, guided solely by his common sense, would at once reject. But it does not appear that the conclusion which Dr. Brewster has drawn, in the present instance, is to be justified on the grounds of its being founded either on mathematical demonstration, or on correct induction. In explaining the result of his experiment, he has mixed up with the facts a bare and unwarranted assumption regarding the power which belongs to the retina.
If the experiment with the card be made as he has directed; it will be found to be true that the image F retains the same position, whichever ray, proceeding from the object, is permitted to enter the eye; and it appears in the same place, whether all the rays, or only one be admitted. But this by no means leads to the conclusion that the direction of the object is ascertained by the retina pursuing the object along any particular line. It shows only, that the same sensation is excited whether a ray falls obliquely upon the retina, either from above or from below, or falls upon it in a perpendicular line; and that, therefore, no contrast can be made between them. It ought rather to have been noticed that all the rays from the external object concentrate towards a single point in the retina F—that each individual ray, however separated from the others in its course, must proceed from the same point in the object, and affect the same spot in the retina—that, consequently, no difference in the sensation is to be expected—and that it is altogether futile to look upon the direction of the several rays as leading to any knowledge of the place of the object.

It is not easy to understand what is meant by the expression 'seeing along' lines, when these lines stretch outwardly from the retina through the humours of the eye and through the atmosphere. Dr. Porterfield has said that 'by virtue of a connate and immutable law, the mind traces back its own sensation, and sees every point of the object, not in the sensorium or retina, but without the eye, in those perpendicular lines,' which Dr. Brewster has called 'the lines of visible direction.' This is supposing us to possess a power which reaches far beyond where the optic nerve is present to exercise it. It is also presuming that the mind has a consciousness of the outward object, distinct from that sensation which is conveyed by the image formed upon the nerve.

But it may be asked, how is the consciousness of the object being external obtained?—how do we acquire the knowledge of the simple fact, that the body, whose image is painted in the interior of our eye, is exterior to us? It is not through the medium of the optic nerve alone that this information is obtained. Our sole knowledge of the existence of an external body, so far as the opticerve is concerned, is acquired through
the rays which emanate from it. These rays, being reflected and dispersed to a distance, according to a law of nature, from all the surfaces of the body, pass into the eye, and form upon the retina an exquisitely minute image or copy of the object. Now it is this image alone which gives rise to the impression of the object in the mind. The outward body itself does not directly excite the nerve: we have to rely for the correctness of our knowledge respecting it, upon the image, formed by the rays, being a faithful representation of it. As the image, therefore, is at a distance from its original, and as it is perfectly distinct from it, how do we learn to associate it with the external object?—how do we discover that it is not an ocular spectrum, or a mere phantasm, that we see? for it is known that an impression may be made upon the retina, and remain there even while the object is removed altogether from our presence. We can only be assured that the image represents an object which is not placed within the eye, but is external to our body, by calling in the assistance of, at least, one other sense: more of these being brought in as evidences may strengthen our conviction; but one in addition is absolutely necessary. We have the means of ascertaining the fact which we desire, in the muscular apparatus that always accompanies the possession of an organ of vision.

The muscles have the power of turning the eyeball either towards the object or in a contrary direction. Of this we are conscious. Now, it appears to be a simple conclusion to arrive at—that the object must have a separate existence of its own, and distinct from the eye which perceives it,—when, in order to see the same object, we invariably find that it is necessary to exercise the muscles in a particular manner. We know that, if the body presented to our sight be in motion, as a bird flying through the air, we must follow it with our eyes, making fresh efforts to keep them in the direction of its flight, otherwise it will disappear. If the image were a mere spectrum, as that produced by looking at the sun, it would present itself in whichever direction we happened to turn our eyes. Hence it follows that, even without calling the sense of touch or any of the remaining senses into operation, but depending upon the knowledge acquired from merely shifting the eye about, we
become convinced that the body whose image is in the eye exists externally. To 'trace' or 'see along' a line, as the expression is used, includes the opinion that the object is placed externally; it likewise includes the idea of guiding or directing the eye; and, in addition to these, it implies the existence of a coloured image painted upon the retina. The notion conveyed by these words is, therefore, most complicated; and they ought never to have been applied, as has been done, in speaking of the functions of a single nerve.

Another inconsistency in the theory may be noticed. The theory rests upon the supposition that the retina can distinguish the direction of the object by seeing along lines which lead from the image to the external body. But it is incomprehensible how the nerve, which is seated at the bottom of the eye, should be able to ascertain, by itself, the direction of rays or lines which terminate in it. These rays of light are only sensible when they arrive at that point in the surface of the retina which is their final destination; and they are not recognizable by any other nerve of sense besides the optic nerve. Now, according to the most elementary definitions in mathematics, when we desire to learn the direction of a line, it is necessary that two points in it, at least, should be made known; but, according to the theory, it is the single point alone which terminates the 'line of visible direction' that is made sensible. If we could conceive that the line, in passing through the anterior surface of the eye, caused a sensation of the particular spot in the cornea where it entered, we might then have two points presented to the mind, by which to estimate the direction of its passage from the object to the retina: but, without such a second point, it appears quite impossible to ascertain its direction.

The question as to the manner in which the idea of the 'direction' of objects is obtained, is to be approached in a very different way than by attending to the rays which proceed from them: it requires a more complex operation of the senses to acquire this knowledge of surrounding objects than has been conceived. We have already seen that, in order to ascertain the simple truth that an object is situated externally, something more is necessary than a mere impression
made upon the optic nerve; and the same will be the case, it is natural to conclude, if we seek to discover its exact place in the external world. When we desire to find out the 'direction' of a body, our true purpose is to ascertain what is its position in relation to other bodies or to some fixed standard. It is sufficiently obvious that the object itself, if it were altogether detached from surrounding bodies, could never present to the mind this idea. To entertain the conception implied by the term 'direction,' it is required that other objects be presented to the sight besides that which is under our particular view; and to institute the proper comparison, or to form the necessary calculation, it is as indispensable that the sensations of these neighbouring objects should be communicated to the mind as that the sensation of the object itself should be so presented. However varied the direction of the rays proceeding from a single object may be when compared with one another, no reasoning upon them can ever acquaint us with the relative position of that body to another, whose rays are not given. Dr. Brewster has forgotten to supply any second body, or to represent its rays, although to establish a 'line of visible direction' the existence of some standard of comparison is necessarily implied.

When an object is presented before the eye, the surface of the retina is not occupied with it alone, but the whole surrounding scene, everything which is near it and can be included within the field of vision, is represented upon the nerve at the same time. Now, these are so many impressions by which the mind can judge of the relative position of objects; and so long as these find admission into the eye, data shall not be wanting for instituting the necessary comparison. If a diagram be drawn, representing the surrounding objects and their images falling upon the retina, it will be easy to understand how the mind acquires the knowledge of the direction of each of them. We may make the diagram correspond with the experiment of Dr. Brewster, as it will serve as well as any other to demonstrate what is required.

The card DE being placed before the eye, may prevent objects situated in that direction from casting their images upon the retina; but, according to the terms of the experi-
ment, it does not interfere with the ray $R L'$. This ray being admitted, is refracted to the same point, $F$, in the retina, to which all the other rays, if they had not been shut out, would also have concentrated. The ray $R L'$ being allowed to enter the eye, there is no reason why objects placed above the card should not also have their images represented upon the retina. Let $b$ and $a$ be such images, formed by the rays $B L$, $A L$. It is obvious that these preserve the same relation to the image at $F$ as they did before the card was used. Now, the true explanation of our seeing this, or, in other words, of our ascertaining any thing respecting the position of $F$, is this: there is, first, an impression of the image $F$ conveyed through the optic nerve to the sensorium; then the image $b$ is presented to it; and, in succession, the image $a$ comes before it,—a process of comparison, which the mind alone can perform, is instituted between these various images, and the result is the knowledge of the relative direction of $F$ to $b$ and $a$.

It is thus that the surrounding bodies, as well as the particular object of sight, require to be included within the sphere of observation, in order to obtain the idea of 'direction.' As to the intermediate process by which these objects are successively presented to the mind, this is a question which embraces subjects of the highest interest.

I would remark, in the first place, that the eye is not a fixed and motionless instrument, and the retina is not possessed of an uniform degree of sensibility throughout all its surface, both of which things the theory which I have been considering would seem to imply. The retina has one spot in its surface,
situated in the axis of vision, which is more acutely sensible to the rays of light than any other part; and the eyeball is admirably provided to turn and present its transparent surface so that it may catch the impressions of objects upon this spot. It is only when images fall upon this part of the retina that the mind is satisfied, or that a perfect sensation is obtained; and the power of guiding the eye with this intention, whether it be through the motions of the body or of the head, or is produced directly by the action of the muscles of the eye, is as necessary to the perception of the direction of the things around us, as the groping about with the hands is to a blind person, to enable him to find his way through the confused furniture of a room. We are sensible of this searching motion of the eye, made in preparation for distinct vision; and it is by calculating the extent of this motion that we estimate 'direction.'

In common language we are accustomed to speak of 'directing' the eye: it is allowed also, that it is by a voluntary act that we accomplish this; but it is not so generally conceded that the exercise of the muscles, by which the eye is moved, communicates a distinct sensation to the mind; and yet it forms an important part in the process of vision. When an astronomer directs his telescope to the heavens, he is aware that his instrument combines in it two separate means of acquiring the knowledge which he seeks. By the apparatus of lenses or mirrors inclosed within the tube, he ascertains the magnitude, brilliancy, or colour of the star; but it is by a perfectly distinct apparatus that he determines its place in the heavens. He consults, for this purpose, the external parts of his instrument, such as the various levers, screws, and joints by which the tube is revolved; and it is by looking to the scale attached to his telescope, that he is informed of the exact bearing or of the direction of the particular star. Now the outward apparatus of muscles by which the eyeball is directed in vision, is surely, in an inquiry of this kind, deserving of attention. There is a sensibility resident in the muscles which gives token of the degree of their contractions; and it is by attending to these impressions, like looking to the scale of the telescope, that we become conscious of the direction of objects.
This sensibility to the exercise of the muscular frame is independent of sight, or any of the other organs of the senses; it is altogether a distinct source of sensation to the mind. If a person were blind, or completely isolated from all outward objects of sense, he would still be sensible of the motions of his arms, or of his body, or of his eyes; and if a standard of comparison could only be communicated to him, he would be able to tell in which direction and to what extent he moved them. Although it has been only lately recognized that a 'muscular sense' exists in the body, yet it appears to be as distinctly one as any of the five senses, smelling, seeing, hearing, taste, or touch. Anatomy discloses to us that nerves whose office it is to convey sensations merely, and which are incapable of influencing the muscles to contract, are distributed with profusion to all the voluntary muscles throughout the body. These sensitive nerves of the muscles, it has been concluded by Sir Charles Bell, convey to the brain the consciousness of the contraction of the muscles which have been previously excited through the proper nerves of motion. They establish a communication, as it were, in a circle, between the muscles and the sensorium, whose office it is to regulate the extent of the action of the muscles; and they carry to the mind that knowledge of the condition of the muscles, without which their actions could neither be controlled nor adjusted, nor be under the guidance of volition.

Let us observe what takes place during vision, and we shall perceive how intimately this 'sense' of the muscular actions is connected with the question before us. When we look at an object placed high above us, the first thing which we naturally do is to throw back the head, to turn the face towards the skies, to elevate the upper eyelids, and to raise the cornea of both eyes upwards. Now these actions are not performed without our knowledge. If we have to inspect an object which is placed on one side, we may be obliged to wheel round before we can see it; at all events it will be necessary to turn the eyeball in the socket towards that side. If we have to examine an object placed at our feet, there is first a corresponding motion of the eyeball and of the head, or it may be of the whole body, in order to be enabled to look downwards.
These motions of the frame, accompanying vision, are familiar to all persons. We can even tell, at a distance, in what direction a person is looking, by observing the position of his body; and if we can see his eyes, we may tell whether he is looking at ourselves, or the particular spot that engages his attention. The boxer or the fencer knows full well how much the motion of the eye has to do with seeing; for it is by watching keenly the eye of his adversary, that he learns the exact place where the blow is to be struck, and can parry it. There is invariably associated, therefore, with seeing, a particular position of the organ of vision; and if it be allowed that we possess the consciousness of this position of our organ, it must, I think, be concluded that this is the source of our ideas of direction. We contrast the position of the eye necessary for seeing one object with distinctness, with that which is required for seeing another. Certain standards of comparison are arbitrarily fixed upon, and it is by referring to these that we assert that an object is placed high or low, to one side or to the other. If we turn our eyes upwards from the ground, we say that the object is high; if we direct it downwards, we say that it is low; and, in the same manner, we say that it is placed to the right or to the left side, according to the direction in which the eyeball is revolved when looking upon it.

Thus it would appear that the motions of the body and of the eyeball together, constitute an important part in our perception through the organ of vision. The consciousness of the action of the muscles accompanies the sensation which the retina bestows; and it is the almost simultaneous reception of these two different kinds of sensation, added to the effects of early habit in associating them, that gives rise to the common feeling of their both being obtained from the exercise of the same sense.

If we now apply this view of the manner in which the 'direction' of objects is discovered, to the problem of 'erect vision from an inverted image,' it will afford an easy explanation of it. I ought first to state, that there are no reasonable grounds for the notion, that an inverted image upon the retina must necessarily be attended with an impression of the object, which is looked at, being also inverted. The opinion has been held,
that originally, as during infancy, all objects are seen inverted, and that some process is required to correct this false impression. But there appears to be no reason for entertaining such a conception. The connexion established between the image upon the retina and the mind which receives the sensation, is altogether so incomprehensible, that no distinction can be supposed to depend upon the image being either inverted or erect. There would be an absurdity in speaking of the image being inverted in reference to the mind, which is incorporeal, as we speak of it being inverted in reference to the eye or the external object; and the process of sensation would not be a whit more intelligible, if the image were placed erect instead of being inverted. It is more just to believe that the image, of itself, can give no impression whatever of the position of the object, but only those sensations which proceed from light, as the varieties of colour, brightness, shadow, outline, &c. The question is simply, How does the idea of direction first enter the mind?—how do we ascertain that the base of an object is placed towards the ground, and its top towards the skies? And this question may be considered as one altogether independent of the position of the image at the bottom of the eye.

If we proceed upon the principle which has been stated above, the answer to this question must be—that we judge of the direction of the various parts of a body by ascertaining in what position we must place the eye, in order to see it distinctly. When a tree, for example, is presented to our view, we direct the eyes downwards to observe its trunk rooted in the earth—we turn them upwards to see its uppermost branch; and we turn them to each side to see the right and left sides of the tree: and it is by referring to these motions that we conclude that one part is above, or another is below. In all of these motions, however rapidly performed, a distinct sensation accompanies the change; and this is communicated to the mind as surely as is the impression upon the retina itself. If we sought for an analogy, we might find it in the hand; for the law is exactly the same by which we take an object, and touching it upon one extremity, we say that is its top; and touching it upon another, we say that is its bottom. We observe what is the extent of motion of the arm in reaching from its highest extre-
mity to its lowest; and it is by this sensation, combined with the sensation communicated to the skin, that we determine the position of the object through the sense of touch.

The most remarkable circumstance connected with this subject is the minuteness and the precision with which the eye can observe the differences of place or direction in objects. This can only be explained by referring to the extraordinary fineness of the sensibility to the different degrees of light which belongs to the spot of the retina situated in the axis of vision, and to the susceptibility of the muscles to perceive the smallest variations in the position of this spot while engaged in directing it towards objects. Things which, from their minuteness, almost elude our naked sight, can be divided into upper, lower, and lateral parts.

The conclusions to be derived from this mode of explaining the nature of a perception through the organ of vision, are both curious and highly interesting. We learn that the ideas of objects, which we are in the habit of saying are acquired through the eye, are never the productions of that one organ; but, as if it were for the purpose of certifying the reality of the things around us, and placing this reality beyond the doubts of philosophers, who may have been bold enough to question it, two distinct senses are called into operation. Thus before we obtain the assurance of the simple fact, that the tree before us has its trunk fixed in the ground, and its leaves in the air, the optic nerve must, in the first place, convey the representation of the tree, that is, its colours, shadows, and outlines, by which it is distinguished from other objects; while, in the second place, the muscles must cause the eye to traverse all its boundaries, taking points, and marking distances, so as to estimate its height and breadth, figure and position. There are thus not only two senses, but a process of comparison, calculation, and judgment, which implies an operation of the mental powers, combined in making this simple perception of a tree complete.

Without going further into this subject, I may be allowed to remark, that to the medical man it is of much importance to study the mode of operation of the organs of the senses, and particularly of that which has been under our consideration. The questions connected with squinting and disordered actions of the muscles of the eye, which are still so little understood,
may derive assistance from attending to the intimate connexion which has been shown to exist between the retina and the muscles. When it is observed that the state of activity of the optic nerve draws along with it an activity of the muscles, and that both these parts are equally engaged in the simplest act of perception, it is to be expected that the derangement of the one will materially affect the other. But the nature of these actions of the muscles of the eye cannot be properly understood, unless we attend also to the involuntary motions of the eyeball; by which the eye, at the instant that the optic nerve falls into a state of repose, becomes subject to the operation of a distinct class of muscles. The consideration of these subjects, together with the study of the complicated process by which an act of perception is completed, may perhaps throw some light upon the questions of disordered vision, hallucinations, and some of the delusions of the mind arising from false perceptions of the objects of sight. For the principles on which these views of the nature of vision are founded, I beg to refer my reader to the 'Essay on Single Vision,' by Dr. Wells; where the first indications will be found of the knowledge of a 'muscular sense;' to the posthumous writings of Dr. Brown; but more particularly to the papers published by Sir Charles Bell, on the nerves which supply muscles, and on the nerves and muscles of the orbit.

ON THE INDURATION OF CHALK AND CHALK EARTH UNDER WATER, AND THE APPLICATION OF THIS PROPERTY IN HYDRAULIC ARCHITECTURE.

By J. PENNISTON.

[In a letter to Dr. Fowler, of Salisbury.]

Dear Sir,

In compliance with your wish, I will relate to you, in writing the practical observations I have been enabled to make of the properties which chalky substances have of consolidating and hardening in the water.

The first circumstance of any consequence that occurred to me in this way was previous to the repair of Harnham Bridge, which it was my professional duty to direct as Surveyor for the county of Wilts.

I was then induced, on the recommendation of the foreman
of the masons, to make the bays of chalk, or rather of chalky earth, which forms the banks adjoining the turnpike-road at the bottom of Harnham Hill.

The stream, as you are aware, at the upper side of this bridge, runs extremely rapid, and I confess I had my doubts whether this material would consolidate in so narrow a width (not exceeding 2\(\frac{1}{2}\) feet), and confined only between two hurdles (such as are commonly used for penning sheep), sufficiently to resist the force of the river.

In the progress of forming this bay, a considerable portion of the finer particles of the earth washed away with the current, but sufficient remained to answer every purpose intended. It was formed by men treading in the earth; and on the evening of the day on which it was made, the whole substance was like a bog, or quagmire, where pressure on any part operated on the whole bulk; but on the following morning it was a perfect wall: it continued for many weeks impervious to the pressure of the stream, and when it was necessary to remove it, it presented so obstinate a resistance, that several pickaxes were broken in attempting to do so; nor was it until the following summer, when the water was lower, that it was fully cleared away, and then with the same labour and loss of iron as before.

The next proof I had of its utility, was at Burford Bridge (a village just above Amesbury, Wilts). Here the pier of a cast-iron bridge was literally underwashed, and in great part destroyed, by the floods of 1823-4, and the bed of the river so ploughed up, as to be in holes of from five to ten feet deep. These, after restoring the piers, and repairing the bridge, I filled up by driving piles, and ramming in between these piles large rubble chalk, clay, and flints; but in spite of the care I took in the execution of that plan, the floods of the succeeding winter cleared it completely out. A great portion of the lumps of chalk were rolled some scores of yards down the stream. The piles and clay vanished altogether.

The specimens I had had of the chalk earth, induced me to fill in these holes with the same kind of materials; and I employed some horses and carts to bring it from a chalk-pit at some little distance, selecting such as had been pulverized by wet and frost, and carefully discarding the larger lumps.
The immediate effect of this operation was more surprising than the former: as the carts were backed into the river, and their contents tipped into the stream, the consolidation was almost immediate, for as the carts successively came with their loads, the parts which had been previously filled in were capable of bearing the wheels, with their loads on them. The whole wash was filled in at a comparatively slight expense, and remains perfect to this hour.

From these proofs it has occurred to me, that if the same material were used to strengthen the bed of the Thames in the line of the Tunnel yet to be excavated, it might be attended with the happiest results. All that would be required, would be to bring in barges a sufficient quantity of chalk earth, and throw it into the river at low water: the current would do the remainder of the work; nor have I a doubt, if it proved necessary to cut through a portion of this new-made bed in some future excavation, that it might be done with as much security as cutting through a solid rock of chalk.

Having the honour of being acquainted with Mr. Timothy Bramah, I have mentioned to him the result of the experiments I have here described, and my opinion of their applicability, if the Tunnel should be renewed. The trial would not be a very expensive one, and I should be too happy if any suggestion of mine could, in the slightest degree, be beneficial in forwarding a work so nationally desirable to be completed.

I am, dear Sir, &c. &c.

July 16, 1831.

J. Penniston.

FURTHER OBSERVATIONS UPON SILICEOUS DEPOSITS FROM THE URINE.

By ROBERT VENABLES, M. B.,

Physician to the Chelmsford Provident Society, &c. &c.

The existence of silex in the urine has been generally admitted, upon the authority of Berzelius, and he believes that it is accidentally derived from the water which we drink. Silex has been found, in three instances, intermixed with the composition of urinary calculi; in two by Fourcroy and
Siliceous Deposits from the Urine.

Vauquelin, and in one by Professor Wurzer. I believe I was the first, so far as I have been able to ascertain, publicly to notice the deposition of crystallized silex as gravel from the urine, the particulars of which have been already detailed in an earlier number of this Journal. At the period of sending the communication, and for some considerable time afterwards, I continued to enjoy frequent opportunities of witnessing the appearance of this deposit. It was not, however, a constant occurrence, but was occasionally interrupted for a time, and at various intervals reappeared. Upon one occasion, I transmitted the gravel, precisely as passed, to a gentleman * much devoted to researches of this description, with a request that he would acquaint me with his views upon the subject; and upon receiving his reply, I was much chagrined to learn that he had not been able to discover any siliceous matter in the specimen with which I had furnished him. As I placed the utmost reliance upon the correctness of this gentleman's conceptions, and the accuracy of his judgment, I almost regretted that I had committed my paper to the press, the more especially as, upon several after occasions, the gravel exhibited all the sensible characters of the siliceous depositions previously observed; but upon chemical examination, it did not afford even a trace of silex. However, it was not long before I was gratified by the reappearance of the silex, a portion of which—the specimens being passed at different times—I sent to my friend, and found that he confirmed me in the fact of their siliceous nature. The only question with him then was their urinary origin; but upon this subject I fully satisfied myself by having the urine passed in my presence, so as to prevent the possibility of practising any deception.

Since the publication of that paper, Dr. Yelloly, of Norwich, discovered silex in small granules, 'imbedded in the substance of an oxalate of lime calculus,' from the Norwich collection. The calculus weighed about five grains, and was taken from a boy about nine years old. The examination of this calculus, with the chemical proofs of the intermixture of siliceous granules, is detailed at length in Dr. Yelloly's paper, published in the Philosophical Transactions. Dr. Yelloly, in consequence

* Dr. Prout.
of my paper in this Journal, favoured me with a communica-
tion upon this interesting subject, and, at his request, I had the
honour of furnishing him with some specimens of silex passed
by my patient. In comparing them with those discovered by
himself, he observes that 'they bear some resemblance (though
they are more minute and are of an amber tinge) to those
which I have mentioned as coming under my own view*.'

Very lately I had another opportunity of detecting very
small siliceous particles in a crystallized form in some gravelly
fragments passed by a patient of Mr. Green's, who happened
to be visiting at this place. Having accidentally met with this
gentleman, he was mentioning the circumstances of his case,
particularly his frequently voiding quantities of gravelly matter.
Having obtained a specimen recently passed, I found them to
consist of lithic acid with volatile and fixed alkali and lime. The
volatile alkali was evolved by heating with caustic potassa, and be-
came sensible by the pungent smell. The presence of fixed alkali
was proved by fusing a particle with a small quantity of very
finely divided silex. Exposed on charcoal to the flame of the
blow-pipe, the mass fused into a globule, the transparency of
which was different in different instances. The gravel was of a
pale cream colour, and seemed like so many fragments or scales
of the outer covering of a small nucleus, having both a con-
cave and convex surface. Among these I found a small
nucleus of an irregularly rounded shape, and of about the bulk
of a snipe shot. It was insoluble in muriatic acid, but soluble
with effervescence in the nitric.

Among the fragments which were of a cream-colour, I ob-
served several which were of a much whiter appearance, and
about the bulk of mustard-seed shot, irregular in shape. From
their appearance I took them to be the mixed or fusible phos-
phates, but upon urging a particle with the blow-pipe, I was
surprised to find that it underwent little or no observable
change, except that during the ignition it assumed a very bright
or brilliant incandescence. After ignition it had a strongly
alkaline reaction, and when moistened with distilled water it
slaked and fell to powder like caustic lime, which an additional
quantity of the water dissolved. On subjecting this solution

* On the Tendency to Calculus Disorders. Phil, Trans. 1830.
to the action of a stream of carbonic acid gas, from a capillary jet, it became turbid, and a white powder subsided, soluble with effervescence in diluted hydrochloric acid, and which was again precipitated by oxalate of ammonia. Hence there can be no doubt that the base of this particle was lime.

Another fragment of the same external characters being placed on a capsule, was dissolved with considerable effervescence by a few drops of diluted hydrochloric acid. The whole, however, was not entirely dissolved, but there remained at the bottom of the capsule three very minute crystals, which the acid could not dissolve, though aided by heat. The muriatic solution was very carefully withdrawn, and on being neutralized and heated with oxalate of ammonia, oxalate of lime precipitated. The three crystals were now carefully washed and removed to a platinum capsule, and boiled with concentrated nitric acid, but without undergoing the slightest perceptible change. The acid was driven off by evaporation, and the crystals submitted to the action of the blow-pipe on the platinum capsule, but without suffering any alteration. A little potassa and soda being added, on urging with the blow-pipe flame, solution with effervescence was effected, and the whole fused into a perfectly transparent colourless globule. The globule being pulverized and heated with distilled water, hydrochloric acid being added in excess, a gelatinous mass of very small bulk subsided after a considerable interval. When the jelly had consolidated into a closer and much less bulky deposit, which it did after a couple of days, the supernatant fluid was carefully withdrawn, and the precipitate being well washed with hot muriatic acid, was transferred to a small capsule, and the whole evaporated to dryness, leaving behind a white insoluble powder, which resisted the most intense action of the blow-pipe.

This analysis, therefore, fully proves that the little fragment, instead of being, as I at first imagined, composed of the fusible phosphates, consisted of carbonate of lime, with a very minute though still sensible proportion of crystallized silex. Hence then it would appear that silex, though rarely, does occasionally appear in a crystallized form in the urine. Berzelius, indeed, estimates its quantity at .03 in the thousand parts, but
upon this subject I may observe that it is not always present in the urine of the human subject, nor is its appearance constant even when occasionally discoverable. I have repeatedly analysed the urine of the same individual, and have at times readily found distinct and satisfactory traces of silex; while at others, after the most careful and minute investigation, I have been unable to discover the slightest indication of the presence of this substance.

Being anxious to ascertain whether the silex was confined to this one fragment, or whether it existed in any proportion in the remainder, a quantity, amounting to a grain in weight*, was taken promiscuously from the mass, and being introduced into a test tube, and tolerably strong nitric acid added, heat was applied. It dissolved with effervescence. Distilled water was now added, and the tube, fixed in its stand, was placed under a glass jar and left at rest for forty-eight hours. There was no deposition whatever, which would have been the case if there had been any intermixture of silex. Ammonia being added till neutralization was nearly effected, oxalate of ammonia precipitated a considerable proportion of oxalate of lime. The presence of lithic acid was proved by exposing a portion to the action of caustic potassa in excess aided by heat, and then pouring off the clear solution. Acetic acid being added in excess, the precipitate was washed and collected on a capsule. The solution of this precipitate in nitric acid being evaporated to dryness, and then acted on by ammonia, proved the presence of lithic acid by the formation of purpurate of ammonia.

The presence of carbonic acid in the white limy-looking particles was proved as follows: A very small test tube being filled with and inverted over mercury, a particle or two of the calcareous carbonate was introduced, and immediately rose to the top of the mercury. A small quantity of moderately diluted hydrochloric acid was introduced, by means of another test tube, into that containing the carbonate. The diluted acid immediately rose to the surface of the mercury in the tube, and, acting on the carbonate, dissolved it with considerable effervescence; the mercury at the same time descending in

* The entire specimen with which I was furnished did not amount to two grains.
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The tube. Nitrogen gas was now introduced from a capillary pipe connected with a bladder of this gas into the tube, till the whole of the mercury and the muriatic solution were expelled. A longer tube being filled with and inverted over mercury, about two drachms of lime-water was passed into it, and rose to the top of the quicksilver. The gas of the first tube was now passed into the second: the lime-water became muddy, and a diminution in volume succeeded from the absorption of the carbonic acid, the nitrogen remaining behind*. The recently formed carbonate being acted on by diluted hydrochloric acid, dissolved again with effervescence. The muriatic solution being collected by a pipette, was precipitated by oxalate of ammonia in the usual way,—thus proving the composition to be carbonic acid united to lime.

The little nucleus which, it has been noticed, was placed in a capsule and subjected to the action of nitric acid, next attracted my attention. The capsule had been left at rest for several days under a glass, and on examining it there was found a very minute residue of an indistinctly crystalline appearance, but of high refractive density. The capsule was heated and the acid boiled, but the boiling effected no solution. After subsidence, the acid was removed as carefully as possible; it was then evaporated till the whole of the acid was driven off. The insoluble residue was now ignited by the flame of the lamp enlivened by the blow-pipe directed upon the mass in the capsule: it effectually resisted the heat, undergoing no alteration whatever; but, on adding a little soda, it melted with effervescence into a transparent convex button, flattened at the bottom by the shape of the capsule, thus proving the siliceous character of the residue.

Hence, then, it appears that, in this specimen of gravel, there were distinct traces of silex in two separate fragments: in the one, the silex was in a crystallized form and intermixed with carbonate of lime: in the other, it was in an amorphous or pulverulent form, and mixed with lithic acid combined with an alkaline and earthy base†. If I had

* This was the most easy and ready process with so minute a quantity.
† From the nitric solution carbonate of potass precipitated carbonate of lime in large proportion, soluble with effervescence in hydrochloric acid, and reprecipitable by oxalate of ammonia.
detected the silex in the carbonate of lime fragment only, from the minute portion, and no silex being discoverable in the general mass, I might have been tempted to suppose that the carbonate of lime was an accidental impurity, not legitimately referable to a urinary source, although it is not easy to credit or even suspect that the whole of the intermixed fragments of carbonate of lime were mere accidental contaminations.

But the detection of silex imbedded in lithic salts leaves no doubt of its urinary connexion; and this circumstance renders the urinary origin of the crystallized specimen no longer equivocal. What the peculiar circumstances are which determine the mode of appearance, it is difficult, nay impossible, in the present state of our knowledge, to determine. The more the earthy diathesis seems to prevail, the greater the tendency in the silex to separate in the crystallized form; and, indeed, so far as the few facts ascertained upon this subject will warrant an inference, the deposition of silex seems connected with an earthy diathesis. Fourcroy and Vauquelin found the silex in phosphate of lime; Dr. Yelloly found it in oxalate of lime; Professor Wurzer found it in a calculus consisting principally of lithic acid with a small proportion of phosphate of lime. Wurzer's calculus yielded 1 per cent. on analysis. Its composition was as follows:

<table>
<thead>
<tr>
<th>Component</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Phosphate of lime</td>
<td>17.33</td>
</tr>
<tr>
<td>Lithic acid</td>
<td>75.34</td>
</tr>
<tr>
<td>Animal matter</td>
<td>6.33</td>
</tr>
<tr>
<td>Silex</td>
<td>1.00</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.00</strong></td>
</tr>
</tbody>
</table>

Alemani gives the chemical composition of a urinary calculus, containing not only silica in large proportion, but also phosphate of iron. It was as under:

<table>
<thead>
<tr>
<th>Component</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Magnesia</td>
<td>51.00</td>
</tr>
<tr>
<td>Silica</td>
<td>20.00</td>
</tr>
<tr>
<td>Phosphate of iron</td>
<td>21.84</td>
</tr>
<tr>
<td>Carbonate of magnesia</td>
<td>4.00</td>
</tr>
<tr>
<td>Loss</td>
<td>3.16</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100.00</strong></td>
</tr>
</tbody>
</table>

However, the composition of this concretion is so extra-
ordinary that, allowing the analysis—of which, however, the details are not given—to be correct, doubts may be fairly entertained of its urinary origin. From its composition, it is more legitimately referable to the class of mineral productions.

In the case noticed, in an earlier number of this Journal, as occurring to me, and in another, with the circumstances of which I was not personally so well acquainted, the urine showed a great tendency to alkalescence. They were both women; and one of them, Newton, died lately, after having been much afflicted; the other has left this neighbourhood, and I have not heard of her for some considerable time. I regret much that I had not any opportunity of examining the urine in the present case, the gentleman having quitted this neighbourhood the day following that on which I saw him. I understood him to say that he passed water with great difficulty and pain, and that much exertion brought on a discharge of blood. I understood also that, on sounding, no calculus could be discovered; and, indeed, the shape and size of the fragments tend to prove that, if formed, it must be of very small size. I have generally observed that the secretion of much earthy matter is connected with an alkaline diathesis, and, indeed, soon induces disease of the bladder.

The circumstances which give rise to the appearance of silex in the urine are enveloped in the utmost obscurity. I believe it has not been observed to separate from the urine (after being passed) in a crystallized form spontaneously, nor can it be effected by art. I certainly once observed a deposition of something like crystallized silex on the sides of a tall jar, in which the urine of one of the patients, whose case I have described at length in an earlier number of this Journal, had been suffered to stand for several days; but I must observe upon this subject that, owing to an accident, I had not an opportunity of verifying my supposition by a chemical examination. The quantity observed in the present case is so minute, that possibly it may be looked upon rather as an accidental ingredient. To this, however, it may be objected, that its appearance in the crystallized form is not exactly compatible with such a view. That pulverulent, or finely comminuted silex, might be introduced with drink into the stomach, and pass (in the
gelatinous state), as fluids frequently do, to the kidneys by some less circuitous route than the circulation, it is possible to conceive; but that it could pass in a crystallized state, either by this shorter route, or through the more circuitous one of the circulation, I think will not readily be admitted. There is no other way, then, of accounting for the appearance of this singular deposit, but its secretion by the kidney, and its separation, as other morbid concretions. Nor is there more difficulty in conceiving the kidney capable, under certain circumstances, of such an elimination, than an operation of which there can be no question—the formation of the cystic oxide.

NOTES UPON VEGETABLE TISSUE. By JOHN LINDLEY, Esq., F.R.S., &c.

No. 1.—Cellular Tissue.

BOTANISTS generally recognize three principal elementary forms of tissue, of which, under a variety of modifications, all the parts of plants are constituted; these forms are the cellular, the fibrous, and the vascular. As far as regards tissue, in a state of perfect organization, the limits of these divisions are sufficiently exact, and the latter may be understood as elementary forms; but, if we consider tissue with reference to its own constituent parts, we shall find that these three principal forms are constructed of something still more elementary: viz., membrane and fibre; and that, under the head of cellular tissue, are really comprehended certain modifications, composed of nothing but the latter elementary matter.

Cellular tissue is well known to be the basis of vegetation, to be that form which is indispensable to the existence of a vegetable being, and, therefore, to be in all cases present; while the two other forms are present or absent in plants according to their species. In its most common state, it consists of numerous minute, imperforate, transparent vesicles, pressing the one against the other, and by this pressure acquiring various figures, such as the dodecaedral, the prismatic, the columnar, the cubical, &c.; its sides are destitute of all markings, except
such as may arise from the adhesion of grains of grumous matter to them, and evidently consist of nothing but a very delicate membrane. This, which is the general character of cellular tissue, is not unfrequently considered its absolute distinction; it appears, however, from recent observations, that it is subject to some very remarkable modifications.

It is an old idea, that the membrane of all tissue is composed of interlaced fibres, but this opinion seems to have originated in theoretical views, and either not to have been founded upon observation at all, or at least, not upon accurate observation; the existence of such fibres, in any case, has been denied by Mirbel, Link, and others; and it must be evident to any one, that, in cellular tissue generally, no trace of them is visible. Moldenhauer, however, noticed, so long since as 1779, that the cells of the leaf of Sphagnnum obtusifolium are marked by fibres twisted spirally; but this met with scarcely any attention. Link states, that his supposed fibres are nothing but the lines where small cells, contained in a larger one, unite together; and other botanists pass by the subject without remark. It is, nevertheless, certain, that the observation of Moldenhauer was perfectly correct, and that the cellular tissue of Sphagnnum consists of a membrane, within which a fibre is twisted in an irregularly spiral manner; it also appears that this kind of structure is far from uncommon. In November, 1827, I described the tissue of the testa of Maurandya Barclaiana (See Botanical Register, t. 1108) as consisting of cells, formed of spiral threads crossing each other, interlaced from the base to the apex, and connected by a membrane; this was named, at the time, reticulated cellular tissue, and an approach to it has since been remarked in the seed-coat of several Bignoniaceae. In 1828, Dr. Mohl stated, that in the pith of Rubus odoratus, he had seen cellules, the walls of which were marked with delicate fibres having a reticulated appearance; and that other cases existed, in which the fibres (instead of being reticulated) formed curved lines, parallel with each other (see his Memoir über die poren des Pflanzen-zellgewebes). In August of the same year, I was so fortunate as to discover upon the testa of Collomia linearis, the existence of incredible numbers of spiral fibres, lying coiled up spire within spire, and confined by a dry

T2
mucus, so as to be unable to manifest themselves; but the instant water is applied, the mucus dissolves, and ceases to counteract the elasticity of the spiral vessels, (spires,) which then dart forward at right angles with the testa, each carrying with it a sheath of mucus, in which it for a long time remains enveloped, as if in a membranous case.' (See Botanical Register, t. 1166.) I, however, fell into the error of considering them spiral vessels; they are no doubt analogous to those forms of cellular tissue, in which a fibre only is developed, and are probably of the same nature as what Mr. Brown described, in 1814, as spiral vessels in the testa of Casuarina.

These cases had clearly demonstrated the coexistence of both membrane and fibre, in the cellular tissue, and also that if the latter is usually found composed of membrane only, without fibre, it is occasionally composed of fibre without membrane.

Besides these instances, Meyen, about this time, described fibrous cellules in anthers. It was not, however, till last year, that the existence of fibrous cellular tissue was proved to be so extremely common in flowering plants, that scarcely a species can be named in which it does not exist abundantly. It appears from the descriptions of Dr. I. E. Purkinje, (de cellulis antherarum fibrosis, &c.) who, however, does not appear to have been aware of the abovementioned observations, that in anthers, the inner lining of the valves consists exclusively, either of membranous cellules, the sides of which are marked by fibres, arranged either spirally or otherwise, or of fibres only, arising from the cuticle, projecting into the cavity of the anther, and unconnected by any membrane. This statement is illustrated by good figures of nearly three hundred instances, the accuracy of some of which I have so verified, that I feel confidence in that of the remainder.

It seems probable that this structure, now that attention has been called to it, will be found far from uncommon in the cellular tissue of other parts of plants. I have observed it in the leaves of Brassavola tuberculata in the same state (a very imperfect one) as Dr. Mohl found it, and as I have myself seen it in Rubus odoratus: and it exists in a state of beautiful perfection in the leaves of Oncidium altissimum, where some of the cellules, much larger than the rest, are evidently
formed by the development of a spiral fibre within a membrane.

This being the case, there seems to be no doubt that the basis of cellular tissue is both membrane and fibre, and that the character hitherto assigned to it will require to be much modified in consequence. It may possibly become more difficult to define the exact difference between cellular and vascular tissue; but it will be much less difficult to understand the origin of the latter; and a knowledge of the real character of the former will explain the presence of such tissue as the Elateres of Jungermannia among cellular plants, without the necessity of supposing the existence in them of partial tendency to vascularity.

(To be continued.)

ON HARRIOT'S PAPERS. By S. P. RIGAUD, M.A. F.R.S.
Savil. Prof. of Astronomy.
[To the Editors of the Journal of the Royal Institution.]


ALLOW me to request that you would correct a misstatement which appears in the third Number of your Journal. Neither you, nor the author of it, could have been aware of the injustice of the accusation which it conveys, or I am confident that I should not have occasion to make this appeal to you.

In a very curious dissertation on the first invention of telescopes, Dr. Mohl takes occasion to say (page 495 of your first volume), 'it is to be lamented that Harriot's papers and manuscripts are at present buried in one of the libraries of the University of Oxford.' Now the truth is, that these papers and manuscripts are not, and, what is more, never were in any of our libraries. The story is old; but as it appears that an erroneous impression respecting it still exists, it may yet be right to lay the real state of the case before the public.

Harriot lived under the immediate patronage of Henry Earl of Northumberland, from whose daughter the present Earl of Egremont is descended, and from whom he inherited these
papers. Zach, when a young man, was in England with Count de Bruhl, who married the dowager Lady Egremont, and by this means he got access to the manuscripts at Petworth. He found Harriot's papers there in 1784, and early in 1786 he made a proposal to the University of Oxford to prepare a portion of them for the press, if they would undertake the expense of the publication. This was immediately acceded to, and in April 1786, he wrote a long Latin letter on what he intended that his first volume should contain. A life of Harriot, written in imitation of Gassendi's lives of Purbachius, Regiomontanus, Copernicus, and Tycho Brahe, was to make up the first part, and it was to be followed by some original observations of the comets of 1607 and 1618. Upon the receipt of this communication, an order was made, at the next meeting of the delegates of the press, for the printing to be proceeded in as soon as the editor was ready. Nothing, however, was done by him; and after a lapse of eight years, he sent, in May 1794, by Bishop Cleaver, then Principal of Brasenose, not the work which he had promised, completed and ready for the press, but a certain number of the original manuscripts, without any of the apparatus which he was to have drawn up for them. In the intermediate time he had printed an account of the papers in the Astronomical Ephemeris of the Royal Society of Berlin for 1788, which was translated into English, and circulated in this country. It was probably drawn up from loose memoranda: it is easy to understand that the pleasure of his discovery might have led him to overrate what he had found; but such a feeling will not account for the very erroneous statement of facts which he gave, and which may be attributed to an imperfect recollection of the particulars which he intended to describe. He likewise printed, in Supplement I. to Bode's Jahrbuch (1793), an account of the observations of the comets of 1607 and 1618. This was probably what he had intended for a part of his first volume; and if so, it not only marks the time when he had abandoned his original intention, but gives us such a specimen of his work as diminishes the regret which might be felt for his not having gone on with it.

To return, however, to 1794. The delegates of the press
had every wish to promote the publication; but things were now materially altered. They had undertaken to enable Zach to bring out a work, which he professed to be preparing for publication; but he had not only gone back from his engagement, but had thrown them into a situation, in which he would have made them responsible for working up the materials which he had thought proper to select for them. This last circumstance corroborates a correction, which must be made with respect to the manner in which the business was managed. The general idea is simply that Zach, having found these papers, the Earl of Egremont, in consequence, sent them to Oxford for publication; in that case, however, he would most probably have sent the whole, that a judgment might be formed of the connexion and value of the several parts; but the truth is that Zach, from the beginning, merely endeavoured to make the most of his discovery for himself. He applied to the university, in the first instance, to print his work, because, as he expressed himself in his Latin letter of proposals, 'quo tempore et quo auxilio in lucem proferretur, nulla erat post tot tantosve conatus spes relictæ, nullum relictum consilium.' When he had given up this object he printed the observations of the comets (possibly the only part which really called for publication), and then made his retreat, so as to turn the eyes of the world from himself to the university. Nothing, of course, could have been done with the papers without his having previously obtained the permission of the nobleman to whom they belonged; but Zach appears to have made himself the prime agent in the whole business, so that no direct intercourse took place between the university and the Earl of Egremont, till after the undertaking had been finally given up. I suspect that the delegates themselves were not apprised of their only having a portion of the papers; at least I can recollect no allusion to it from those of former days with whom I have conversed on the subject, and the fact has certainly not been generally known; but, however this may have been, it had become necessary for those who acted on the part of the university, to take the precaution of inquiring further, and ascertaining the nature and character of what had been put into their hands. The late Dr. Robertson was, therefore, re-
quested to make a report on those papers which were connected with abstract mathematics; and the astronomical part was put, for the same purpose, into the hands of the late Mr. Powell, of Balliol. This took place in July 1794. I mention the specific dates to show that I am not writing from vague traditional accounts, but from precise documents which are still in existence. In the following October, Dr. Robertson reported that what had been submitted to him was not calculated for publication; and here the matter rested for some time. Mr. Powell had been prevented from attending to the business, and the delegates, therefore, at length referred his part of the papers also to Dr. Robertson. His opinion, which he gave with the reasons for it in 1798, was in this case likewise against the publication; and in the following year the whole was restored to the Earl of Egremont.

In spite of the fear of being tedious, I have thought it right to enter into all these particulars, because they prove that no blame, in the slightest degree, attaches to the university. None, indeed, could have been brought forward if these facts had been generally understood. But unfortunately they were not. Dr. Hutton inserted Zach's account of the papers in his Dictionary (Art. Harriot), and, without sufficient inquiry, added, 'It is with pleasure I can announce that they (Harriot's papers) are in a fair train to be published; they have been presented to the University of Oxford, on condition of their printing them; with a view to which they have been lately put into the hands of an ingenious member of that learned body to arrange and prepare them for the press.' The first edition of the Dictionary was printed in 1796, and some allowance might then be made for misapprehension; but Dr. Hutton was the old personal friend of Dr. Robertson, and might have obtained any information that he desired from him on the subject; some correction ought, therefore, to have been introduced in the second edition of 1815. Zach's erroneous statement, however, was reprinted in that and many other books both here and abroad: in some cases the substance of Dr. Hutton's inaccurate addition was annexed, and the story was repeated till the whole of it was received as authentic. In this manner obloquy has been brought upon Oxford, because Zach had
too eagerly raised expectations which did not admit of being fulfilled.

By the kindness of the Earl of Egremont I have recently been entrusted with these very papers; some of them are very curious, but, as far as I have had time to examine their contents, I have found very little which it would be useful to publish. I can, however, refer you in this respect to better authority than my own. In 1822, Dr. Robertson communicated his reports on them to Dr. Brewster, and they were printed in the Edinburgh Philosophical Journal (vol. vi. p. 314). They refer, of course, only to those papers which were sent to Oxford; of the remainder any one in London may form his own opinion, as a considerable quantity of them was given, I believe, in 1810, by Lord Egremont, to the British Museum. There were, before that time, some other MSS. of Harriot's in the same place (No. 6001-2, and 6083 of the Harleian collection); Anthony Wood (Ath. Ox. vol. i. p. 391, 1st. ed.) speaks of a MS. of his, entitled 'Ephemiris Chyrometrica,' which he says was in the library of Sion College, but no mention of it is to be found in Reading's Catalogue; and, although Harriot was an Oxford man, I am not at present aware of our having any papers of his in any of our libraries.

S. P. Rigaud.

ON THE MAGNETIC INFLUENCE EXHIBITED DURING AN AURORA BOREALIS.


In an account which I gave of some observations on the aurora borealis of the 7th of January last, I stated that it was my intention to adjust a magnetic needle for the purpose of observing the effects produced on it during an aurora, should there be any recurrence of the phenomenon. This I immediately did, but was not fortunate enough to be able to make any observations until the aurora of the 19th of April.

In order that the nature of the action on the needle may be clearly understood, it is necessary that I should point out the
manner in which the needle was adjusted, and how the character of the forces acting upon it may be inferred from the changes observed in its direction.

I suspended a light needle, six inches in length, by a fine brass wire of a diameter, and twenty-three inches long, within a compass-box, having a ring graduated to thirds of a degree. In the direction of the axis of the needle, and at equal distances from its centre, were placed two twelve-inch-bar magnets, the south-pole of each being towards the south. These were made gradually to approach the needle until their repulsive force on its poles was somewhat greater than the terrestrial directive force. When this was the case, there were three positions of the needle in which the forces acting upon it were in equilibrio, viz., when its south pole or marked end pointed south—when it pointed somewhere between north and west—and likewise when it pointed somewhere between north and east. At the time when the observations which I am about to give were made these positions were nearly south, N. 37° W., N. 37° E., and the observations were made on the deviations of the needle when it pointed between north and west.

Being engaged on the 19th of April in preparing for a journey, I should not have observed the needle on that evening, nor have been aware of the occurrence of the aurora, had not Mr. Faraday called to inform me of it. A few minutes before ten o'clock, when I first saw it, there was a steady stream of yellowish light in the west, 12° or 14° in breadth near the horizon, and clearly perceptible to the height of 40°, although the moon was quite free from cloud: to the north there were streamers shooting upwards, and masses of white light sometimes forming irregular arches. Shortly afterwards there arose from the horizon a very strong stream of light, nearly in the magnetic meridian, 3° or 4° in breadth, very distinct to the height of 50°, and gradually lost towards the zenith. This stream continued steady for about four minutes, when it gradually disappeared. Immediately after this I observed the needle, adjusted as I have described.

At 10 h. P.M., I found the needle vibrating between N. 43° 40' W. and N. 42° 40' W. The vibrations, continuing
about 1° in extent, gradually increased towards the west, and decreased towards the north, until the needle reached N. 55° 30’ W., at which time there was a strong stream of light from the magnetic north. When this had disappeared, the needle returned gradually, and very steadily, towards north; at length, reaching N. 34° W.: at 10 h. 15 m., the direction of the needle was N. 34° 40’ W. Soon after this, I again examined the needle, and then made the following observations:

At 10 h. 30 m. the needle vibrated between N. 40° W. and N. 39° W.
10 33 the direction of the needle was N. 36 30’ W.
10 35 . . . . . . N. 35° W.
10 37 . . . . . . N. 34° W.
10 37 ½ . . . . . . N. 33° 30’ W.
10 39 the needle vibrated between N. 34° 20’ W. and N. 33° 40’ W.
10 42 . . . . . . N. 36° W. and N. 35° W.
10 44 . . . . . . N. 37° W. and N. 36° W.

At this time there were no streamers, and the light was very faint in the north: barometer 29.94, thermometer 42°. I regret that circumstances would not allow of my continuing my observations throughout the night, which I was very desirous of doing. The next morning at 7 h. 20 m. the needle pointed N. 40° W.

The mean direction of the needle, when uninfluenced by the aurora, I consider to have been N. 37° W. As the needle assumed this position in consequence of the attractive force of the earth, acting on its south pole towards north, and of the repulsive force of the magnet, acting upon the same pole in the opposite direction, a deviation towards west would indicate a diminution in the terrestrial horizontal intensity, and a deviation towards north an increase in that intensity, the intensity of the magnets remaining the same.

In a paper published in the Cambridge Philosophical Transactions for 1820, I first pointed out that the change in the direction of the horizontal needle, arising from extraneous action, would be best determined by referring the action to a needle freely suspended by its centre of gravity, and then referring the direction of this to the horizontal plane; and stated, that in this manner we should be able to account for the changes which have taken place in the variation and dip of the
needle during a long series of years. Taking this view of the subject, Capt. Foster*, by a series of observations made at Port Bowen, in 1825, and published in the Philosophical Transactions for 1826, showed that the changes in the horizontal intensity of the needle might be referred to changes in the dip, the terrestrial intensity in the direction of the dip remaining constant, or nearly so. If then we consider that, during the time of the aurora, the absolute terrestrial intensity remained constant, the change which I observed in the direction of the needle indicating a diminution in the horizontal intensity, it will follow that, during this aurora, the force developed was such as to cause an increase in the dip of the needle.

The change that took place in the direction of the needle was so considerable (more than 21° in less than 15 minutes), that it required no nicety of observation to mark its progress. I have before mentioned, that I was so fortunate as to have Mr. Faraday with me at the time: the changes were so manifest, that he could observe them at a short distance from the instrument, at the same time that I was noting them more minutely with the assistance of a glass; and he agreed with me, that the effects could not be more decisive of the influence exerted upon the needle during the aurora.

It has been stated, that auroræ have occurred, during which no effect has been observed on the needle; that this was remarkably the case in Capt. Foster's observations at Port Bowen; and that these observations are 'a refutation of the supposed connexion between tremors of the needle and aurora borealis.'

With regard to these observations we may remark, in the first place, that the needle was observed to be continually in a state of tremor, so that it must have been difficult to decide whether any effects were produced on the needle during the time of an aurora; and, in the second, that, although magnetic effects may, in all cases, be simultaneous with the aurora, yet the direction of the horizontal needle may not invariably be

*By the untimely death of this meritorious and estimable officer, science has lost an able, zealous, and indefatigable auxiliary—his friends one whom they must long continue to deplore.
affected. In order, however, to determine how far any effects were manifest in the direction of the needle, we will analyse the abstract of the observations made by Capt. Foster, at Port Bowen, in the months of January and February, 1825, during which months it happens that, in each, the aurora was visible and invisible during the same number of nights as nearly as possible.

**Aurora visible.**

<table>
<thead>
<tr>
<th>Date. 1825.</th>
<th>Amount of Variation.</th>
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</thead>
<tbody>
<tr>
<td>January 12</td>
<td>0° 51'</td>
</tr>
<tr>
<td>15</td>
<td>4 13</td>
</tr>
<tr>
<td>16</td>
<td>2 25½</td>
</tr>
<tr>
<td>17</td>
<td>2 29</td>
</tr>
<tr>
<td>18</td>
<td>2 56</td>
</tr>
<tr>
<td>20</td>
<td>1 08</td>
</tr>
<tr>
<td>21</td>
<td>1 17</td>
</tr>
<tr>
<td>22</td>
<td>1 20</td>
</tr>
<tr>
<td>24</td>
<td>1 03½</td>
</tr>
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<td>26</td>
<td>2 00</td>
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</tr>
<tr>
<td>29</td>
<td>1 05</td>
</tr>
<tr>
<td>30</td>
<td>1 31½</td>
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**Means** . . . 1 47

**Aurora invisible.**

<table>
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<th>Date. 1825.</th>
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<tbody>
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<tr>
<td>2</td>
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<tr>
<td>3</td>
<td>0 50</td>
</tr>
<tr>
<td>4</td>
<td>0 56½</td>
</tr>
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<td>5</td>
<td>2 33</td>
</tr>
<tr>
<td>6</td>
<td>2 50</td>
</tr>
<tr>
<td>7</td>
<td>2 03</td>
</tr>
<tr>
<td>8</td>
<td>no observations</td>
</tr>
<tr>
<td>9</td>
<td>recorded.</td>
</tr>
<tr>
<td>10</td>
<td>1 23</td>
</tr>
<tr>
<td>11</td>
<td>2 01½</td>
</tr>
<tr>
<td>13</td>
<td>1 00½</td>
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<tr>
<td>14</td>
<td>1 22</td>
</tr>
<tr>
<td>19</td>
<td>1 56</td>
</tr>
<tr>
<td>23</td>
<td>1 16</td>
</tr>
<tr>
<td>25</td>
<td>1 12½</td>
</tr>
<tr>
<td>31</td>
<td>0 26</td>
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</tbody>
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**Means** . . . 1 28

<table>
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<tr>
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<th>Amount of Variation.</th>
</tr>
</thead>
<tbody>
<tr>
<td>February 1</td>
<td>0 39</td>
</tr>
<tr>
<td>2</td>
<td>0 52½</td>
</tr>
<tr>
<td>3</td>
<td>0 17½</td>
</tr>
<tr>
<td>4</td>
<td>0 54</td>
</tr>
<tr>
<td>5</td>
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**Means, Jan. and Feb.** . . . 2 02.7

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**Means, Jan. and Feb.** . . . 1 13.7
Mr. Christie on the Aurora Borealis.

From the means, it appears that the variation, during the days on which the aurora was visible, was greater than during those on which it was invisible; in the month of February, more than double, and on a mean of the two months nearly so. Taking individual observations, we have, in the month of January, when the aurora was visible, six days on which the variation exceeded the mean variation during the month, and eight days on which it was less; when the aurora was not visible, five days on which it exceeded, and ten days on which it was less than the mean: and in the month of February, we have, when the aurora was visible, eleven days on which the variation exceeded the mean variation of the month, and only three on which it was less; when it was not visible only one day on which it exceeded, and thirteen on which it was less than the mean. So that, whatever may be the cause of the aurora, it is evident from these observations, that, during its occurrence at Port Bowen, the needle had in general a tendency to make wider excursions, although this tendency may, in many instances, have been counteracted.

I am aware that results, directly the reverse of these, have been drawn from Captain Foster's observations. In the 'Edinburgh Journal of Science,' vol. viii. p. 200, it is remarked that, 'In the two months during which twenty-eight auroræ occurred, the mean monthly excursions of the needle on each side of its mean position was only $1^\circ 37^\frac{1}{2}$; whereas during the two months when there were no auroræ, it was almost exactly double, viz., $3^\circ 18^\prime 41^\prime$. If this difference, which is far too great to be accidental, shall be confirmed by future observation, it will prove that, in the arctic latitudes, and in those periods which abound with auroræ, the excursions of the magnetic needle are diminished; while, in our latitudes, the causes which produce auroræ increase the excursions of the magnetic needle.'

If we admit this, these observations prove that, during an aurora, magnetic forces are developed, a supposition which they have been considered to refute; but I think we cannot allow that the difference here noticed, in the extent of the variation, can be fairly connected with the aurora. The mean variation for the month of March, during which the aurora was only
visible on three days, was $2^\circ 14'25''$; that for April $2^\circ 52'44''$, and for May $3^\circ 44'39''$, during which latter months the aurora was not seen on any occasion. So that there appears clearly to have been a progressive increase of the variation, quite independent of the aurora; and to this progressive increase we ought to attribute the difference above noticed, more especially as, during the months in which the aurora was visible, the effect appears to have been to increase, instead of diminishing, the variation during an aurora. No one can set a higher value than I do on Captain Foster's observations, but I consider that conclusions have been drawn from them which they do not warrant.

I have already stated, that although magnetic effects may in all cases be simultaneous with the aurora, yet the direction of the horizontal needle may not invariably be affected. This will be evident, if we consider the effects that may be produced on a magnetic needle, freely suspended by its centre of gravity, and refer the direction of this needle to the horizontal plane. If the forces developed are wholly in the vertical plane passing through this needle, it is evident that, although the inclination of the needle may be increased or diminished, yet no change will take place in its horizontal direction, and consequently no changes, in such case, would be observable in the horizontal needle. That this may frequently be the case is evident from the circumstance that the most brilliant beams of the aurora generally affect the magnetic north. I am not aware of the observations which may have been made on the direction of the horizontal needle, during the aurora of the 19th of April last, but as I have before stated, the greatest deviation of the needle which I observed, took place at the time when a strong stream of light issued from the magnetic north. Now, although the effect was here so sensible, owing to the peculiar adjustment of the needle, yet this effect may not have been observable on a horizontal needle, under the influence of terrestrial magnetism alone, however delicately that needle may have been suspended.

If observations were made on a dipping needle, the effects of the forces developed in the plane of the meridian during an
aurora might become sensible; but as this is at best a very imperfect instrument, and as these forces would probably in all cases be small, compared with the other forces giving direction to the needle, the effect produced would, very probably, be quite insensible. To obviate this, the directive force of the needle should be diminished, by placing magnets in the direction of the axis of the dipping needle, with their poles opposite to the corresponding poles of the needle, the magnets being placed at such distances, that the force acting upon the needle in the direction of the dip should be extremely small. In observing, however, with a needle so adjusted, it would be necessary to be extremely cautious that the instrument and the magnets should be so securely fixed, that their relative positions could not alter during the observations,—as a very minute change would produce a very sensible deviation of the needle. In order to observe the effects produced by forces not acting in the meridian, it would be necessary also to adjust a horizontal needle in a similar manner.

As the mechanical difficulties, occurring in such an adjustment of the dipping needle, are considerably greater than in that of a horizontal needle, I consider that it would be better to adjust two horizontal needles in this manner:—

1. A light needle being suspended by untwisted fibres of silk, a fine hair, or a very fine wire, two bar magnets are to be placed as I have described, and at such a distance that the marked end or south pole of the needle may still point to zero. Keeping the magnets in the meridian, and still at equal or nearly equal distances from the needle, they are to be made to approach it until the marked end deviates about 30° to the east, or west of north. If the needle be now led towards 180° or south, by means of a small piece of iron held on the outside of the compass box, it will remain at 180, provided the axes of the magnets are in the meridian: if it does not point to 180, the nearer ends of the magnets must be slightly moved east or west, without changing their distances, until it does. The magnets should now be firmly fixed in their positions by copper nails, and it would be advisable to cover them up with some bad conductor of heat, as a change in their temperature will
cause a change in their intensity, and should this be much diminished, the needle will quit its position at $180^\circ$, and resume that at zero. This needle being left in this position, its deviations will indicate corresponding changes in the direction of the magnetic meridian.

2. Another needle should be adjusted in a similar manner, but the magnets should be brought so near to it that the positions in which the needle will rest become $180^\circ$, N. $70^\circ$ E. and N. $70^\circ$ W.; and instead of being left with the marked end pointing to $180^\circ$, like the former, it should be led to the position between N. and E., or N. and W. (whichever happens to be most conveniently circumstanced for observation), at which it will remain. The changes in the direction of this needle will principally indicate changes in the terrestrial horizontal intensity, corresponding to changes in the dip.

To those who are desirous of observing the magnetical effects produced during an aurora, and who have leisure to watch for the occurrence of this phenomenon, I would recommend such adjustments of two horizontal needles. During the time of an aurora, these needles should be carefully watched, the observer being very careful to remove from his person every article of steel or iron. Their directions should be noted at very short intervals; if their motion be vibratory, the limits should be marked; and the precise time when any change in the direction of their motion takes place should be carefully noted. Another observer should at the same time note any remarkable circumstance in the aurora,—as the appearance of columns or arches of light, their magnetic bearings and attitudes nearly, with the precise time of their occurrence. It is desirable, also, that the directions of the needles should be observed every day at intervals of an hour, throughout the twenty-four hours, particularly that of the needle pointing south, as this would be but little influenced by changes in the temperature of the magnets. By means of the latter observations, not only the times of maximum east and west variation, and the relative extent of the variation each day would be determined, but a comparison would be afforded between the ordinary diurnal excursions of the needle, and those during an aurora; and they would besides enable us to determine with
Mr. Christie on the Aurora Borealis.

greater certainty, whether decided changes in the direction of the needle were simultaneous with the occurrence of distant aurorae.

Royal Military Academy, 27th September, 1831.

ON THE PHYSICAL CAUSE OF ENDOSMOSIS.

By M. DUTROCHET.

Read to the Académie des Sciences, 25th July, 1831*.

WHEN two liquids, differing in capillary ascension, are separated by a thin and permeable partition, two currents, flowing in opposite directions through this partition, are produced; the strong current is that of the liquid which would rise highest, directing itself towards that which would rise the least, and the weak current is that of the liquid which would rise the least, directing itself towards that which would rise the most. The progressive augmentation of the volume of the liquid, which would rise the least, is the result of this double phenomenon. This augmentation is in proportion to the difference which exists between the force of the two opposite currents: it results from the excess of the strong current as compared with the weak current. This excess manifests itself by a simple dynamic effect, for the two opposite currents are in equilibrium, or in a state of compensation to the extent of their equal parts. The force resulting from this excess is that of the endosmosis.

When I first discovered this phenomenon, I was led to consider it as the result of an electric impulsion; and, in fact, the curious electric phenomenon discovered by M. Porret appears susceptible of being referred to endosmosis, may even be said not in any manner to differ from it. In this phenomenon two portions of pure water are separated by a membrane and electrified, the one positively, and the other negatively, by the two poles of a voltaic pile. The water electrified

* The Committee have succeeded, by means of their foreign correspondent, in establishing arrangements by which they may obtain original communications from abroad for publication in this Journal. The present paper by M. Dutrochet, on the important subject of endosmosis, is a paper of this kind.
positively, passes through the membrane to the water electrified negatively, gradually increasing the volume of the latter.

The exact resemblance of this effect to that of endosmosis produced by the difference in the density of the liquids, led me to consider the latter phenomenon as the result of an electric impulsion. The electricity appeared to me to be produced by the difference of density in the two liquids separated by the membrane. Further reflection has, however, induced me to abandon this idea: the body of water in contact with the positive pole disengages oxygen in a state of elasticity; this water, therefore, becomes charged with hydrogen in a state of solution: the body of water in contact with the negative pole disengages hydrogen in a state of elasticity; this water, therefore, becomes charged with oxygen in a state of solution. Thus we have, on one side, water charged with oxygen, and on the other, water charged with hydrogen, or, in other words, two liquids of unequal density. From that moment the phenomenon of endosmosis presents itself, and the water charged with oxygen being necessarily of greater density than that charged with hydrogen, has its volume increased at the expense of the latter. Electricity here is not the immediate, but the remote cause of the phenomenon: it is simply the cause of the difference in density of the two portions of water. This difference is undoubtedly very small, and, therefore, the phenomenon of endosmosis is manifested in a very slight degree.

A very celebrated mathematician (M. Poisson) has sought to explain the phenomena of endosmosis on the principles of capillarity. The following is a summary of the theory which he has recently broached on the subject. The two heterogeneous liquids being introduced into the same capillary canal by its two extremities, are at first both concave, but as soon as they unite, the one remains concave, while the other becomes convex, adapting itself to the concavity of its antagonist; then, by a mechanism founded on calculations made by the learned mathematician, the liquid which rises the highest, and which has remained concave, passes through the membrane, repelling the liquid opposed to it, and runs out: it thus augments the mass of this opposed liquid, with which it is mingled.
Other physiologists, observing that the phenomenon of endosmosis does not take place when two liquids which are not susceptible of being mixed (such as oil and water) are placed in relation to each other, have supposed that the reciprocal dissolution of the liquids played a principal part in this phenomenon, and that the gradual augmentation of the volume of the liquid of the greatest density, was the result of the greater facility of permeation possessed by the liquid of less density which was opposed to it. This theory is destroyed by positive facts; thus, for example, sulphuric acid and water, which have the greatest tendency to a mutual dissolution, do not produce any endosmosis.

The only mode of arriving at a certain theory upon the phenomenon in question, is to observe and appreciate its effects mathematically; this is what I have endeavoured to do by determining, in the first place, the laws regulating the force and velocity of endosmosis. I have ascertained that this force is in proportion to the difference in the density of the two liquids. Thus, for instance, if a solution of muriate of soda, the density of which is 1.06, be brought into relation with water, the density of which is 1, there will be a force of endosmosis which will vary according to the extent of surface of the membrane of the endosmometer. If, in the same instrument, there be put a solution of the same salt, the density of which is 1.12, this solution, being brought into relation with water, will produce a force of endosmosis, which will be the double of that produced, under the same circumstances, by the solution having the density of 1.06. The two forces of endosmosis produced by these two saline solutions will, therefore, be to each other as the two excesses of density of these solutions above the density of the water, that is :: 00-6 : 0-12, or as 1 : 2. I have endeavoured to ascertain whether there was any relation between this law of the endosmosis and that regulating capillary ascension, and for that purpose have examined the comparative forces of capillary ascension of pure water, and of the two saline solutions in question. I took a glass tube, the capillary attraction of which raised water to a height of 12 lines (one inch) in a temperature of 10° C. = 50° F., and found that the same
tube, under the same circumstances, raised the solution of muriate of soda, the density of which was 1.06, to a height of \(9\frac{1}{3}\) lines, and the solution, the density of which was 1.12, to a height of \(6\frac{1}{3}\) lines. Hence result the following calculations:

1st. The capillary ascension of the water being \(12\),
   And that of the first solution \(9.125\)
   The excess of the capillary ascension of the water is \(2.875\)

2nd. The capillary ascension of the water being \(12\),
   And that of the second solution \(6.25\)
   The excess of the capillary ascension of the water is \(5.75\)

These two excesses are precisely in the proportion of 1 to 2, the same proportion which was found resulting from the experiments on the force of endosmosis produced by bringing the two saline solutions in relation with pure water. Thus we find the excess of the capillary ascension of pure water over that of the liquid opposed to it, which is denser, and consequently rises the least, determines the force of the endosmosis, which latter is, therefore, a special result of the capillary force.

The only action of the difference in the density of the liquids is to produce difference of their capillary ascension, so that liquids of smaller density, which have a different capillary ascension, produce an endosmosis differing, and in proportion to the degree of their power of ascension: thus I have found that a solution of sulphate of soda, brought into relation with pure water, produced an endosmosis double of that which a solution of muriate of soda of the same density produced under the same circumstances. The cause of this is found in the measure of the capillary ascension of the two solutions. The capillary ascension of water in a glass tube being 12 lines, that of the solution of sulphate of soda, the density of which was 1.085, was 8 lines, while that of the solution of the muriate of soda, of the same density, was 10 lines. The excess of the capillary ascension of the water over that of the solution of sulphate of soda is 4, and over that of the muriate 2, or in the proportion of 2 to 1. Now this is precisely the proportion of that existing between the endosmosis produced in the experiment made by placing each of these solutions, of equal density, but different capillary ascen-
Dutrochet on the

sion, in relation with the water. This phenomenon is, therefore, unquestionably produced by the excess of capillary ascension of one of the liquids separated by the partition of the endosmometer.

Hence endosmosis is the result of the opposition of two unequal capillary forces acting at the two extremities of the same capillary tube. These two forces impel the two opposite liquids towards each other in unequal quantities, so that one of them (that which has the smallest force of capillary ascension) is gradually augmented in volume; and it is this excess of capillary force which produces the endosmosis. Having demonstrated that the endosmosing liquid is impelled towards the liquid with which it unites, in a quantity proportionate to the excess of its capillary ascension over that of the liquid towards which it is so impelled, it remains to be explained by what mechanism this phenomenon is produced. It appears certain that, under these circumstances, there are two opposite currents, the one strong and the other weak: these two currents may be seen by putting muriatic acid into a glass endosmometer, and plunging it into a glass vessel filled with water. We shall then see the water rise, forming striæ in the acid, and the descend, forming similar striæ in the water. It cannot be supposed that these two opposite and unequal currents pass simultaneously through the same canals; but it is possible that each capillary canal serves alternately to transmit the two opposing currents, and the following fact appears to prove it to be so. I put nitric acid into an endosmometer closed with a piece of bladder, and added to it some very small fragments of gold leaf. I then plunged the apparatus into water. The endosmosis was immediately produced, and I perceived that the water which it introduced into the acid raised rapidly some of the fragments of gold leaf, whilst others remained stationary against the membrane. The fragments of gold leaf, which had experienced an ascending impulse, fell again by their own weight upon the surface of the membrane: they remained motionless there for an instant, and were then again strongly impelled upwards: all the fragments of gold leaf presented these alternations of rapid ascension and fall, followed by a short repose on the membrane, but without being at all simul-
taneous. This fact proves evidently that the capillary canals, which transmit the water impelled towards the acid, or the acid impelled towards the water, do not contain a current constantly moving in the same direction. The fragments of gold-leaf, when they fall after their movement of ascent, are naturally directed towards the canals, which serve at the moment for the current descending from the acid towards the water; they remain motionless on this spot until the canals upon the orifices of which they are placed change their descending into an ascending current, when they resume their upward motion under the influence of the ascending liquid. Without these alternations of the two opposing currents in the same capillary canal, it is not easy to conceive how the endosmosis can be equal to the excess of the capillary action of the opposite liquid. In order to explain the phenomenon, we should have to admit either that the two opposing currents existed simultaneously in the same canal, which is impossible, or that the number of the capillary canals of the membrane is equally divided between the two liquids, and consequently between the two currents, and there appears no reason to suppose the existence of this equal division. But, as experience proves that the capillary canals do not transmit the same liquid without discontinuance, it becomes necessary to admit that the same canal serves alternately for the two opposite currents: this is the only manner in which we can understand the exact relative proportion which exists between the quantity of liquid which is accumulated and the excess of the capillary action of that liquid over that of its antagonist. If, in opposition to what appears to be proved by experience, we assume that there is but one current through the membrane, and that the opposite current is but an optical deception, the result of the affinity of mixture of the two liquids, then the effect of endosmosis would be explained by the opposition of two unequal forces of capillary impulsion; part of the greater force would be employed to counterbalance the smaller force, the effect of which it would thus suspend or neutralise, and there would only remain the excess of the greater force of capillary impulsion over the smaller force to produce endosmosis. In this view of the case, it would not be necessary to admit that the same capillary canal serves
alternately to transmit two opposite currents: all the capillary canals of the partition of the endosmometer would be occupied by currents flowing in the same direction.

Since the capillary action perfectly accounts for all the conditions of the phenomenon of the endosmosis, it is evident that the affinity of mixture of the liquids has no share in the production of this phenomenon; it exists, it is true, as an accessory phenomenon, but has no dynamic effect. If the miscible quality of the liquids be necessary to the existence of the endosmosis, it by no means follows that their mixture acts as a dynamic cause of the effect produced. Endosmosis results from the opposed association of two unequal capillary actions. Now the capillary actions of oil and of water mutually destroy each other; a tube, which has its interior sides rubbed over with oil, will not occasion the capillary ascension of water, so that there is not in this case any opposition of two unequal capillary actions; there is but one, and therefore there is no endosmosis.

The simple capillary action, which alone has been hitherto known, is a force which never impels liquids beyond the sphere of capillary action; the double capillary action, which I have discovered, impels the two opposing liquids in opposite directions across the capillary sphere of action, and tends to drive them out in unequal quantities on each side. All the organic animal or vegetable membranes, which may be used in closing endosmometers, are eminently adapted to produce endosmosis. Among the mineral substances, which may be used in very thin plates for the same purpose, baked clay is the best adapted to produce this phenomenon; plates of carbonate of lime scarcely produce it at all; I was, indeed, disposed to believe that the latter substance was totally incapable of producing it, but, on repeating my experiments, I succeeded in obtaining a very feeble, but still perceptible endosmosis with a plate of white marble of the thickness of one millimetre.

All the liquids which are susceptible of a chemical combination, with the permeable partition of an endosmometer, suspend the endosmosis, after having produced it for a longer or shorter period. Thus, when an acid, alkaline, or saline solution is placed in an endosmometer, an endosmosis is at first
produced, but after some time this effect ceases, and the liquid raised above its level has a tendency to fall by filtration through the permeable partition. This occurs when the partition is modified by the acid, the alkali, or the salt. Alcohol produces the same effect, which results with extreme promptitude from the action of sulphuric and hydro-sulphuric acids, so much so, indeed, as to induce me to believe that these two acids are opposed to the endosmosis. The organic liquids, not having any chemical action on the membranes, or mineral partitions which may be employed to close the endosmometers, produce an endosmosis which would not be liable to any suspension if they always remained the same. But these liquids are decomposed and become acid or alkaline, and often become charged with sulphuretted hydrogen. From that moment, their endosmosing action becomes liable to be destroyed. Thus a membrane acidified, salified, or alkalised, as much as it can be by the acid, saline, or alkaline liquid with which it is in contact, will no longer produce any endosmosis with the same liquid. In order to obtain an endosmosis, which will not cease spontaneously, a permeable partition must be placed in contact, on one side with pure water, and on the other with a liquid which has no chemical action on it. Thus if we put a solution of muriate of soda into an endosmometer closed with a plate of clay, an endosmosis will be produced which will not cease. But if the partition of the endosmometer were membranous, the endosmosis would cease as soon as the partition became salified. Sulphuretted hydrogen puts an end to the endosmosis produced with a plate of clay, as well as with a membrane; because, in both cases, it combines with the elements of the permeable partition. The extreme rapidity of this combination is the cause of the rapid manner in which that substance puts an end to the endosmosis. The same effect is produced by sulphuric acid; but I repeat, that it will also be produced by all liquid substances, which are susceptible of combination with the elements of the permeable partitions of the endosmometer; the only difference in that respect is in the rapidity of the combination. It is not easy to determine with exactness the physical causes of these latter phenomena; but it is evident that they depend upon the capillary action which is modified in the permeable partitions by the chemical
modifications sustained by the latter; and in fact, it is well known, that the capillary action experiences as much variation in its effects from solids of different natures as from different liquids.

ON A DOUBLE-ACTING AIR-PUMP.

By the Rev. WILLIAM RITCHIE, M.A., F.R.S.,
Prof. of Nat. and Exper. Philos. Royal Institution.

SEVERAL attempts have been made to construct double-acting air-pumps; but from the fact that none of them are in use, we may conclude that the practical difficulties attending the construction of them were too great to bring them in competition with those in common use. The following contrivance appears to me sufficiently simple, and will obviously double the power without adding materially to the expense, and with very little additional friction.

It consists of a barrel similar to Smeaton’s air-pump, having a solid piston, with a piston-rod working air-tight in a collar of leather. The piston-rod has a hole drilled along its axis the whole length of the barrel, for the purpose of receiving a brass rod, about the \( \frac{5}{8} \)th of an inch in diameter. The upper end of the rod is slit about an inch, and slightly opened, so as to act as a spring by its friction in raising and depressing
the lower valve. To the lower end of the rod is fixed the conical metallic valve \( V \), which is allowed to rise and fall about the tenth of an inch. A bent tube, \( T \), connects the upper and lower divisions of the barrel formed by the solid piston. This tube is continued from the top to the plate of the air-pump. At the entrance of this tube into the upper end of the barrel, is placed a valve of oiled silk, opening inwards, to allow the air from the receiver to expand into the upper part of the barrel when the piston is depressed. Two valves, either conical or of oiled silk, are placed on the upper and lower ends of the barrel at \( F \), \( E \) opening outwards, to allow the air in the barrel to escape into the atmosphere. When the piston is depressed, the conical valve shuts the communication between the barrel and the receiver, and the air is forced out at the valve \( F \), whilst the air in the receiver rushes into the space above the piston to supply the vacuum thus formed. When the piston begins to rise, the conical valve, on the end of the brass rod, is raised, and the air from the receiver follows the piston till it has reached the top of the barrel, and expelled the air through the valve \( E \). The next depression of the piston performs a similar office, and thus the full of the barrel of air, of the same density as that in the receiver, is at each stroke expelled, and consequently the exhaustion will go on with twice the rapidity of that produced by a single barrelled air-pump of the same size.

Instead of the hollow tube for the piston-rod the wire might be made to pass through the piston, as in the French construction, with two conical valves on the extremities; but the construction I have described seems to me the least liable to objection.

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ON THE METHOD OF OBSERVING THE FIXED LINES IN THE SOLAR SPECTRUM.

By J. T. Cooper, Esq.

As many with whom I am acquainted have sought in vain for those dark bands which occur in the solar spectrum produced by prismatic refraction, usually known by the appel-
lation of Fraunhofer's lines; and as I have never seen, in any work, the precise method described that is necessary to be employed for their successful production, it occurred to me that a description of the instrumental means I adopt might not be unacceptable to some of your readers, and enable those of them who have more opportunity than myself, and are desirous of pursuing the investigation into the cause of their production, to proceed in their researches without the loss of time consequent on the experiments of any kind, where everything has to be sought. It is, therefore, with this view that I have written, and endeavoured, if possible, to save a portion both of time and expense to those who may feel inclined to enter this rich field of inquiry, and to enable those who may wish to determine the refractive and dispersive powers of substances to do so with precision; and, should you concur with me in the propriety of these suggestions, to request you to give them insertion in your useful publication.

In article 422 of Mr. Herschel's admirable paper on Light in the Encyclopaedia Metropolitana, he says that 'with glass prisms of our manufacture it would be quite useless to attempt the experiment.' An assertion coming from such high authority is of itself sufficient to deter any one from making the trial; and he recommends the substitution of hollow prisms, filled with highly refractive media, in lieu of the glass prism. With this assertion I am in some degree disposed to coincide, but certainly not to the extent he there intimates. True it is that not one prism in twenty is fit to be employed for the purpose; yet I have obtained several of British glass, both of flint, plate, and crown, and of various refracting angles, that have shown not only the most prominent of the lines, but even those that may be considered as of the second and third order, and in such abundance, under favourable circumstances, that it would be no easy matter to count them: suffice it to say, that a good prism is necessary, and such can be met with, though not without some difficulty; but neither its size, as respects its length, the breadth of its sides, the refracting angle, nor the kind of glass of which it is made is a matter of much moment*; yet

* I have in my possession equilateral prisms of flint, plate, and crown glass, which are only three-quarters of an inch long, and the sides less than three-tenths
the preference is to be given to one of flint glass with a large refracting angle (say 50° or 60°), because of its high refractive and dispersive powers; all that is required is, that it should approach as near to perfect homogeneity as possible. A few streaks running parallel to the refracting edges may be disregarded; but if they are thrown into waves or curved lines, by injudicious workmanship, the prism is utterly useless for this purpose. Such a prism being obtained, let it be placed twenty, thirty, or more feet distant from, and with its refracting angle parallel to a very narrow linear opening in the window shutter of a darkened room: such an aperture or opening may be conveniently formed in a piece of tin foil, or thin sheet lead, by the point of a sharp penknife; and means must be taken to illuminate this aperture either by the rays of the sun reflected from the surface of a plane mirror, or by the light from a bright sky: the former is decidedly to be preferred; that part of the solar microscope which is employed for a similar purpose when that instrument is used, will be found very convenient for this. The prism being placed in the beam of light transmitted by the aperture, either in a vertical or horizontal position, and its refracting angle as nearly parallel as possible to the aperture, turn it round on its axis until the refracted spectrum of the illuminated aperture formed by the prism appears to be stationary; the prism is then in the position of minimum deviation, and the most favourable one for the production of the lines.

If the spectrum be now carefully examined, a number of narrow black stripes will be seen crossing it at various distances from each other; but as there is some difficulty in seeing them with the unassisted eye, on account of their minuteness, it is better to employ a telescope for the purpose, which, however, need not be of large dimensions, nor possess very high magnifying power; the one I generally employ for this and similar purposes is 1.6 inch aperture, and 18.3 inches focal length, mounted on the common portable stand, and has amplifying

broad, with which I have seen the lines very distinctly; also in a small triangular prism of rock crystal, with its refracting angles parallel to the axis of the original prism, I have seen the lines both in the ordinary and extraordinary spectrum of the aperture perfectly sharp and well-defined.
powers from 15 to 130 times, the object-glass of which may be placed as near as may be convenient to the prism, and a power of 40 or 50 times selected for the occasion; when, however, the adjustment of the focal length of the telescope is made to see the spectrum perfectly sharp and distinct at its edges, the dark lines will be seldom seen, or, if seen at all, but very faint and indistinct; but if the drawer, or eye-piece, of the telescope be pushed in about half an inch, they will then be seen to perfection; and by a nice adjustment, I have seen the major part of the lines nearly as sharp and well defined as the spider lines in the micrometer, employed for the measurement of their distances from each other.

Lambeth, 8th October, 1831.

ON THE ACOUSTIC FIGURES OF PLATES.
By Professor Strehlke.

In the second Number of the 8vo. vol. of Gilbert’s Annalen, Professor Strehlke communicated some very interesting experiments on this subject, which led him to conclude,

1. That acoustic figures are composed of curves; and
2. That these curves do not intersect each other.

And as Chladni had expressed some doubts on the accuracy of the experiments, (chiefly on account of their having been made with metallic plates,) Professor Strehlke afterwards repeated them on glass plates, and convinced himself of the correctness of his former experiments, and of the opinion to which he was led by them. The difference between the figures on metallic and on glass plates, says Professor Strehlke, when speaking of his late experiments, is very trifling; and if the experiments are made with sufficient accuracy, the above result will be equally proved by the figures on either, but the lines are much more distinct on ground-glass plates, metallic plates, or glass plates covered with leaf gold, than on plates of polished glass; and when he used a plate of the latter kind, but covered on one side with leaf gold, the distinctness be-
tween the two figures was very different, so much so that whilst, on the one surface, the curves evidently came only near each other, it was doubtful whether the lines on the other surface were intersected or not. Great attention is further to be paid to other circumstances which might influence the experiment, for the least change of temperature, a very slight degree of humidity, the commencement of oxidation, or any external agitation in the vicinity of the plate, is sufficient to disturb the formation of distinct figures. The quantity of sand * is also of great importance in that respect, and ought not to exceed above three or four grains on a square line; the bow must be moved up and down steadily, and until the figure has ceased to undergo any further change; and if the figures are to be measured, it is indispensable to reproduce them as long as they are not formed by one row of grains only, the joint central line through which may then be regarded as the quiescent line; for if there were several grains, three, for instance, we should by no means be justified in considering the middle one as the representative of the quiescent line, as either of the two others might in reality stand for it; for it is as probable that one of the outer grains is balanced by the two others, as that the middle one is fixed to the two outer grains, &c., for that the plate rests in mathematical lines is, we believe, universally admitted; and although sometimes, if much sand be used, broad lines are formed, the outer rows may always be seen to move as long as the plate sounds, whilst one line is completely quiescent.

In the following experiments the plates were supported on one side only, either on a vertical wooden bar with a small piece of cloth at the point of contact, or merely on the spread fingers of the left hand; either of these two methods is preferable to the use of the screw, which scarcely admits of the reproduction of the same figure; for the least difference in the tension or in the plane where the plate is fixed, changes the figure, &c., which is not the case if the plate is supported in the manner above described, where it is sufficient that the

* Professor Strehlke always used sea-sand, the grains of which, under the microscope, appeared as spheroids from 0″′.03 to 0″′.05 diameter.
resting point be nearly the same, in order to produce the same figures.

The lines were measured by a scale of sufficient accuracy, to convince Professor Strehlke that the results were correct to the \(\frac{1}{100}\)th of a line.

One of the most simple acoustic figures, fig. 2, is formed when three or four corners of the plate are supported, and the bow is drawn at the middle of one of the sides: it may be obtained by simply holding the plate with the left hand, placing the thumb on E, fig. 1, the first finger on C, and the little finger on D, and then drawing the bow along the middle of DF. On all regular plates of metal or glass the figure will be found to be a hyperbola, the angles between the principal axis and asymptotes of which exceed 45°. The direction of the principal axis cannot be determined previously to the experiment; but after it is found, it will be proper to mark it down on the plate, as the direction of a great many other figures is dependent on it.

On three plates, two of brass and one of copper, the ordinates were equally divided by the axis; B was taken as the commencement of the co-ordinates, and on the supposition that the curve was a conic section of the equation

\[ y^2 = px + qx^2; \]

the most probable value of the coefficients was determined by the method of the least squares.

In a square plate of brass (No. 1.) of 34.3 Parisian lines in length, and 1\".1 thick, the equation deduced from the measured value of the co-ordinates was

\[ y^2 = 10836 \cdot x (x + 6\".27), \]
and the relation between the observed and calculated values of $y$ was

\[
\begin{array}{|c|c|c|c|}
\hline
\text{Observed.} & \text{Calculated.} & \text{Difference.} \\
\hline
x & y & \bar{y} & m' \\
\hline
1.05 & 2.94 & 2.89 & + 0.05 \\
1.72 & 3.88 & 3.86 & + 0.02 \\
2.99 & 5.43 & 5.48 & - 0.05 \\
4.43 & 7.15 & 7.18 & - 0.03 \\
5.71 & 8.64 & 8.61 & + 0.03 \\
9.20 & 12.41 & 12.42 & - 0.01 \\
\hline
\end{array}
\]

So that the results of calculation and observation may almost be considered to accord.

The calculated distance between the summits of the curve, $= 6'''.27$, was also found to agree with observation, it being in three different experiments, $6'''.25$, $6'''.26$, and $6'''.24$.

The plate was now ground, in order to see what change this would produce in the situation of the curve. The following equation was obtained:

\[
y^2 = 1.1032 \cdot x \cdot (x + 6'''.228);
\]

and the difference between measurement and calculation was,

\[
\begin{array}{|c|c|c|c|}
\hline
\text{Observed.} & \text{Calculated.} & \text{Difference.} \\
\hline
x & y & \bar{y} & m' \\
\hline
0.06 & 2.33 & 2.24 & + 0.09 \\
1.39 & 3.46 & 3.42 & + 0.04 \\
2.16 & 4.52 & 4.47 & + 0.05 \\
2.88 & 5.46 & 5.38 & + 0.08 \\
3.77 & 6.37 & 6.45 & - 0.08 \\
4.60 & 7.45 & 7.41 & + 0.04 \\
6.29 & 9.25 & 9.32 & - 0.07 \\
8.75 & 12.05 & 12.03 & + 0.02 \\
\hline
\end{array}
\]

so that scarcely any alteration had been produced in the curves.

Both branches of the hyperbola appeared to be equal; this was however only apparent, for when it was taken as the origin of the co-ordinates, it was found that

\[
y^2 = 1.1627 \cdot x \cdot (x + 5'''.379),
\]

and the unit of measurement and calculation to be
by which it appears that the elasticity of the one-half of the plate slightly differed from that of the other.

In another brass plate (No. 2.) of 34".3 length, and 0".66 thick, the equation for the branch B of the hyperbola was

\[ y^2 = 1.1421 \times x \times (x + 6''126) \ldots (1) \]

and that for A

\[ y^2 = 1.1472 \times x \times (x + 6''062) \ldots (2) \]

which shows that the elasticity in both halves was almost equal.

The difference between measurement and calculation appears from the following tables:

(1)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>( x )</td>
<td>( y )</td>
<td>( y )</td>
</tr>
<tr>
<td>0.78</td>
<td>2.30</td>
<td>2.36</td>
</tr>
<tr>
<td>1.39</td>
<td>3.43</td>
<td>3.31</td>
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<tr>
<td>2.22</td>
<td>4.38</td>
<td>4.43</td>
</tr>
<tr>
<td>3.02</td>
<td>5.46</td>
<td>5.43</td>
</tr>
<tr>
<td>3.74</td>
<td>6.34</td>
<td>6.30</td>
</tr>
<tr>
<td>4.60</td>
<td>7.20</td>
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<tr>
<td>5.15</td>
<td>7.98</td>
<td>7.94</td>
</tr>
<tr>
<td>8.48</td>
<td>11.69</td>
<td>11.69</td>
</tr>
</tbody>
</table>

(2)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>( x )</td>
<td>( y )</td>
<td>( y )</td>
</tr>
<tr>
<td>0.89</td>
<td>1.66</td>
<td>1.70</td>
</tr>
<tr>
<td>1.44</td>
<td>3.60</td>
<td>3.53</td>
</tr>
<tr>
<td>2.52</td>
<td>4.99</td>
<td>4.99</td>
</tr>
<tr>
<td>3.71</td>
<td>6.40</td>
<td>6.46</td>
</tr>
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<td>4.82</td>
<td>7.76</td>
<td>7.76</td>
</tr>
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<td>5.90</td>
<td>8.97</td>
<td>9.00</td>
</tr>
<tr>
<td>6.87</td>
<td>10.16</td>
<td>10.10</td>
</tr>
<tr>
<td>9.86</td>
<td>13.41</td>
<td>13.42</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>( x )</td>
<td>( y )</td>
<td>( y )</td>
</tr>
<tr>
<td>1.16</td>
<td>3.19</td>
<td>3.10</td>
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<tr>
<td>2.10</td>
<td>4.57</td>
<td>4.43</td>
</tr>
<tr>
<td>3.10</td>
<td>5.65</td>
<td>5.71</td>
</tr>
<tr>
<td>3.99</td>
<td>6.81</td>
<td>6.78</td>
</tr>
<tr>
<td>4.99</td>
<td>7.92</td>
<td>7.95</td>
</tr>
<tr>
<td>5.87</td>
<td>8.92</td>
<td>8.96</td>
</tr>
<tr>
<td>7.04</td>
<td>10.30</td>
<td>10.29</td>
</tr>
<tr>
<td>9.14</td>
<td>12.63</td>
<td>12.62</td>
</tr>
</tbody>
</table>
In a square plate of copper (No. 3.) of $34''\,3$ in length, and $1''\,33$, the equation for $B$ was found to be

$$y^2 = 1027 \cdot x \cdot (x + 3''\,846),$$

and the distance between the two summits $3''\,88$; the results of observation and calculation agree in the following manner:

<table>
<thead>
<tr>
<th>Observed</th>
<th>Calculated</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>$w$</td>
<td>$y$</td>
<td>$y'$</td>
</tr>
<tr>
<td>0.51</td>
<td>1.44</td>
<td>1.51</td>
</tr>
<tr>
<td>0.79</td>
<td>1.83</td>
<td>1.94</td>
</tr>
<tr>
<td>1.33</td>
<td>2.60</td>
<td>2.66</td>
</tr>
<tr>
<td>1.62</td>
<td>3.05</td>
<td>3.02</td>
</tr>
<tr>
<td>2.48</td>
<td>3.99</td>
<td>4.01</td>
</tr>
<tr>
<td>3.10</td>
<td>4.93</td>
<td>4.91</td>
</tr>
<tr>
<td>4.12</td>
<td>5.82</td>
<td>5.81</td>
</tr>
<tr>
<td>5.04</td>
<td>6.65</td>
<td>6.78</td>
</tr>
<tr>
<td>5.76</td>
<td>7.53</td>
<td>7.54</td>
</tr>
<tr>
<td>6.70</td>
<td>8.59</td>
<td>8.52</td>
</tr>
<tr>
<td>7.70</td>
<td>9.64</td>
<td>9.55</td>
</tr>
<tr>
<td>8.75</td>
<td>10.54</td>
<td>10.64</td>
</tr>
<tr>
<td>9.92</td>
<td>11.80</td>
<td>11.84</td>
</tr>
</tbody>
</table>

In order to ascertain whether the distance of the summits was in any way related to the length of the side of the plate, this distance was measured on a great number of plates, and then compared with the length of the side. In the following table $2a$ is the distance in decimal fractions, the entire length of the side being 1.000, and $\varphi$ the angle between the asymptote of the hyperbola, and the principal axis.

<table>
<thead>
<tr>
<th></th>
<th>$2a$</th>
<th>$\varphi$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brass plate, No. 1.</td>
<td>0.181</td>
<td>46.9</td>
</tr>
<tr>
<td>Ditto ditto, No. 2.</td>
<td>0.177</td>
<td>46.56</td>
</tr>
<tr>
<td>Ditto ditto, similar to No. 2.</td>
<td>0.116</td>
<td>45.46</td>
</tr>
<tr>
<td>Ditto ditto, of 53'',6 in length</td>
<td>0.207</td>
<td>46.36</td>
</tr>
<tr>
<td>Ditto ditto, of 57'',8 in length</td>
<td>0.151</td>
<td>46.13</td>
</tr>
<tr>
<td>Copper plate, No. 3.</td>
<td>0.112</td>
<td>45.23</td>
</tr>
<tr>
<td>Two copper plates similar to the brass plate No. 2.</td>
<td>0.263</td>
<td>47.36</td>
</tr>
<tr>
<td>Zinc plate of 53'',6</td>
<td>0.326</td>
<td>49.46</td>
</tr>
<tr>
<td>Ditto ditto</td>
<td>0.175</td>
<td>46.8</td>
</tr>
<tr>
<td>Ditto ditto ditto</td>
<td>0.189</td>
<td>46.18</td>
</tr>
<tr>
<td>Tin plate of ditto</td>
<td>0.167</td>
<td>46.25</td>
</tr>
<tr>
<td>Plate of bronze</td>
<td>0.164</td>
<td>46.12</td>
</tr>
<tr>
<td>Glass plate covered with a thin layer of sealing-wax</td>
<td>0.155</td>
<td>45.47</td>
</tr>
<tr>
<td>Ditto ditto with a solution of gum in alcohol</td>
<td>0.179</td>
<td>46.37</td>
</tr>
<tr>
<td>Ditto ditto ditto ditto sulphur, ether</td>
<td>0.127</td>
<td>45.28</td>
</tr>
</tbody>
</table>
These observations do not seem to lead to any certain result, except that if $2a$ increases, $\phi$ increases also, in accordance with what is observed in plates which are fixed between screws, where the distance between $A$ and $B$ may be increased *ad libitum*, by fixing the plate at a greater or less distance from its middle. Thus, if the brass plate, No. 2, (in which $2a=0.177$, and $\phi=46^\circ\cdot56'$) was fixed between screws at a point of the principal axis to the right from $B$, $2a$ was $0.341$, and $\phi=51^\circ\cdot53'$; if to the left from $B$, $2a$ was $0.148$, and $\phi=44^\circ\cdot16'$. If the plate was fixed at any other point the tone remained the same, but the axis of the hyperbola was perpendicular to that of the former figure.

Another very simple acoustic figure is obtained by supporting the sides $D\ C\ D'$ (fig. 3) at the middle, and producing the vibration at one of the corners; it consists of two hyperbolic branches, the principal axis of which coincides with the diagonal of the square: sometimes fig. 4 is produced, the major axis of which is always in the direction of the principal axis of the hyperbola in fig. 2.

The formation of this figure seems to be fully accounted for by the theory of undulations, for in a vessel of the same form as the plate, and filled with liquid, the interferences of the undulations will produce the square $C\ D\ C'\ D'$, provided the resistance which the undulations experience in the directions $C\ C'$, and $D\ D'$, is equal; if not, and if the resistance to the direction $D\ D'$, is greater than to that of $C\ C'$, the angles of the square will be changed into hyperbolic corners, and the longer axis of the ellipse will be in the direction $C\ C'$, and the shorter in that of $D\ D'$; and if further, the resistance in the same direction is unequal, the figure will become irregular with regard to its opposite parts.

The difference of the two axes in fig. 4, is proportionate to the distance between the summits of the hyperbola in fig. 2, for

On the brass plate No. 1, $C\ C'$ was $=30''\cdot58$ $D\ D' = 27''\cdot92$, and $AB=6''\cdot27$
Ditto ditto No. 2, $=31\cdot36$ $=28\cdot03$ $=6\cdot12$
On a brass plate of the same dimension $=30\cdot86$ $=29\cdot36$ $=3\cdot38$
On the copper plate No. 3, $=29\cdot53$ $=29\cdot03$ $=3\cdot58$

On two copper plates, on which $AB$ had been found to be
0.263 and 0.326, (the side of the plate being 1.000,) D D' was 25''2 and 26''6, the curves had the form of fig. 5, C and C' being beyond the plate.

The curve D C D' E' was also measured and calculated on the brass plate No. 1. The co-ordinates on C C' were measured, and for the curve A C B the fourth part of D C D' C', the equation was found to be

\[ y^2 = 0.4663 \cdot x \cdot (x + 25''829), \]

and the results of observation and calculation agreed in the following manner:

<table>
<thead>
<tr>
<th>x</th>
<th>y</th>
<th>y</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.78</td>
<td>3.10</td>
<td>3.10</td>
<td>-0.01</td>
</tr>
<tr>
<td>1.27</td>
<td>4.76</td>
<td>4.77</td>
<td>-0.01</td>
</tr>
<tr>
<td>2.41</td>
<td>5.65</td>
<td>5.63</td>
<td>+0.02</td>
</tr>
<tr>
<td>3.16</td>
<td>6.54</td>
<td>6.53</td>
<td>+0.01</td>
</tr>
<tr>
<td>3.79</td>
<td>7.31</td>
<td>7.23</td>
<td>+0.08</td>
</tr>
<tr>
<td>4.54</td>
<td>7.92</td>
<td>8.02</td>
<td>-0.10</td>
</tr>
<tr>
<td>5.21</td>
<td>8.78</td>
<td>8.68</td>
<td>+0.10</td>
</tr>
<tr>
<td>5.87</td>
<td>9.28</td>
<td>9.31</td>
<td>-0.03</td>
</tr>
</tbody>
</table>

For A' C' B' the equation was

\[ y = 0.4434 \cdot x \cdot (x + 26''777), \]

with the following result of observation and calculation,

<table>
<thead>
<tr>
<th>x</th>
<th>y</th>
<th>y</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.50</td>
<td>2.38</td>
<td>2.06</td>
<td>-0.08</td>
</tr>
<tr>
<td>1.33</td>
<td>3.99</td>
<td>4.07</td>
<td>-0.08</td>
</tr>
<tr>
<td>2.11</td>
<td>5.21</td>
<td>5.20</td>
<td>+0.01</td>
</tr>
<tr>
<td>2.88</td>
<td>6.20</td>
<td>6.15</td>
<td>+0.05</td>
</tr>
<tr>
<td>3.66</td>
<td>6.98</td>
<td>7.03</td>
<td>-0.05</td>
</tr>
<tr>
<td>4.43</td>
<td>7.87</td>
<td>7.83</td>
<td>+0.04</td>
</tr>
<tr>
<td>5.16</td>
<td>8.64</td>
<td>8.64</td>
<td>+0.00</td>
</tr>
<tr>
<td>6.09</td>
<td>9.42</td>
<td>9.42</td>
<td>+0.00</td>
</tr>
</tbody>
</table>
In $\Delta DA'$ it was necessary to incline $D'D'$ towards $C'C'$, in order to divide the ordinate equally, the co-ordinates were consequently not quite perpendicular. It was found that

$$ y^2 = 0.4989 \cdot x \cdot (x + 33''005), $$

from which $y$ was calculated with the following result,—

<table>
<thead>
<tr>
<th>Observed.</th>
<th>Calculated.</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>$x$</td>
<td>$y$</td>
<td>$y''$</td>
</tr>
<tr>
<td>0.50</td>
<td>2.77</td>
<td>2.92</td>
</tr>
<tr>
<td>1.44</td>
<td>5.07</td>
<td>5.02</td>
</tr>
<tr>
<td>2.33</td>
<td>6.40</td>
<td>6.46</td>
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<tr>
<td>3.24</td>
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<td>7.72</td>
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<tr>
<td>4.02</td>
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<td>8.69</td>
</tr>
<tr>
<td>4.90</td>
<td>9.69</td>
<td>9.70</td>
</tr>
</tbody>
</table>

In the curve $B'D'B'$, the co-ordinates were perfectly perpendicular, and the equation deduced was

$$ y^2 = 1.001 \cdot x \cdot (x + 15''14), $$

which shows a very great difference of elasticity on the two sides of the plate; the centre of the plate was also almost by $0''.6$ nearer to $D'$ than to $D$.

In the same curve, on the brass plate No. 2, the equation for $A'C'B'$ was found to be

$$ y^2 = 0.445 \cdot x \cdot (x + 24''.459), $$

and for $A'C'B'$

$$ y^2 = 0.4002 \cdot x \cdot (x + 28''.597); $$

where the results of calculation and observation were also found to correspond.

The other acoustic figures consist merely of a repetition of the above curves. Fig. 8 consists of four squares, each of which contains Fig. 2, and will be obtained by supposing the plate at $F$ and $D'$ (Fig. 3), or $C$ and $E$ (Fig. 1), at the centre of any of the four squares. The direction of the principal axes will be parallel to that of $AB$ in Fig. 2; the asymptotes of the inner hyperbolas become also hyperbolic curves, so that the whole figure might be constructed à priori, provided the elasticity is perfectly equal; if not, some of the hyperbolas will have their axes perpendicular to the direction of $AB$ in Fig. 2.

The measurement of the curves was made on a square brass plate, $53''.63$ in length, and $0''.7$ thick. The line $FT'$, which
is parallel to the side of the square, and divides the ordinates into two equal parts, was at the distance of almost the fourth part of the side from the edge: the equation for A B A was

\[ y^2 = 1.643 \cdot x (x + 5''-554); \]

and for E F E

\[ y^2 = 1.533 \cdot x (x + 5''-591); \]

and the results of observation and calculation were found to correspond in the following manner:

For A B A,

<table>
<thead>
<tr>
<th>Observed</th>
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<td>x</td>
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<td>5.98</td>
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For E F E,

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<tr>
<td>4.71</td>
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<td>8.62</td>
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For C D C the equation was

\[ y^2 = 1.676 \cdot x (x + 4''-662); \]

and for G H G

\[ y^2 = 0.9213 \cdot x (x + 11''-034), \]

with the following results:

For C D C,

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<tr>
<td>0.28</td>
<td>1.39</td>
<td>1.52</td>
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<tr>
<td>0.55</td>
<td>2.16</td>
<td>2.19</td>
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<tr>
<td>1.05</td>
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<td>3.71</td>
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<tr>
<td>1.94</td>
<td>4.50</td>
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<tr>
<td>2.22</td>
<td>5.10</td>
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For G H G,

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<tr>
<td>$x$</td>
<td>$y$</td>
<td>$\frac{y}{m}$</td>
<td>$\Delta$</td>
</tr>
<tr>
<td>0.33</td>
<td>1.99</td>
<td>1.86</td>
<td>+ 0.13</td>
</tr>
<tr>
<td>0.83</td>
<td>2.99</td>
<td>3.01</td>
<td>− 0.02</td>
</tr>
<tr>
<td>1.27</td>
<td>3.77</td>
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<tr>
<td>2.11</td>
<td>4.99</td>
<td>5.05</td>
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<tr>
<td>3.77</td>
<td>7.15</td>
<td>7.17</td>
<td>− 0.02</td>
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The equation $y^2 = 1.479 \cdot x (x + 5^\circ 44)$, is the expression of the curve A M N.

The whole figure is consequently composed of hyperbolas of different form, according to the different degree of elasticity at the various parts of the plate.

It may be easily anticipated what figure will result from the division of the square into nine smaller ones, &c.

If the plate being supported at D and D' (Fig. 3), and at the middle of F C or C G, the vibration is produced at C, Figs. 9 and 10 will be formed, in which A B is composed of

![Fig. 9.](image1) ![Fig. 10.](image2) ![Fig. 11.](image3) ![Fig. 12.](image4)

two hyperbolas meeting at the centre of the plate. Or, if A and B (Figs. 11 and 12), and a point, which is distant from C or C' by a third of C B, are supported, Figs. 11 and 12 will be obtained, which differs from Figs. 9 and 10 only as far as A B comes nearer a straight line, and that the lines at the corners are almost parts of circles. The tone is for both the same, but that of Figs. 11 and 12 is the fullest of the two.
If the plate, instead of being supported, is fixed between a screw at a point distant about one fourth of the side from it, and half of the side from the edges, a remarkable transversion of the Figs. 9 and 10 take place. Suppose two lines drawn through the above point, parallel to the sides of the square, and the four right angles to be 1, 2, 3, and 4: if the plate is fixed in angle 1, at the distance of a few lines from the point of intersection, Fig. 9 will be formed; if in angle 2, it will be Fig. 10; in angle 3, Fig. 9; and in angle 4, Fig. 10 again: this is always the case, and it is not even necessary that the point where the plate is fixed should be always exactly at the same distance from the point of intersection; the distance of the curves from each other only will be changed.

The measurement of Fig. 12, on the copper plate No. 3, led to the following result: e was considered as the commencement of x and y, and the equation of the curve on the copper plate No. 3 was

\[ y^2 = 0.8904 \cdot (143.2 - x^2) \]

the calculation of y from which was found pretty nearly to correspond with the result of observation.

On the brass plate No. 1, the expression of the curve was

\[ y^2 = 0.899 \cdot (144.8 - x^2) \]

and on the brass plate No. 2,

\[ y^2 = 0.8962 \cdot (334.58 - x^2) \]

If half of the greater axis of the ellipse is made = a, and half of the lesser axis = b, the length of the side of the plate being = 1.000, it was found that

on the copper plate a, was = 0.348 and b = 0.328
on the brass plate No. 1 = 0.350 = 0.332
and on the brass plate No. 2 = 0.341 = 0.323

or that, in general, the lesser axis of the ellipse is nearly one sixth of the length of the side.

If the plate is divided into four squares, Figs. 11 and 12 being repeated four times, will produce Fig. 13 or 14.

Fig. 13 is obtained when the plate is supported at A B, and at a third point, distant from any corner by one third of the
side, whilst the vibration is produced at the middle of a side. It consists of the two branches of a hyperbola, the principal axis of which is in the direction of $AB$ in Fig. 2, and of the ellipse $FF'$ and $DD'$, of which $D$ and $D'$ are smaller than $F$ and $F'$.

On the brass plate No. 2, $F'$ and $e$ were taken as the commencement of the co-ordinates, and the equations for the two ellipses were

$$y^2 = 5.3477 + 1.6366 \cdot x - 0.6725 \cdot x^2$$

and

$$y^2 = 2.8798 \cdot x \left(11'' 01 - x\right).$$

Fig. 14 is formed by supporting the plate at $ACBD$, so that $DB = \frac{1}{3} AB$; and by producing the vibration in $E$, the long axis of $ACBF$, and of the central ellipse, is in the same direction as the principal axis of the hyperbola in Fig. 2. On the brass plate No. 2, the figure was not quite closed at $A$ and $B$.

Figs. 11 and 12 may further be transformed into Figs. 15 and 16: if the plate is supported at $AB$, and at a point of the diagonal perpendicular on $AB$, at the distance of $\frac{1}{6} AB$ from $S$, and the vibration is produced at $E$, $ES$ being $= \frac{2}{3} AS$. In either case the ellipses of Figs. 11 and 12 seem to be changed into hyperbolas, the principal axis of which is either perpendicular to the diagonal $AB$ (Fig. 15), or parallel to it. The equation for $DC$ (Fig. 15) was found to be

$$y^2 = 80.546 \cdot 1.7848 \cdot x - 0.9485 \cdot x^3.$$

Fig. 6, which will be easily recognised as the repetition of a more simple figure, is produced when the plate is supported at
Strehlke on the Acoustic Figures of Plates.  305

S S' and A (or, instead of A, at A E or E'), and caused to vibrate at B. The long diameter C C' of D C D' C', as may be anticipated from the above, is parallel to the principal axis of the hyperbolas in Fig. 2; and this is also the case with the lateral ellipse B B' F F'. But if, instead of supporting the plate, it is fixed between a screw, at a point of F E, which is nearer to the centre than E, the long diameters of the curves are in the direction of D D', and the other parts of the figure are changed accordingly.

If, in Fig. 6, the plate is supported at S S', and at the middle of H B, Fig. 7 is formed, in which E E' and D D' are always in the direction of the principal axis in the hyperbola of Fig. 2.

EXPERIMENTAL ILLUSTRATION OF THE EQUALITY BETWEEN THE RADIATING AND ABSORBING POWERS OF THE SAME SURFACE.

BY THE REV. WILLIAM RITCHIE, M.A., F.R.S.,
Prof. of Nat. and Exper. Philos., Royal Institution.

PROFESSOR Leslie has shown, by a series of ingenious experiments, that those surfaces which radiate heat most copiously also absorb it in the greatest quantity; but, as far as I know, it has not been experimentally demonstrated, at least in a manner adapted to illustration before a large audience, that the radiating and absorbing powers are exactly equal to each other. The simplicity of the following mode of illustration, and the clear conviction which it brings to the minds of an audience, have induced me to give it a place in the Journal of the Royal Institution.

* The instrument consists of a large differential thermometer, with cylindrical chambers made of the thinnest tin-plate, similar to what I formerly described in the transactions of the Royal Society*. The horizontal branch, A B, of the glass tube is

* Phil. Trans., 1827, p. 123.
about a foot long; the vertical portions, AD, BC, being each

![Diagram]

four or five inches. Near the extremities of the vertical portions two small bulbs are blown, for the purpose of holding a part of the coloured liquid and preventing it entering the cylindrical chambers. The cylindrical chambers may be three or four inches in diameter and half an inch thick, having tin tubes soldered in the rim for receiving the extremities of the glass tube, which is rendered air-tight by common electric cement. The surface of one of the cylindrical chambers is coated with lamp-black, whilst the opposite surface of the other chamber is left perfectly bright. To the glass tube is attached a small scale, divided into any number of equal parts. The liquid which I prefer, on account of the delicacy of its motions, is spirit of wine, tinged in the usual way.

A cylinder of tin-plate, of the same diameter as the chambers of the thermometer and about an inch thick, having one of its sides coated with lamp-black and the other left bright, is to be placed exactly half way between the chambers, and then filled with hot water, when the following phenomena will be observed:

1. When the coated side of the cylinder D is turned opposite the coated side of the chamber D', the fluid in the stem BC will sink with extreme rapidity. The reason of this is obvious: From the coated side of the cylindrical canister we have an immense quantity of radiant heat shooting out at right angles to the surface, and falling on the powerfully absorbing surface of the cylindrical chamber of the instrument; whereas, from the opposite metallic surface of the canister we have a very scanty portion of radiant heat, and that, too, falling on a surface which absorbs only a minute portion; hence the striking difference between the temperature of the air in the two chambers of the thermometer.

2. Remove the canister, and again place it exactly in the
Radiating and Absorbing Powers.

middle between the two chambers as before, with its coated side turned towards the bright side of the cylindrical chamber and its bright side towards the coated surface of the other chamber, fill it with hot water, and the liquid in the stem will be found to remain perfectly stationary. The reason of this beautiful result is equally obvious: From the coated surface of the canister we have a copious flow of radiant heat,—suppose ten times as much as from the metallic surface,—which falls on a surface of feeble absorbing power, which we shall suppose takes in only one part out of ten of the whole radiant stream; from the other side of the canister we have only a very small portion of radiant heat, which we suppose equal to one, which portion is absorbed by the coated surface of the chamber, and rapidly conveyed to the included air; and, since the effect on both chambers is the same, we conclude the radiating and absorbing powers are exactly equal to each other.

If the surfaces be scratched or coated with other substances, the same law will be found uniformly to obtain. The instrument which I have described is not only useful for the striking illustration of this fact in the lecture-room, but, with slight variations, may be successfully employed for illustrating the whole theory of radiant heat.

ON THE PENETRATIVENESS OF FLUIDS.

By J. K. MITCHELL, M.D., Lecturer on Medical Chemistry in the Philadelphia Medical Institute.

[Continued from page 118.]

HAVING completed the first series of experiments on molecular infiltration, before entering upon an account of the second, reserved for the next number of the Journal, it may be refreshing both to experimenter and reader, in a very toilsome investigation, to pass in cursory review some of the almost infinite theoretic and practical suggestions, which flow from the facts before us.

The most striking generality, is that of the high power of penetrativeness of gases for organic molecular tissue, long known to be infiltrable by liquids, but until now not generally known to admit of any permeation, by at least, insoluble aeriform substances.
Secondly. We are struck with an unexpected result, the great power of gases to infiltrate each other. It has been long known, that aëriform substances confined in the same apartment, finally mingle uniformly, and that, even if the lighter one be placed above the other. To account for this, and some other facts of the same class, Mr. Dalton supposed that each gas, in reference to the vertical relation of its particles, stood in an attitude of independence of any other gas present, as much as if no such gas were confined along with it, no particle of one gas being supposed to rest on any particle of the other, the interstitial cavities of one gas being in fact a vacuum for the reception of the molecules of the other, each for each.

The power, however, of this infiltration being known, we are entitled to conclude, that the interspaces of gases are reciprocally occupied with a force similar, and probably equal to that which causes the imbibition of liquids by solids, and produces solutions of substances, even of the highest cohesive attraction. Solutions may now be esteemed infiltrations by solids and liquids of the tissues of each other, requiring, perhaps, only a fitness in size, rather than a chemical or cohesive attraction, for we see it subverting even the greatest cohesive power, and holding no apparent relation with known chemical affinities.

The atmosphere cannot any longer be considered as a mixture in the common acceptation of the term. Its gases penetrate each other interstitially with great mechanical force, so great as to defy all mere mechanical means of separation. It is an exemplification of solution.

When the particles of a solid separate and enter the tissue of a liquid, it is termed solution, when the liquid penetrates the solid, and the latter maintains its solidness, it is usually called infiltration, imbibition, absorption, &c. &c. The processes are perfectly alike in principle, the different names being expressive of that, and of certain accompaniments or effects also.

By means of our second generality, we are enabled satisfactorily to explain many phenomena not heretofore easily accounted for. Thus we understand how a gas or odour flows so rapidly through the whole tissue of a still atmosphere, and why some gases do so more speedily than others. An explanation is also given of the diffusion of odours, even against a draught, or current, and it accounts for this fact, among others, that brimstone thrown into the fire, is perceived by the olfactories, when the draught of the chimney is even perfect.
As proved in some experiments, already detailed, many solids are dependent on water for the power of penetrating tissues, or gases, &c., and it appears probable that many odorous solids, in particular, enter the atmosphere, solely by penetrating its hygrometric constituent. Thus, in solution, colouring matters readily, in certain cases, pass through membranes impenetrable without such aid, and every one has perceived the singular smell of a dusty road, after a shower, even at a very considerable distance. In a damp day, or immediately after rain, we more distinctly and vividly enjoy the fragrance of the parterre. Malaria seems to be dependent on the same cause for its penetration into the atmosphere, for every one knows the greater hazard of a residence in low damp situations, and the general unhealthiness of a damp summer, or autumn. As electricity is a great hydragogue, and substances in a negative state forcibly attract moisture, we might expect to find that season most damp and unwholesome in which the atmosphere maintained an electro-negative condition, and that driest and most healthful when it was electro-positive. Facts on this subject are yet to be created; but this one presents an aspect germain to the subject. Mr. William Mason, of Philadelphia, a philosophical instrument-maker, respected both for his ingenuity and correct moral character, informed me, that when, in 1820, the yellow fever existed here as an epidemic, he could not excite an electrical machine at his residence, in the infected district, although at his shop, which lay at some distance from it, the operation of the machine was sufficiently powerful*.

There exists between the lower surface of air, and the upper surface of water, a space possessed of powers analogous to those of the interspaces of substances in general. Along this plane, certain sub-

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* Aqueous gas penetrates the air more or less rapidly according to the temperature and moisture of the atmosphere. According to our law of progressive diminution, evaporation is slower in a moister atmosphere, and vice versa. The following experiment shows that aqueous gas has also its rate of penetration. A long tube, surmounted by a bladder, held water and mercury; the former of which being above, was in contact with the membrane. Although the mercury rose gradually as water escaped, yet some air found its way through the bladder, and occupying the upper part of the tube, separated the liquid and bladder from each other. Under such circumstances only, air and aqueous gas could reach its lower surface. Notwithstanding this, and the gradual increase of the quantity of air, the mercurial column continued to rise, showing that the rate of the penetration of aqueous gas, is greater than that of atmospheric air, by which it could not be counterbalanced. Curious to see the effect, I tied over the summit of the tube, a bag, holding carboxylic acid, which thus replaced the atmosphere. Almost immediately, the mercury gave intimation of descent, by losing its convex summit. It did fall, and carboxylic acid entered through the membrane, faster than the moisture had at any time escaped.
stances dart with surprising facility, losing as their particles separate, all cohesion, and acting repulsively. The oils are remarkable in this regard, and camphor exhibits, because of it, curious and agreeable movements, when thrown upon perfectly clean water*

But it is chiefly with reference to physiology, pathology, and practical medicine, that we see, in the foregoing experiments, things of much real value. They throw a particular light on the functions of respiration and cuticular absorption, and will probably lead to the employment of gaseous agents of cure with confidence and certainty.

The experiments on the mutual action of gases and liquids, show that although a gas may, when alone presented to a liquid for which it has no chemical affinity, penetrate its molecular cavities, yet, it will again leave it to join any gas whatever, which is brought into communication with the liquid. Thus carbonic acid or nitrous oxide readily penetrates blood or water, but returns from either into the air or any other gaseous substance, which contains no carbonic acid, or nitrous oxide. It is in this way, probably, that the oxygen disappears, and an exactly equal quantity of carbonic acid replaces it in the bronchial cells. Oxygen penetrates slowly the membranous tissue, to infiltrate and brighten the blood; carbonic acid is immediately formed, and being a gas differing from the remainder of the air yet in the air-cells, its tendency is to return, to penetrate that air, and thus escapes through the trachea along with it. The oxygen enters, because there is oxygen enough behind to permit that, and it is also an observed fact. The carbonic acid formed, makes its escape, because invited by the molecular tissue of atmospheric air. Keeping up any reference to known facts, we can scarcely doubt the truth of our explanation, or venture to adopt any other. The investigations of John Davy, and our careful repetition

* The best mode of examining this property of camphor is the following:—Take a piece of cork, a flat four-sided prism, and attach to its narrow sides, close to the ends, and diagonally opposite to each other, two small pieces of camphor. Resting with its broad surface upon a considerable plane of quite clean water, the apparatus will regularly rotate, and that either until the camphor is consumed, or the interspace is filled with that substance, or an emanation from it. Oil, by filling the space, immediately suspends the motion. If a cork be greased slightly, or camphorated at the end, it will move in a direction from that end, and with considerable velocity. The same thing happens when fine dry flour is attached, or when the butt end of the cork is dipped into ether or alcohol. A cavity being made in the upper surface of a floating cork, near the end, filled with ether, and connected by a cotton filament with the water, it will sail about a pneumatic trough for a considerable time, always moving towards the solid end. A little rudder being attached to the cork, and slightly inflected, the vessel may be made to sail entirely round a circular tub.
of his experiments, with others, fully as conclusive, leave no doubt
of the entire absence of carbonic acid in the blood.*

It must, therefore, be produced in one of two modes, either by
the penetration of oxygen into the blood, and its union there with
carbon, or the exit of carbon from the blood, to unite with oxygen
in the air-cells. Now, as carbon is one of the most fixed substances
in nature, and has not been proved capable of such transmission, we
are, if facts be our guide, compelled to adopt the other theory,
which is perfectly in accordance with the laws of gaseous infiltra-
tion. If it be asked how the carbonic acid is formed in the blood,
at so low a temperature, we reply, that carbonic acid is actually cre-
ated at a lower temperature, by the agency of infiltration, when
oxygen gas is imbibed by a piece of fresh cold charcoal. The dif-
fERENCE in the rate of permeation, is quited sufficient to account for
the escape of all the carbonic acid formed by the infiltrated oxygen.

Our theory does not account for the production of animal heat, but
it is presumed that no well-informed physiologist now seeks for it in
the action of the lungs, or the process of decarbonization. The simple
fact, that cold-blooded animals breathe without any increase of tem-
perature, proves that mere breathing to any amount will not produce
heat. Like all the other animal functions, that productive of heat is
dependent on a normal condition of blood, and is thus indirectly go-

dred by the act of respiration. As in cold-blooded animals, there
is no apparatus for producing heat, respiration does not in any way
influence their temperature. So in some of the cases quoted by John
Hunter, where blue-boys maintained a temperature preternaturally
great, the blood was very imperfectly decarbonized. In such cases
the calorific function found some novel stimulant.

Our experiments afford ready explanations of the effect of the
various gases when respired. Carbonic acid not only cuts off the
necessary supply of oxygen, but also penetrates into the blood, and
passes through the route of the circulation.

We perceive why nitrous oxide, so identical with oxygen in all
its chemical habitudes, should act so differently on the human sys-
tem. It penetrates at least sixteen times as rapidly, and probably
acts then solely as oxygen would do. Hence we see why it does not

* Having filled a phial with hydrogen gas, blood was received into it from a
vein, so as to exclude the agency of oxygen. When completely full of blood, the
phial was closed by sheet gum elastic, and immediately subjected to the action of
the air-pump. Under such circumstances, no gas of any kind could be immedi-
ately separated from the blood; but after coagulation was completed, a bubble
of air, about the size of a pin's head, was perceived beneath the membrane, and
that that was atmospheric air, or nitrogen, was proved by its long continuance
there, without apparent diminution or escape.

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exhaust us; for it not only acts upon excitability, but creates a fresh supply of it, so that its consumption is not felt. We can also easily see why an animal was destroyed in ten minutes by breathing hydrogen, while carbonic acid produced the same effect in two minutes. In Section I. Article IX. of his Physiological Researches, Bichat relates some curious exemplifications of the passage of gases into the blood-vessels through the lungs of living animals. For instance, hydrogen gas could be set on fire, as in bubbles it escaped from a remotely situated blood-vessel. As he had used some force, by means of a stop-cock and syringe adapted to the trachea, to throw in and retain the gas, he ascribes its entrance to that cause. We see however that though impulsion augments the effect, yet that it is existent independently of any *vis a tergo*. Gases not at all soluble in blood, will not pass without force, but that force is, in some degree, applied in every act of expiration. Those soluble in blood find ready entrance when not held back by the interstitial molecular power of the other gases with which they enter the bronchiæ.

The emptiness of the blood-vessels after death, or rather their fullness of gaseous matter, is no longer a case of difficult solution. Always present in the air-cells after death, air and carbonic acid gas must find a ready entrance into the emptied capillaries of the lungs, always prompt to dilate through the influence of the elastic matter which exists in and around them in the lungs. As any kind of air acts as a stimulant to the heart's cavities *, a gaseous circulation is kept up, and the æriform matter passes into the great channels of circulation.

It does not appear difficult to understand why so penetrating and poisonous a gas as sulphuretted hydrogen should often exist in the intestines without injury; for, being mixed up with other gases, its tendency to infiltration is greatly restrained. When undiluted, its diffusion through the whole system is fearfully rapid.

'Of all the gases,' says Dr. Ure, 'sulphuretted hydrogen is the most deleterious to animal life. A greenfinch plunged into air which

* In 1823, being engaged in dissecting a sturgeon, (Acipenser brevirostrum?) its heart was taken out and laid on the ground, and after a time, having ceased to beat, was inflated by mouth for the purpose of drying it. Hung up in this state it began again to move, and continued for ten hours to pulsate regularly, though more and more slowly. Left at 1 A.M. in slow motion, it was found next morning still and hard. When last observed in motion, the auricles had become so dry as to rustle as they contracted and dilated.

With the heart of a Testudo serpentaria, (Snapper,) I lately repeated the experiment, and found it beat well under the influence of oxygen, hydrogen, carbonic acid, and nitrogen, successively thrown into it. Water also stimulated it perhaps more strongly, but made its substance look pale and hydropic, and in one minute destroyed action beyond all known means of restoration.
contains only $\frac{1}{300}$ of its volume, perishes instantly. A dog of middle size is destroyed in air that contains $\frac{1}{300}$, and a horse would fall a victim to an atmosphere containing $\frac{1}{250}$.

Dr. Chaussier proves, that to kill an animal it is sufficient to make the sulphuretted hydrogen gas act on the surface of its body, when it is absorbed by the inhalents.

One of the objections to the belief in aerial poisons most confidently urged by antimiasmaticists, is the absence of all proof of absorption of gaseous matter, and indeed this was the sole difficulty of any real moment in the way of the triumphant establishment of the theory of miasm. Will it now be going too far to say, that this difficulty is removed, and that we can explain why miasmata affect persons so differently who reside in different apartments of the same house, or who live on opposite sides of the same street. Although being a very little nearer to the source or to the ground may not appear important, yet the difference of a few yards makes in either case a momentous distinction. Very near to its source a gaseous substance occupies a larger portion of the atmospheric space, and presents not only more matter, but matter less restrained by the molecular power of the air with which it is mingled. Not only is a greater quantity presented, but it is withheld from admission into the tissues by a slighter restraint.

As pressure unquestionably affects the rate of gaseous infiltration, a difference in the amount of atmospheric pressure will perhaps be considered of some importance, and assist in accounting for the general unhealthiness of low situation and intertropical latitudes.

Spontaneous evaporation has been long a subject of interest to the philosopher, and has not hitherto admitted of adequate explanation. Now we perceive, that in elevating moisture into the atmosphere, a very powerful agent is at work, one capable of subverting the cohesion even of solids, and of producing the continued infiltration of the atmosphere. Heat being also capable of destroying the attraction of aggregation, augments evaporation and interstitial infiltration. On this (I speak it hesitatingly) depends the power of steam. Caloric penetrates gases as they do each other, and escapes from them in exactly the same manner when substances which contain less of it invite its penetrant power in a new direction. Thus, for illustration, carbonic acid penetrates common air, and, so far as we know, will expand it, if constantly supplied, to an amount of power not yet measured. But so soon as another gas or penetrable substance is presented, it begins to withdraw from the air and to penetrate that. The hollow intestine used in one of our experiments
was powerfully inflated by its entrance, and yet as rapidly collapsed
when the gas was invited outwards by the presence of another gas
on its exterior. The resemblance of phenomena does not end here.
Each penetrates different substances with different degrees of facility,
and the quality of the surface is often to both as influential as the
character of the substance which affords it. The fact, the force, the
enlargement of bulk; the penetrativeness varying usually with the
substance and surface to be acted on, being however uniform rela-
tive to all gases, the constantly diminishing rate of progression, the
issuing out again when invited by new substances, or a vacuum, or
when mechanical compression is applied,—all afford evidence of ana-
logy as perfect as is perhaps ever offered to the view of philosophy.
We are struck with its resemblance to water in one respect.
Highly concentrated caloric invites the penetration of all liquids,
and perhaps of all solids; and thus, while held in solution by it,
they obtain a penetrativeness themselves which does not naturally
belong to them, and are elevated into the atmosphere in spite of
specific gravity, however high, or of atomic weight, however consi-
derable. Some facts, not yet sufficiently studied, lead me to the
perhaps hasty conjecture, that even the decomposing influence of
caloric is owing to this power. Water exercises it in that way in
some cases, such as that of acetate of lead.

The great length to which my remarks have unexpectedly ex-
tended, and the call of the printer, prevent me from going fully into
the consideration of the connexion of our experiments with patho-
ology and therapeutics. Their bearing on these departments of
medical science will furnish subject matter for a future essay. In
the mean time, we feel entitled to believe that we better comprehend
some of the phenomena of colic, tympanitis, and emphysema, and
see more clearly the cause of the value of certain methods of cure.

Bichat was among the first to produce the passage of air of various
kinds into the blood-vessels and cellular tissue of the lungs, by forcing
it into the air-cells and there confining it. Even when the blood-
vessels were full of froth, and emphysema became extensive, he
could perceive not the slightest laceration of the bronchiae. When
the impulsion was moderate, the air passed only into the blood-
vessels; when more violent, its presence became manifest in the
cellular tissue. In certain cases referred to by authors, violent ex-
ertion, laborious respiration, and severe flatulency of the intestines,
have forced air into the blood-vessels and cellular tissue. Colic has
produced also tympanitis, and few practised physicians are ignorant
of the fact, that great gaseous distention of the abdomen has disap-
peared without the apparent escape of any wind. When we consider attentively the laws by which are regulated the entrance and exit of gases under the action of their penetrativeness, we feel scarcely at a loss to understand these phenomena.

The prodigious accumulation of gas in the stomach and bowels, in hysteria and epilepsy, may be explained, by supposing the air, which exists by infiltration in every part of the animal economy, to be forced by the violent compression of spasmodic action into the hollow viscera, where already existent gases invite its entrance. In some experiments on the effect of certain gases on living cavities, made by my ingenious friend Dr. Finley, their escape was so rapid as to create surprise.

The establishment of the fact of the penetration of liquids, each according to its peculiar rate, and the modifications of that rate dependent on extrinsic force, such as impulse or invitation, electricity, &c. teach us many valuable lessons both in philosophy and medicine. Especially I would invite attention to the cause of the remedial influence of pressure, as auxiliary to other means of cure.

Recapitulation.—1st. Substances formed of organic matter are generally penetrable by gases of all kinds, and by several, if not by all liquids.

2d. Each animal or vegetable tissue is differently penetrable as to time by different fluids.

3d. But all fluids penetrate any particular substance at rates susceptible of being ascertained. The gases retain the relation observed by reference to one substance in all other cases. Whatever may be the greater or less penetrability of any given tissue, the gases penetrate it, relatively to each other, according to the ratio observed in experiments on other tissues.

4th. The ratio is not so uniform in the instance of denser fluids. Liquids, though rateable with regard to permeation of any given substance, do not act similarly on different organic substances. Thus water penetrates most, if not all animal tissues, better than any other liquid whatever, and consequently passes through them to accumulate in any of its own solutions, and in alcohol or ether; while these two latter substances penetrate gum elastic with more facility than either water or its solutions. Therefore, with regard to gases, the ratio of penetration depends on them alone; while, in the case of liquids, it depends on the joint agency of both liquids and tissues.

5th. When the quantity of the fluids is limited, there is a gradu-

ally diminished rate of progression as the infiltration proceeds. It is proportional to the state of dilution, and ceases when the substances have become, on both sides of the membrane, of uniform condition, unless some extrinsic power is then operative.

6th. The power of the penetrativeness is very considerable, being certainly superior to that of two, and possibly equal to more than that of forty atmospheres.

7th. Penetrativeness acts not only on organic tissues, but also on gases and liquids, and with apparently equal power on all. For, after permeating a membrane, the gas or liquid goes on into the molecular tissue of the gas or liquid beyond, and no pressure which the membrane can bear acts as a restraint on the progression.

8th. Although of such high mechanical power, the penetration can be, to a certain degree, affected by extrinsic agency. Thus pressure or attraction will cause permeation, where it would not otherwise take place, as when a single gas or liquid travels not only through, but beyond a membrane, where there exists nothing to imbibe it, which it would not do, unless subjected to propulsion. Electricity, possessed of hydragogue powers, acts on water in a similar manner, causing it to collect on either side of an animal membrane, at pleasure, although no other liquid is there to receive it.

9th. The penetrativeness of gases for each other seems to vary in velocity, but not in force.

10th. Reference to the abovementioned laws and modifying agencies enables us to explain many phenomena hitherto imperfectly understood. We, by means of them, comprehend the uniform constitution of mixed gases in any vessel, or in the atmosphere, notwithstanding the greatest difference in specific gravity. It explains the diffusion of odours, the nature and power of spontaneous evaporation, and the probable nature and progression of caloric under slow conduction. It affords us new views of the theory of respiration, and accounts, in that process, for some well-ascertained facts, for which there previously existed no adequate explanation.

It shows us how emphysema and tympanitis may happen without secretion of gases, or lesion of tissue, and how a spontaneous cure may be produced. It leads to the probability of the existence of gaseous matter of very various kinds in almost every part of the animal frame, resident there molecularly, and not en masse, but susceptible of being collected into mass in the great cavities or the cells of the tissue, or the blood-vessels, by mechanical or electrical influence, or the attractive interstitial agency of other masses of air.

It teaches the important truth, that water is the great general
infiltrator and diluent, a knowledge of whose habitudes will be thus rendered both clearer and more useful.

Before closing my remarks, I am happy to be enabled to say, that a considerable number of my medical friends visited my laboratory, and saw for themselves the verifications of my statements. I solicited their observation, both for the confirmation of my own impressions, and for the greater readiness of reception which the public always affords to facts which have appeared in a similar light to several different individuals of adequate judgment.

In my next I hope to present a table of the rates of penetrative-ness of liquids for animal membranes. I hope also to ascertain the amount of force. On the relation of the respirable gases to the blood, and other liquids, I possess already many interesting facts, which will be then promulged.

*Philadelpdia, September 15th, 1830.*

Since the foregoing paper was sent to the editor of this Journal, I have had an opportunity of reading M. Dutrochet’s short essay, entitled ‘*Nouvelles Recherches, &c.*’ In it I find that the author has discovered his mistake relative to the action of acids in general, but has fallen into one quite as important, respecting the agency of diluted sulphuric acid. He now considers it a *nullifier* of endosmose, instead of a promoter of exosmose, being not only itself inactive, but the cause of inactivity in other solutions. Feeling confident of the power of diluted sulphuric acid to receive as much water as the animal membrane could convey, I, in conjunction with Professor Finley, carefully repeated our experiments on that substance. In every case, where the solution exceeded 1°, (Beaumé,) it was ade-quate to the occupation of as much water as could be presented by the membrane. At 2°, 11°, and 25°, the acidulous liquid gave the same *rate* of aqueous infiltration as did alcohol, ether, &c. A solution of sulphate of soda, at 11°, and at 3° Beaumé, and a solution of ammonia at 40° centesimal alcometre, being infiltrable by water, at a rate not less than that of the animal membrane, of course afforded, when compared with that liquid, exactly the same results. Although all these substances gave evidence of having been con-temporaneously transmitted through the membrane, yet the quantity, easily appreciated chemically, was not so great as to make a sensible difference in the altitude of the column, whose rise represented the transmission of water. When, by the entrance of a considerable quantity of water, the acid was so far diluted as to intermingle with it more slowly than the membrane could present it, a rapid diminu-
tion of ascent ensued. At length, so little was received, as to barely compensate for the effect of gravitation. Finally, the diminished power of reception being below the effect of gravitation, the liquid descended again, and the two columns reached a common level. Seeing these causes of change, we can estimate the rate solely by observing the time taken to traverse a short space, and that immediately at the commencement of the experiment. Unless the less penetrant liquid be of much more power of reception than is actually necessary, its dilution soon destroys its adequacy, and lessens the apparent rate, just as, in forming solutions, we perceive a great diminution of solvent power as the point of saturation is approached. In addition, when both liquids are traversing the membrane at the same time, there is a progressive approach to a common state, favourable to repose. M. Dutrochet, therefore, by observing the effect of solutions of different strength, in a considerable length of time, (an hour and a half,) obtained results, not the act of the membrane, but of the solution—not the maximum effect of the tissue, but the constantly diminishing action on water of a gradually diluted solution. His results might therefore have been anticipated by calculation; for, as water dissolves less and less, in a given time, of any soluble substance, so a soluble substance acts on water presented to it in a steadily declining ratio. When the demand for water is above the powers of supply through the membrane, the rate will be regulated solely by the water and membrane, and is the same for a great variety of substances. When the demand becomes less than the supply, the case is one of simple solution, with which the membrane may be supposed to have no connexion. It is then acting the part of a still surface of water.

The following facts, ascertained at an early period of this investigation, will place this principle in a strong light. An inverted siphon, such as already described, was filled with atmospheric air, a portion of which, by placing thirty-four inches of mercury in the long limb, was confined in the shorter one. There being here the same gas on both sides of the membrane, the current set in the direction given by impulsion, and the long column fell—

- $\frac{3}{8}$ths of an inch in 2 hours and 30 minutes, or 50 min. per $\frac{1}{8}$th.
- $\frac{3}{4}$ths more in 2 hours and 39 minutes, or 53 per $\frac{1}{8}$th.
- $\frac{3}{8}$ths more in 2 hours and 26 minutes, or 48$\frac{3}{4}$ rds per $\frac{1}{8}$th.
- $\frac{1}{8}$th in 1 hour and 1 minute, or 61 per $\frac{1}{8}$th.

$1\frac{1}{4}$ inch in the whole in 8 hours and 36 minutes.
At this period of the experiment, when the mercurial column stood two inches and a half lower, proportionally, than at the commencement, a vessel containing carbonic acid gas was placed over the shorter limb. Immediately the long column began to rise—

\[
\frac{3}{8}\text{th} \text{ of an inch in } 20 \text{ minutes, or } 10 \text{ minutes per } \frac{1}{8}\text{th.} \\
\frac{3}{8}\text{th} \text{ more in } 10 \text{ or } 10 \text{ per } \frac{1}{8}\text{th.} \\
\frac{1}{8}\text{th in } 12 \frac{1}{2} \text{ or } 12 \frac{1}{2} \text{ per } \frac{1}{8}\text{th.} \\
\frac{1}{8}\text{th in } 37 \frac{1}{2} \text{ or } 37 \frac{1}{2} \text{ per } \frac{1}{8}\text{th.} \\
\frac{1}{10}\text{th in } 60 \text{ or } 120 \text{ per } \frac{1}{8}\text{th.}
\]

The column appearing stationary, was left nine hours unobserved; at the end of that time—

\[
\frac{3}{8}\text{ths were lost in } 9 \text{ hours, or } 41 \frac{1}{2} \text{ min. per } \frac{1}{8}\text{th.} \\
\frac{3}{8}\text{ths in } 3 \text{ hours } 21 \text{ minutes, or } 40 \frac{1}{10} \text{ min. per } \frac{1}{8}\text{th.} \\
\frac{3}{8}\text{ths in } 1 \text{ hour } 24 \text{ minutes, or } 42 \text{ min. per } \frac{1}{8}\text{th.}
\]

At this moment the mercury came into contact with the membrane, all the gas being excluded.

The uniformity of descent, and the progressively diminishing rise, are striking facts. It will also be observed, that the carbonic acid seemed to cease action, because of a weight of nearly thirty inches of mercury; whereas, in another experiment, sixty-three inches were readily driven upwards. We therefore easily perceived the cause of Dutrochet’s mistake.

One other nullifier of endosmose is thought by Dutrochet to exist. A solution of hydro-sulphuret of ammonia at first quickened, and then totally arrested the motion of the fluid in the stem of his endosmometer; for which he accounts by supposing the final production of sulphuretted hydrogen in the solution, and the extinctive agency of that.

The great activity of gaseous sulphuretted hydrogen, on which Dutrochet made no experiments, led me to suspect that its solution was gifted with considerable penetrant power, and by thus counter-balancing the amount of penetrating water, appeared to act in arrest of motion, presenting just such a case as we witnessed when comparing together olefiant gas and arsenuretted hydrogen. For verification, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with water, and scarcely any motion observed. A similar solution, enclosed by an animal membrane in a wide-mouthed bottle, was placed in a vessel of pure water, mouth downwards. In this instance the membrane gave no sign of inflection at first, but after several hours showed a slight bend inwardly. In both these cases the portion of liquid, originally clean water, when tested by acetate of lead, afforded the deep
black precipitate, indicative of the presence, abundantly, of sulphuretted hydrogen.

In a second experiment, with a solution of sulphuretted hydrogen enclosed in a bottle, the water placed in the outer vessel contained the slightest trace of acetate of lead. Scarcely was the bottle immersed before the precipitation of the lead commenced. Finally, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with alcohol confined in its shorter limb. In this instance, and in every repetition, the movement was manifested towards the alcohol, the rise of which showed that the penetrative power of liquid sulphuretted hydrogen is somewhat greater than that of water, and of course much greater than that of alcohol. These experiments were made with extraordinary care, because by them seemed to hang the fate of this whole question of principle. The whole doctrine of regular rate of penetration, &c. must fall to the ground if my trials had been confirmatory of the observations of M. Dutrochet.

The totally different results, as to the force of penetration, at which M. Dutrochet and myself have arrived, render necessary a few words of explanation.

It will be conceded that the fairest mode of estimating the force is when the liquid is fresh and the process just well begun. The altitude of the highest column of mercury which it can raise will represent its power, and that column should, if possible, be laid on it at once. In this manner I proceeded, and found that both bladder and gum elastic were broken by a column higher than sixty-three inches, although, just before giving way, the column was rising. It could rise solely by the power of penetration, no other known agent of motion being present. But M. Dutrochet, laying on a column less than sufficient, left his apparatus to raise that column for a day or two, until the process of elevation ceased. The height then reached he considered as representing the power of endosmose. The attentive reader will readily perceive, in this plausible experiment, the same error which deprived the facts, as to time, of value. The solution had become diluted, and the water on the other side had become impregnated, and, independently in a great measure of the weight of the column, the causes of production of penetrating currents had ceased, and these beautiful experiments reported, not the weight which could be raised, but the time required by such a solution to distribute its qualities uniformly, or nearly so, on both sides of the membrane. Left in that state the column descends, thus evincing the cessation of penetration, not its forcible repression.
This is well proved by his latest experiment, in which having raised a column of mercury by the penetration of water into a solution of gum Arabic to twenty-eight inches, and while still rising, he replaced the external water by a solution of gum Arabic, when an immediate ascent was observed. The substitution of clean water again caused an elevation of the column.

On the whole, captivating as is the method, and elegant as are the experiments, of this little volume of M. Dutrochet, it does not bring additional support to his doctrine of endosmosis. Yet whatever may be the issue of the experimental investigation to whose rigid scrutiny this most important subject is committed, the philosopher and physician can scarcely find language adequately to express the obligation, the high obligation, under which science has been laid by the elegant labours of M. H. Dutrochet. In him we discover the punctum saliens of a principle which is the master spirit of animal and vegetable motion, the ruling power of chemical science, the governing influence of atmospheric composition, the presiding genius of respiration, circulation, and nutrition, the cause of disease, and the restorer of health. But whatever may be now his fame, how little is it compared to that which may be anticipated for him by one who takes even a careless view of the mighty field of novel observation just redeemed from the rich wilderness of nature! This tribute is paid the more unhesitatingly because it is due, and because I have so freely criticised and censured where the cause of science and truth demanded severity. It is in great men, and in great discoveries, that blemishes are most ungraceful and most injurious. The very magnitude and extent of the principle for whose detection we must thank Dutrochet, give a fearful importance to the slightest coextensive errors.

September 18th, 1830.
ANALYSIS OF BOOKS, &c.


This tribute to the memory of one of the most gifted men and distinguished philosophers of the age, has been printed solely for private distribution. It has almost the interest of an autobiography, having been drawn up by a gentleman who had the advantage of a long and intimate acquaintance with him, from some short memoranda of Dr. Young's own writing, in the possession of a near connexion. The author modestly states, that 'having never been engaged in the pursuits of accurate science, he feels himself incompetent to give more than an imperfect sketch, which he trusts to see filled up hereafter by an abler hand.'

No apology, it is presumed, will be necessary for transferring to these pages the substance of this account of Dr. Young, who, from his connexion with the Royal Institution, as one of its professors, and as the editor of the first series of its Journal, independent of his claims as a scholar and philosopher of the first class, especially merits distinguished notice in this work.

Thomas Young was born at Milverton, in Somersetshire, on the 13th of June, 1773. His parents were both of them Quakers, and of the strictest of that sect; his mother was a niece of Dr. Richard Brocklesby, a physician of eminence, well known from his connexion with the distinguished literary and political characters of his time, and who numbered among his most intimate friends, Johnson, Burke, and Windham.

To the influence of the early impressions of the Quaker tenets, Dr. Young 'was accustomed to attribute, in some degree, the power he so eminently possessed of an imperturbable resolution to effect any object on which he was engaged, which he brought to bear on everything he undertook, and by which he was enabled to work out his own education almost from infancy, with little comparative assistance or direction from others.' The earliest years of Dr. Young were chiefly passed in the family of his maternal grandfather, Mr. Robert Davis, of Minehead, who, in the midst of mercantile avocations, had cultivated a taste for classical literature, with which, by earnest endeavour, he seems to have imbued the mind of his grandson, who appears to have been a forward if not a precocious child. It is said that he could read with fluency when he was two years old; and soon after this, in the intervals of his attendance on a village schoolmistress, he committed to memory a number of English verses, and even was taught to recite some Latin poems, the words of which he
retained without difficulty, although unacquainted with their meaning. Before he was six years old, he was sent to a school kept by a dissenting minister at Bristol, where he remained about a year and a half, and became essentially his own instructor, and had generally studied the last pages of the books used before he had reached the middle under the eye of his inefficient master.

It has been remarked, 'that the early quickness with which learning is imbibed, is not always the indication of permanent ability; facility of acquiring does not in general establish a power of retention; whilst what is received with difficulty, is frequently preserved and digested in the mind.' The case of Dr. Young, however, was one of those happy exceptions to this remark; and in none of those extraordinary instances recorded by Baillet in his work 'sur les Enfans célèbres par leurs Études,' is there a more remarkable instance of the promise of youth being realized in the man.

To one of those accidental circumstances, which, though they do not create a peculiar genius, yet very often determine its bent, may be attributed that love of science which distinguished Dr. Young; and which (says his biographer) 'had probably no small influence on the issues of his future life.'—'His father had a neighbour, a man of great ingenuity, by profession a land-surveyor; and in his office, during his holidays, he was indulged with the use of mathematical and philosophical instruments, and the perusal of three volumes of a Dictionary of Arts and Science. These were to him sources of instruction and delight of which he seemed never to be weary.'

In his visits to this neighbour, Young had acquired some knowledge of the art of land-surveying, and used to amuse himself in his walks by measuring heights with a quadrant. In 1782 he was placed at the school of Mr. Thompson, at Compton, in Dorsetshire, where he went through the ordinary course of Greek and Latin, with the elements of mathematics; here also he had access to a moderate miscellaneous library, and by rising earlier and sitting up later than his companions, with the assistance of a schoolfellow, he acquired some knowledge of the French and Italian languages.

Botany having about this time engaged his attention, and desiring to possess a microscope for the purpose of examining plants, he attempted the construction of one from the descriptions of Benjamin Martin. This led him to optics; and having procured a lathe in order to make his microscope, like most young experimenters, he forgot or neglected science, for a time applied himself to the acquisition of manual dexterity, and every thing gave way to a passion for turning, until, falling upon a demonstration in Martin's Philosophy, which exhibited some fluxional symbols, he was not satisfied until he had read and mastered a short introduction to the doctrine of fluxions.

Before he quitted school, a Hebrew Bible being left in his way, he began by enabling himself to read a few chapters; this led him to the study of the other principal oriental languages; and on quit-
ting Mr. Thompson's, at the age of fourteen, it appears that he was more or less versed in Greek, Latin, French, Italian, Hebrew, Persian, and Arabic; and had laid the foundation of that calligraphic skill for which he was afterwards so remarkable, and in which he rivalled even the neatness and beauty of the pen of Porson.

He was about this time attacked by symptoms of what his friends feared to be incipient consumption, but by the attention of his uncle Dr. Brocklesby, and Baron Dimsdale, his health was restored; his indisposition scarcely interrupted his studious labours, and it is said that he merely relieved his attention by what to him stood in the place of repose—a course of Greek reading in such authors as amused the weariness of his confinement.

In the year 1787, he met, at the house of a relation, a friend of Mr. David Barclay, of Youngsbury, in Hertfordshire, who was then wishing to form an arrangement for the education of his grandson; and it was at length agreed that the youths should pursue their studies together, under a private tutor in Mr. Barclay's house. The tutor, however, did not come, and Young, who was only a year and a half older than his companion, took upon himself provisionally the office of preceptor. They were afterwards joined by Mr. Hodgkin, author of the 'Calligraphiae Graecæ,' who was of somewhat maturer years, and then seeking to perfect himself in the higher branches of classical attainments. But Young did not relinquish the task he had undertaken, and continued to be the principal director of the studies of the whole party.

Thus passed the five years from 1787 to 1792, the summers being spent in Hertfordshire, the winters in London, and with no other assistance than that of a few occasional masters, when in London, he had rendered himself singularly familiar with the great writers of antiquity, keeping ample notes of his daily studies. 'His reading was not,' says his biographer, 'for the purpose of merely gaining words and phrases, and the minuter distinctions of dialects, but was invariably also directed to what was the end and object of the works he laboured through;' he had drawn up an admirable analysis of the various conflicting opinions of the ancient philosophers, and it is probable that the train of thought into which this led him was not without its effect in mitigating his attachment to the peculiar views of the Quakers. He had now acquired great facility in writing Latin; composed Greek verses, which were well received by the distinguished scholars of the day, and applied himself assiduously to the higher mathematics. To the studies of botany, zoology, and especially of entomology, he at the same time paid considerable attention.

In the winters of 1790 and 1791, he attended the chemical lectures of Dr. Higgins, and having previously prepared himself by reading on the subject, he began to make simple experiments of his own. But he is said to have been at no period of his life fond of repeating experiments, or even of originating new ones; 'considering that, however necessary to the advancement of science, they
Memoir of the Life of Dr. Thomas Young.

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demanded a great sacrifice of time, and that when the fact was once established, that time was better employed in considering the purposes to which it might be applied, or the principles which it might tend to elucidate.' At Dr. Brocklesby's recommendation, and under his superintendence, he now directed his views to the studies necessary for the practice of physic, and made to him a regular report of his literary and scientific pursuits. The Doctor lived in intimacy with Mr. Burke and Mr. Windham, and having communicated to them some of his nephew's Greek translations, he was introduced to those two distinguished persons. Mr. Burke is said to have been so greatly struck with the reach of Young's talents, and the extent of his acquirements, and more particularly by his great and accurate knowledge of Greek, that he was in no small degree indebted to the good offices of that eminent statesman for the interest which his uncle afterwards took in his future settlement in life.

'It may probably be considered that it was at this period his character received its development. He was never known to relax in any object which he had once undertaken. During the whole term of these five years, he never was seen by any one, on any occasion, to be ruffled in temper. Whatever he determined on he did. He had little faith in any peculiar aptitude being implanted by nature for any given pursuits. His favourite maxim was, that whatever one man had done, another might do; that the original difference between human intellects was much less than it was generally supposed to be; that strenuous and persevering attention would accomplish almost anything; and at this season, in the confidence of youth and consciousness of his own powers, he considered nothing which had been compassed by others beyond his reach to achieve, nor was there anything which he thought worthy to be attempted which he was not resolved to master.'

His biographer thinks, with justice, that 'this self-conducted education in privacy was not without its disadvantages—that though the acquirements he was making were great, he was not gaining that which is acquired insensibly in the conflict of equals in the commerce of the world—the facility of communicating knowledge in the form that shall be most immediately comprehended by others, and the tact in putting it forth that shall render its value immediately appreciated.'

His first communications to the press were made in 1791, through the medium of the 'Monthly Review,' and the 'Gentleman's Magazine,' and towards the end of 1792 he established himself in lodgings in Westminster, where he resided two years, attending the lectures of Baillie and Cruickshank on anatomy, and was, during that period, a diligent pupil of St. Bartholomew's Hospital.

In 1793, he made a tour in the west of England, principally to study the mineralogy of Cornwall; and about this time the Duke of Richmond, then Master-General of the Ordnance, who had long been a friend of his uncle, offered him the situation of assistant-secretary in his house. Mr. Burke and Mr. Windham recommended him to proceed to Cambridge and study the law, but his own predilec-
tions and habits decided him, upon due consideration, to determine in favour of the practice of physic, as most congenial to his scientific pursuits, and to which the position occupied by his uncle seemed to offer a favourable introduction.

In this year he communicated to the Royal Society his Observations on Vision, and his Theory of the Muscularity of the Crystalline Lens of the Eye, which became the object of much discussion; John Hunter laying claim to having previously made the discovery. Dr. Young was soon after elected a fellow of the Royal Society, when he had just completed his twenty-first year; and in the autumn of 1794 he went to Edinburgh, and attended the lectures of Doctors Black, Munro, and Gregory.

He now separated himself from the Society of Quakers, and, amidst the most active pursuit of his medical, scientific, and classical labours, still found leisure for cultivating those arts in which his early education had left him deficient. The versatility of his genius reminds us of what has been recorded of the Admirable Crichton. It is said that 'everything, be its nature what it might, was with him a science; and that whatever he followed, he followed scientifically. Of music he was extremely fond, and of the science of music he rendered himself a master. He had at all times great personal activity, and in youth he delighted in displaying it.'—'He diversified his graver studies by cultivating skill in bodily exercises; took lessons in horsemanship, in which he always had great pleasure; and practised, under various masters, all sorts of feats of personal agility, in which he excelled to an extraordinary degree.' As a characteristic anecdote, it is recorded that, in instructing himself in a minuet, he made it the subject of a diagram.

Toward the close of 1795 he went to the University of Göttingen, where he took his doctor's degree, and excited the wonder of that laborious school by his extraordinary attainments and almost incredible industry. Here he composed a treatise 'De Corporis Humani Viribus Conservatricibus,' leaving few volumes unconsulted which had any connexion with the subject he was treating. He had purposed visiting Italy previously to his return to England, but was prevented by the victories of the French; he therefore proceeded to Dresden, for the purpose of studying the works of Italian art in the galleries there, and of comparing what he saw with that which he had learnt of them from the lectures of the professors at Göttingen. He also made a short visit to Berlin.

'During his residence in Germany he gained a very general and accurate acquaintance with its language and literature, which he kept up throughout his life; he remarked that he found in Germany a love of new inventions, singularly, and somewhat pedantically, combined with the habit of systematizing old ones, and of giving an importance to things in themselves trifling, which in his case rather confirmed an original habit of dwelling on minutiae more than his subsequent experience led him to think was advantageous.'
On his return to England he entered himself of Emmanuel College, Cambridge, of which Dr. Farmer, an intimate friend of his uncle, was then master. He proceeded to take his regular degrees in physic in that university, but did not attend any of the public lectures, contenting himself with pursuing the various studies in which he was engaged, living on terms of intimacy with the most highly-gifted members, and discussing subjects of science with the professors, but finding no rival in the variety of his knowledge, and few competitors in most of its branches.

Dr. Brocklesby died in 1797: part of his fortune, his books, his pictures, and his house, he left to Dr. Young, who now found himself in circumstances of independence, surrounded by a circle of distinguished and highly valuable friends, which he continued to prize and to enjoy through life. When his residence at college was completed he settled himself as a physician in London, in Welbeck Street, where he continued to reside during twenty-five years.

In 1801, Dr. Young was appointed Professor of Natural Philosophy in the Royal Institution, where he continued for two years to lecture alternately with Sir H. Davy. In 1802, he published his 'Syllabus, a Course of Lectures on Natural and Experimental Philosophy, with Mathematical Demonstrations of the most important Theorems in Mechanics and Optics.' This syllabus contained the first publication of his discovery of the general law of the Interference of Light, being the application of a principle which has since been universally appreciated as one of the greatest discoveries since the time of Newton, and which has subsequently changed the whole face of optical science *. As a lecturer at the Royal Institution Dr. Young was not eminently successful, for though his lectures were replete with interesting original matter, he was not happy in conveying it in a sufficiently intelligible manner to the capacities of a mixed audience, consisting in a great degree of persons of fashion and of the world. Dr. Young's style and manner were quite opposite to those of his eminent colleague, Davy; he was compressed and laconic, and presumed his audience better instructed in the arcana of science than such an assembly could possibly be: it has even been said that it would hardly have been possible for men of science to have followed him at the moment without considerable difficulty.

At this period Dr. Young became, jointly with Davy, editor of the Journal of the Royal Institution; the first volume, and part of the second, were published under his superintendence. It was also at this time that he gave his two Bakerian Lectures on the subject of Light and Colours, to the Royal Society. Developing the law of interference, and entering into all the details of the theory to which it leads; dwelling upon the difficult points, at the same time, with

* It was not until the year 1827, that the importance of this law could be said to be fully admitted in England: it was in that year that the Council of the Royal Society adjudged Count Rumford's medal to M. Fresnel, for having applied it, with some modifications, to the intricate phenomena of polarized light.
more candour than might have been consistent with his object, had he been anxious to obtain proselytes.

'The summer of 1802, he accompanied the present Duke of Richmond, and his brother Lord G. Lennox, in his medical capacity, to Rouen, and in an excursion from thence to Paris, was first present at the sittings of the National Institute, at that time attended by Napoleon; where he made the acquaintance of several leading members of that distinguished body, into which he himself was eventually elected. On his return he was constituted Foreign Secretary to the Royal Society, an office which he held during life, being long their senior officer, and always one of the leading and most efficient members of their council.'

In 1804, he married Eliza, daughter of J. P. Maxwell, Esq., of Cavendish Square,— an union to him productive of uninterrupted happiness during the remainder of his life. At this time he resigned his professorship in the Royal Institution, from an erroneous impression that it would be likely to interfere with his success as a medical practitioner. The remarks of his biographer on this occasion must not be withheld.

'His resolution at that juncture was to confine himself for the most part to medical pursuits, and to make himself known to the public in no other character. But he had resolved on that which to him was impossible. He never slackened either in his literary or philosophical researches. He was always aiding, and always willing to be the counsellor of any one engaged in similar investigations. He was living in the first circles of London, amongst all who were the most eminent. The nature of his habitual avocations was necessarily well known; and, therefore, in putting forth his non-medical papers separately and anonymously, he was making a fruitless as well as voluntary sacrifice of the general celebrity to which he was entitled; and shrinking, as it were, from the cumulative reputation which he must otherwise have enjoyed, he waived, in some degree, the advantage which is given by a great name towards the pursuit of even professional success.'

In 1807, Dr. Young published his 'Course of Lectures on Natural Philosophy and the Mechanic Arts,' in two volumes, 4to.; a work of first-rate merit, which cost him nearly five years' labour to perfect. The mass of references contained in the second volume, to those works which the student engaged in minute inquiries in any branch may consult with advantage, affords evidence of the extensive reading and industry of this eminent philosopher. Owing to the failure of the booksellers engaged in the publication of Dr. Young's Lectures, the immediate sale of the work was so greatly injured that it did not repay the expenses of the publication. Indeed, for some years, its great merits were not so extensively appreciated in England as on the Continent; but at length justice has been done to it in the country which gave it birth,—it is a mine to which every one engaged in scientific pursuits must have recourse with advantage, and it is no less true that 'it contains the original hints of more things since claimed as discoveries, than can perhaps be found in a single production of any known author.' 'One of the men most distinguished for science in Europe has been known to say, that if his library were
on fire, and he could save only one book from the conflagration, it should be the Lectures of Dr. Young.'

In 1810, Dr. Young was appointed Physician to St. George's Hospital; but his private practice, though respectable, was never extensive. His biographer has, we think, pointed out the true cause with great discrimination: 'In his profession, his published labours would prove him to have been of the most learned of scientific physicians, and his judgment and acuteness were equally great; but in the practice of medicine he was not one of those who were likely to win the most extended occupation among the multitude. He was adverse to some of the ordinary methods by which it is acquired. He never affected an assurance which he did not feel, and had, perhaps, rather a tendency to fear the injurious effects which might eventually result from the application of powerful remedies, than to any overweening confidence in their immediate efficacy. His treatises bear the same impress. That on Consumption is a most striking instance of his assiduity in collecting all recorded facts; and his abstinence from drawing inferences from isolated cases, or putting forth that which he did not feel was established with certainty. Possibly he herein was an example that increase of knowledge does not tend to increase of confidence, and that those whose acquirements are the greatest meet in the progress of their investigations with most that leads to distrust.'

Dr. Young had previously given a course of lectures on the Elements of the Medical Sciences at the Middlesex Hospital, of which a syllabus was published in 1809. These lectures, he himself said, were little frequented, 'on account of the usual miscalculation of the lecturer, who gave his audience more information in a given time than it was in their power to follow.'

In 1813, he published his 'Introduction to Medical Literature, including a System of Practical Nosology,' a work of considerable labour and of the highest practical utility. To this work he prefixed a preliminary 'Essay on the Study of Physic,' partly founded on that of the German Professor Vogel, in which is contained his own conception of the qualities requisite to constitute a well-educated physician, by which it will appear that his notion of the character was elevated above the ordinary standard of humanity: 'he enumerates nearly every possible quality of which man could wish, but of which few could hope, the attainment.'

Dr. Young was a frequent contributor to the Quarterly Review, having been induced, at the instance of his friend Mr. George Ellis, to furnish articles on medical subjects. His communications, however, soon branched into other lines, connected with the higher departments of science, and containing frequently more of original research than of immediate criticism. In the catalogue of his writings, which accompanies this memoir, will be found a list of his papers in that journal. We shall only here mention an admirable philosophical dissertation on the Structure of Language, contained in the
Analysis of Books, &c.

review of Adelung's Mithridates, Vol. X., October, 1813. This is remarkable, as it was the immediate means of leading him to the investigation of the lost literature of Ancient Egypt. The account of his discoveries on this subject is given in the words of his biographer, because an unjust attempt has been made to wrest from Dr. Young the merit of having first discovered a key to the hieroglyphics.

‘In the year 1814, Sir William Rouse Boughton had brought with him from Egypt some fragments of papyri, which he put into the hands of Dr. Young; the fragment of the Rosetta Stone having about this time been deposited in the British Museum, and a correct copy of its three inscriptions having been engraved and circulated by the Society of Antiquaries. Dr. Young first proceeded to examine the Enchorial Inscription, and afterwards the sacred characters; and, after a minute comparison of these documents, he was enabled to attach some “Remarks on Egyptian Papyri, and on the Inscription of Rosetta,” containing an interpretation of the principal parts of both the Egyptian inscriptions on the pillar, to a paper of Sir William Boughton’s which was published by the Society of Antiquaries in 1813, in the 18th volume of the Archaeologia.

‘Dr. Young now found that he had discovered a key to the lost literature of Ancient Egypt. He had occupied himself, though without deriving from it the assistance he at first expected, in the study of the Coptic and Thebaic versions of the Scriptures; but having satisfied himself of the nature and origin of the Enchorial character, he produced the result to the world anonymously in the Museum Criticum of Cambridge, part vi., published in 1815; being then determined to prosecute the discovery, but at the same time abstaining from claiming it in a more substantive form, from the resolution he had previously taken to be known only as a medical author.

‘The labour he bestowed on these investigations, and the minuteness and accuracy with which he copied the papyri and compared the materials which came into his hands, would be nearly incredible to those who had not access to him whilst employed on this pursuit.

‘In 1816, he printed and circulated two additional letters relating to his hieroglyphical discoveries and the inscription of Rosetta; the first addressed to the Archduke John of Austria, who had recently been in this country, the other to M. Akerblad. These letters announce the progress of the discovery of the relation between the Egyptian characters and hieroglyphics, forming the basis on which Dr. Young continued his inquiries, as well as of the system afterwards carried further in its details by M. Champollion, whose attention had long been directed to similar studies, and in which he has since so greatly distinguished himself. The letters were first published when reprinted in the seventh number of the Museum Criticum, in 1821; and were, with the former letters in that work, beyond all question or dispute, the earliest announcement of the discovery of a key to a character which had remained uninterpreted for ages.’

The whole results of his discoveries on this subject were first brought out in a perfect and concentrated form in the article Egypt, published in the Supplement to the Encyclopaedia Britannica, to which work Dr. Young furnished sixty-three articles, scientific, bio-
graphical, and literary, which are designated by two consecutive letters of the sentence Fortunam ex aliis. His adoption of this motto is deemed 'to have been caused by the consideration that he had not then succeeded to his wish or expectation in his profession, and that he had reason to complain that the extent and utility of his labours in science, after having been fully appreciated by the philosophers on the Continent, had not appeared to have met with the same acceptance among his own countrymen.'

This feeling was, however, transitory, and was, indeed, hardly well founded. The fact is, that Dr. Young, by those best qualified to form a judgment, was acknowledged to rank in the highest scale, if not to stand at the head, of the men of science and literature of England, and his reputation was duly appreciated on the Continent; but the studious concealment with which his manifold contributions to the stock of human knowledge in science and philology were stolen into the world, prevented him from enjoying that wide-extented fame with his countrymen to which he was justly entitled, and which he really did enjoy in an extensive circle of truly eminent friends.

The philosophical articles of Dr. Young in the Supplement to the Encyclopædia Britannica, contain the results of his most elaborate investigations. His biographical sketches in the same work are admirably given; and the Life of Porson, in particular, has been pointed out as 'a masterly production, containing a very interesting indication of some of Dr. Young’s opinions both on the value of classical studies and on the mechanism of the human mind.' The article LANGUAGES, in the same work, contains the fruits of his investigations on the subject, into which he had been led when engaged in reviewing Adelung’s Mithridates for the Quarterly Review.

Early in 1817, Dr. Young paid a second visit to Paris, and was received with that consideration due to him in the scientific circles there. He was happy in renewing his intercourse with Humboldt, Arago, Cuvier, and Gay-Lussac; and such was the pleasure derived from his flattering reception, that, having occasion to return to London for a short period, he was induced to make a second visit of a few weeks to Paris in the summer of the same year.

In 1818, he was appointed one of the Commissioners for taking into Consideration the State of the Weights and Measures employed throughout Great Britain. To this Commission he acted as Secretary, and furnished the scientific calculations and the account of the measures customarily in use, attached to the three Reports laid by them before Parliament. It appears to have been Dr. Young’s opinion that, ‘though theoretically it might be desirable that all weights and measures should be reducible to a common standard of scientific accuracy, yet that practically the least possible disturbance of that to which people had been long habituated was the point to be looked to, and on this ground he was extremely averse to unnecessary changes.’

Towards the end of the same year, Dr. Young was appointed
Secretary to the Board of Longitude, with the charge of the super-
vision of the Nautical Almanack, having been before named one of
the Commissioners without his previous knowledge. This appoint-
ment was to him a very desirable one, though the labour in which it
involved him was great. His anxiety to increase his medical prac-
tice henceforth ceased, and it made that the business of his life which
had always been congenial to his inclination.

For the first sixteen years after his marriage, Dr. Young had been
accustomed to pass his summers at Worthing, with a view to the
practice of his profession. He now discontinued his visits, and de-
voted the summer of 1819 to a hasty tour into Italy. In about five
months he visited the most remarkable places, and examined the
Egyptian monuments preserved in the museums of that country, re-
turning to England by Switzerland and the Rhine.

From the year 1820 to the close of his life, Dr. Young continued
to furnish a variety of astronomical and nautical collections to Mr.
Brande's 'Journal of Science,' together with some philological
papers.

"In 1821 he published anonymously an "Elementary Illustration of
the Celestial Mechanics of Laplace, with some additions relating to
the motion of Waves and of Sound, and to the Cohesion of Fluids." This
volume, and the article "Tides" in the Supplement to the Encyclo-
pedia, Dr. Young considered as containing the most fortunate results
of his mathematical labours. He proceeds (says his biographer) in
his own course and manner of investigation, and uses his own processes,
and the great reach of mind displayed in these works seems universally
acknowledged; but whether he have sufficiently established all the points
which he considered himself to have proved, remains matter of dispute
amongst those most qualified to judge. They were spoken of in the highest
terms of praise by Mr. Davies Gilbert from the chair of the Royal So-
ciety; but there are some amongst the most distinguished of surviving
English philosophers, who still think that his theory of the Tides rests
too exclusively on analogies, and that many of the elements of the com-
putation are too much out of human reach to render the boldness of the
original thought susceptible of being subjected to the severity of mathe-
atical deduction."

' Dr. Young, as a mathematician, was of an elder school, and was
possibly somewhat prejudiced against the system now obtaining amongst
the continental and English philosophers; as he thought the powers of
intellect exercised by a preceding race of mathematicians were in no
small danger of being lost or weakened by the substitution of processes
in their nature mechanical.'

He again visited Paris in 1823, and in the same year published
his 'Account of some Discoveries in Hieroglyphical Literature and
Egyptian Antiquities,' in which he gave his own original alphabet,
his translations from papyri, and the extensions which his alphabet
had received from M. Champollion. This was his first acknowledged
non-professional publication since 1804,—having attained his fif-
tieth year, as he states in his preface, and determined to throw off
the shackles by which he had considered himself bound by the eti-
quette of a medical practitioner.
He made an excursion to Spa and to Holland in 1821, and in this year undertook the medical responsibility and the mathematical direction of a Society for Life Insurance, and declined all participation in the speculation, but had the disinterested satisfaction of witnessing its prosperity. This connexion led him into researches in which he took great interest, and produced his 'Formula for Expressing the Decrement of Human Life,' published in the Philosophical Transactions for 1826; and a 'Practical Application of the Doctrine of Chances,' published in the Journal of Science for October in the same year.

In the previous year he had removed from Welbeck Street to a house which he had built in Park Square, in the Regent's Park, where he led the life of a philosopher, and expressed himself as having now attained all the main objects he had looked forward to in life—of his hopes or his wishes; this end being, to use his own words, 'the pursuit of such fame as he valued, or such acquirements as he might think to deserve it.' In 1827, he was elected one of the eight foreign members of the Royal Institute of France.

With the exception of the consumptive tendency by which his youth had been visited, his health had hitherto been uninterrupted by a day's serious illness. In the summer of 1828 he went to Geneva, and appeared to suffer what was to him an unusual degree of fatigue; on great bodily exertion there was a perceptible diminution of strength, and symptoms of age appeared to come upon him, which contrasted strongly with the freedom from complaints he had hitherto enjoyed.

The Committee of Finance having recommended to the Government the abolition of the Board of Longitude, a bill was passed to that effect, permitting the Admiralty to retain the officer entrusted with the calculations of the Nautical Almanack: this occurred during the time that Dr. Young was abroad, but he continued to execute these duties. Whether the measure was well or ill founded we shall not stay to inquire, but it produced great heart-burnings and discontent amongst those scientific men who considered themselves or their friends treated unhandsomely, as well as illiberally, in the manner in which their services had been dispensed with. It appears that the occasional assistance of men of science was found to be so necessary to many departments connected with the Admiralty, that it was found expedient to form a new council of three members for the performance of duties which had before devolved on the Board of Longitude, and for this purpose Dr. Young, Captain Sabine, and Mr. Faraday, were appointed.

The consequences of this change involved Dr. Young in more labour than his declining state of health rendered him competent to perform without injury, and exacerbated a complaint which must have been long, though insensibly, in progress, but which now was bringing him rapidly to a state of extreme debility. From the month of February 1829, his illness continued with some slight
variations, but he was gradually sinking into greater and greater weakness till the morning of the 10th of May, when he expired without a struggle, having hardly completed his fifty-sixth year. He was attended throughout his illness by his friends Dr. Chambers and Dr. Nevinson. The disease proved to be an ossification of the aorta, and every appearance indicated an advance of age, not brought on probably by the natural course of time, nor even by constitutional formation, but by unwearyed and incessant labour of the mind from the earliest days of infancy. His remains were deposited in a vault in the church of Farnborough, Kent.

It has been truly said of this extraordinary man, that as a physician, a linguist, an antiquary, a mathematician, scholar, and philosopher, he has added to almost every department of human knowledge that which will be remembered to after times. In the eloquent eulogy pronounced by Mr. Davies Gilbert from the Chair of the Royal Society it is observed, that 'he came into the world with a confidence in his own talents, growing out of an expectation of excellence entertained in common by all his friends, which expectation was more than realized in the progress of his future life. The multiplied objects which he pursued were carried to such an extent, that each might have been supposed to have exclusively occupied the full powers of his mind; knowledge in the abstract, the most enlarged generalizations, and the most minute and intricate details, were equally effected by him; but he had most pleasure in that which appeared to be most difficult of investigation.' 'The example (says Mr. Gilbert) is only to be followed by those of equal perseverance,' the concentration of research within the limits of some defined portion of science, is rather to be recommended than the endeavour to embrace the whole.

Dr. Young's opinion on this subject is stated by his biographer to have been, 'that it was probably most advantageous to mankind that the researches of some inquirers should be concentrated within a given compass, but that others should pass more rapidly through a wider range—that the faculties of the mind were more exercised, and probably rendered stronger, by going beyond the rudiments and overcoming the great elementary difficulties of a variety of studies, than by employing the same number of hours in any one pursuit—that the doctrine of the division of labour, however applicable to material product, was not so to intellect, and that it went to reduce the dignity of man in the scale of rational existence. He thought it impossible to foresee the capabilities of improvement in any science, so much of accident having led to the most important discoveries, that no man could say what might be the comparative advantage of any one study rather than of another; and though he would scarcely have recommended the plan of his own as the model of those of others, he still was satisfied in the course which he had pursued.'

It has been said that the powers of the imagination were
the only qualities of which Dr. Young's mind was destitute; the
writer of this memoir thinks this want at least doubtful from the
highly poetical cast of some of his early Greek translations, and is of
opinion that it might with more justice have been said 'that he never
cultivated the talent of throwing a brilliancy on objects which he had
ascertained did not belong to them,' and that his entire devotion to
the simple truth, on all occasions, made him averse to the slightest
degree of exaggeration, or even of colouring; and that, whether
gifted or not with imagination, Dr. Young would, on principle, have
abstained from its indulgence.

In all the relations of private life, Dr. Young was as exemplary as
his talents were great, and his whole career was one unbending course
of usefulness and rectitude.

* * * To this Memoir is appended a complete catalogue of all the Works and
Essays of Dr. Young, from memoranda in his own hand-writing, for which we
hope to find space in a subsequent number of this Journal.


Agricultural and Rural Economy.

Improvement in the Breed of Cattle.—On the 12th of September,
M. Silvestre, in the name of himself and M. Husard, made a report
on a memoir of M. Girou de Busaringues, on the amelioration of
the breed of sheep, oxen, and horses. The reporter, after enlarging on
the importance of the subject, and the sagacity and experience of
M. Girou de Busaringues, said, that the memoir in question very
properly confined itself to results instead of speculations. With re-
spect to increasing the size of cattle, the author proves that the
height which it is expedient to endeavour to make them attain,
should be in proportion to the pasture peculiar to the country. He
then establishes that it is better, in almost all cases, to keep cattle in
the fields, and not nourish them in stalls; and afterwards expresses
an opinion that the fineness of the wool of sheep is in an inverse
proportion to the height of the animal, but in this opinion the re-
porters do not agree with him, as they consider the fineness of the
wool to be dependent on totally different causes. The author then
points out the different parts of the various animals to which the
peculiar attention of the cultivator should be directed. Thus in
sheep the greatest possible length should be given to the body, that
being the part from which the wool is derived. In horned cattle, the
increase of the production of milk is the most essential thing; while,
in horses, regard must be had to the purposes to which they are to
be applied. The author's observations, in the present memoir, are
confined to the race-horse, and are merely a resumé of the opinions
which he has before given to the world, in his work entitled 'Etude
de Physiologie appliquée aux Chevaux.'
In pointing out the means of producing the desired amelioration in form, the author remarks that, in horses, it is the male which usually transmits more particularly the exterior form, especially that of the lower extremities; while the internal organs, and the external form of the carcass and crupper of the female, are more generally produced in the young. Hence in endeavouring to improve a native race, it is of more importance to cross it with well-formed stallions than with mares. In copulation, the mare should be at least as high as the horse, and arrived at full development; and the author proves, from a variety of experiments made both on sheep and horses, that the antiquity of the race exercises considerable influence on the qualities of production; so that in crossing a native breed by covering indigenous mares with foreign stallions, it is better to select such mares as have no distinctive mark of race, as the author has himself experienced that the foals of mares of an old Navarroise race have invariably possessed the characteristics of that race, although the dams were covered by a fine Arabian stallion. This observation, which is peculiar to the author, accounts for the jealous care with which the Arabs preserve the pedigree of their mares, and also for the difficulty which has been experienced in deriving as much advantage as had been expected from the importation of Arabian stallions. The advice on the food and health of the animals, with which the memoir concludes, does not materially differ from that of other good writers on the subject. The reporters concluded by recommending the memoir as far as it goes (being but a slight sketch) to the warm approbation of the Academy.

Sand Manure.—On the 19th of September, M. Chaptal, in the name of himself and M. Silvestre, read a report on a memoir presented by M. Dutrochet, at the last meeting, and entitled 'Sur le Sable Silicieux comme Substance fertilisante.' The earth forms the support of plants; air, water, heat, and manure, are the nutritive principles which stimulate the action of their organs. Arable land is generally formed of a mixture of four primitive earths, the various proportions of which constitute the difference of soils, but no one of which would be alone sufficient to constitute good arable land. Chemical analysis has informed us of the proportion in which these earths ought to be mingled in order to constitute a good soil; and it is of the highest importance to agriculturists that they should avail themselves of this information, in regulating and improving their ground. In the best soils silicis is predominant; in the most fertile of the banks of the Loire, it forms forty-nine per cent. Davy found it sixty per cent. in the best soils of England; and Giobert mentions that it is as high as seventy-nine per cent. in a very fertile soil in the neighbourhood of Turin. The experiments of M. Dutrochet have confirmed the advantage of employing siliceous sand in certain earths. He covered an argillaceous field with non-effervescent pit-sand (sable de mine), and obtained from it much more
abundant crops than from similar fields which had not been prepared in the same manner. M. Dutrochet has not contented himself with the mere relation of facts, but has, with great ingenuity, accounted for them by tracing the fertilising qualities of silex to the manner in which it renders the roots of the plants accessible to the air and water, from which they derive their principal nourishment. The reporter, in conclusion, submitted that the memoir deserved the high approbation of the Academy—an opinion which was unanimously adopted.

BOTANY.

Respiration of Plants.—On the 11th July, M. Dutrochet read a memoir on this subject. He commenced by referring to the experiments of Bonnet and Adolphe Brogniart, from which it had been ascertained that the lower surface of leaves contains a number of aerial cavities, communicating with the external air of the openings of the stomata, which, it was presumed, were instrumental to the reception of the principles producing that elaboration of the sap in the leaves which has induced some physiologists to consider leaves as the lungs of plants. M. Dutrochet's object was to verify this supposition. Having observed that many leaves, particularly those of leguminous plants, lost the whitish tint of their lower surface on being plunged into water, he was induced to suppose that this phenomenon might be occasioned by the introduction of water into the cavities previously occupied by air. To ascertain this, he put a bean-leaf into a glass vessel filled with water, and, having completely submerged the leaf, he placed the vessel under the receiver of an air-pump. As the air became exhausted, bubbles of air were seen to issue from the leaf, particularly from every point of the lower surface. After the lapse of half an hour the air was re-admitted, and the lower surface of the leaf instantly lost the whitish tint, which it had preserved until that moment. On taking the leaf out of the water, he found that the lower surface had, in fact, become precisely of the same colour as the upper surface; thus proving that the white tint of the lower surface of the leaf is entirely produced by the presence of air. Sometimes leaves appear to have white spots on the surface: these are proved also to result from the same cause, and disappear as soon as the air is removed by the action of an air-pump. The introduction of the air into the parenchyma takes place through the openings of the stomata, which does not prevent those openings serving at the same time for the transpiration of the leaves and the absorption of atmospheric air. M. Dutrochet then ascertained, not only that there is an immediate and easy communication between these aerial cavities by means of all those parts of the leaf which are not separated by thick nerves, but that the aerial cavities of leaves form part of a pneumatic system, extending throughout the whole plant. To prove the direct correspondence between the aerial cavi-
ties of the leaves and the aërial canals of the petiole, M. Dutrochet plunged a leaf of the *Nymphaea lutea* into a glass vessel filled with water, leaving the severed extremity of the petiole out of the water. On placing the vessel under the receiver of an air-pump, and exhausting the air, he did not see any air issue from the submerged parts of the leaf; and on re-admitting the air into the vessel, a quarter of an hour afterwards, the lower part of the leaf retained its whitish tint, which proved that it had not lost the air which in its natural state filled its aërial cavities. M. Dutrochet then recommenced the experiment with the same leaf, taking care to submerge it entirely; and as soon as he began to exhaust the air, numerous air-bubbles escaped from the severed extremity of the petiole, but none from the edge or border of the leaf. When, however, the air was re-admitted, a quarter of an hour afterwards, the lower surface of the leaf instantly lost its whitish tint and became as green as the upper surface; thus proving that the air had entirely escaped from the aërial cavities through the petiole, and that these cavities had become filled with water.

The hair or nap which is frequently found on leaves, particularly the lower surface, is considered by M. Dutrochet as being filled with air, and as forming the respiratory conduits of the nerves over which they are placed, while the stomata are seen in the intervals left between those nerves. In the rose-laurel the lower surface has no stomata, but is covered entirely with nap or hair.

The aërial tubes of the *Nymphaea* are slightly hexagonal, and from the angles spring hairs, which, being of the same height in the different united tubes, form for each ternary system of tubes the starry figure noticed by M. Amici. M. Dutrochet considers these hairs as conduits which, by their capillarity, absorb the air contained in the canals and carry it into the vegetable tissue, in the same manner as the ramifications of the trachea in insects do into every part of the living animal. Other plants offer different dispositions, but all are calculated to absorb the air from the aërial cavities and carry it into the most remote parts of the plant. It results, therefore, from the experiments of M. Dutrochet, that in every part of plants there are aërial organs, filled with a gas compounded of oxygen and azote in variable proportions, but in which the oxygen is always in a smaller proportion than in the atmospheric air, which proves that it has been absorbed by the interior organs of the plant. The same circumstance is observed in analysing the air contained in the trachea of insects, which proves that their mode of respiration is the same; that is to say, the transport of respirable elastic air into all their parts. But the origin of this air is not quite the same, since insects derive it wholly from the atmosphere, while the plants generate a considerable part of it in their tissue by the influence of light. The azote, which is not absorbed in the internal respiration of the plants, is necessarily expelled; and, in fact, we perceive that flowers exhale a great deal of azote while they absorb oxygen, particularly
under the influence of light. Leaves, on the contrary, exhale oxygen when exposed to the solar light, and only respire in the shade or during the night. The oxygen which issues from the stomata when the leaf is subjected to the influence of light is only a part of what is produced; the rest passes from the aerial cavities into the conduits of the petiole, and thence into the whole vegetable tissue. As there is a continual production in the green matter exposed to the light, the oxygen as it is formed impels forward that which has been previously formed; and this mode of circulation supplies in vegetables the place of that which is produced in animals by muscular contraction.

M. Dutrochet concludes by proving, from a variety of experiments, that as the respiration of plants is supported both by the oxygen which is contained in the air penetrating from the exterior, and by that which is formed internally by a chemical action of the light, (which latter is indispensably necessary to their existence,) the absence of light, by diminishing the irritability of the members of the vegetable kingdom, becomes for them a direct cause of asphyxia, and that plants may, therefore, be hindered of respiration, and consequently killed either by the action of the air-pump or by total obscurity.

Irregularity of Flowers.—On the same day, M. Cassini, in the name of himself and M. Mirbel, made a report on a memoir by M. Adolphe Brogniart, on the relative insertion of the different parts of each floral verticillus, and on its influence on the regularity or irregularity of flowers. According to a modern theory, almost universally adopted, a flower is composed of several verticilli of foliaceous organs superposed and immediately approaching each other, so that the outward or lower verticillus forms the calyx, the next one the corolla, and so on for the stamina and pistils. M. Brogniart, in examining this theory, was led to inquire whether each of these successive rings is in reality a perfect verticillus, that is to say, whether all the pieces which compose it are exactly of the same height round their axis. He remarked, that the calyx of the helianthemis, of several caryophylli, &c., have evidently some folioli of the calyx situated more externally, and consequently lower than the others. The manner in which the petals cover each other in many flowers during preflorescence, that is to say, before they are fully opened, also appears to him a proof that these pieces are inserted at different heights. Thus, according to M. Brogniart’s opinion, the similar organs which form each of the rings in most flowers do not form exact verticilli, but are disposed at different heights round the axis, or short branch, which bears them; and as the lower pieces are necessarily the outermost, it follows, that their mode of envelopment must indicate their primitive insertion. Thus the imbricated preflorescence, which presents the pieces of the corolla lapped over each other, indicates their disposition in alternate order, while the
valvary preflorescence, in which no piece laps over another legiti-
mately, indicates the disposition in form of verticillus, as perhaps does
also the returned preflorescence, in which each piece laps over its
neighbour on one side, and is lapped over by that on the other side.
Although uniformity is necessary in the arrangement in form of verti-
cilli, it is by no means so in the alternate disposition of the floral pieces
any more than in that of leaves; and M. Brogniart points out several
modes of alternation, the most frequent of which is, where the five
pieces forming the calyx, or corolla, are disposed on a spiral of rather
more than one turn and a half; this is the quincuncial preflorescence,
corresponding to the disposition of real leaves in quincuncx. M.
Brogniart admits that the different modes of preflorescence of the
corolla are not always the same in the same plants, and that, there-
fore, in order to trace the theory of original insertion from the pre-
florescence, it is necessary to regard only the most usual dispositions,
and take no notice of the exceptions, a rule generally necessary in
natural history, and which proves the importance of being always on
our guard against that spirit of system which will only admit abso-
lute theories.

M. Brogniart, however, endeavours to account for the anomalies
of preflorescence by conjectures, more or less plausible, founded
principally on the inequality of growth of the different pieces of the
corolla. It is this irregularity which generally constitutes the irre-
gularity of the flower, and on this M. Brogniart founds a general
law, which is important and worthy of attention, as establishing an
interesting relation between the mode of preflorescence and the regu-
larly or irregularity of the flower.

If a family of plants presents a valvary or returned preflorescence,
both of which indicate a disposition exactly verticillated, that family
will scarcely ever present any irregular flowers, nor will there be
found near it any other family derived from the same prototype, and dis-
tinguished only by the irregularity of the flowers. If, on the contrary,
the preflorescence be imbricated, which indicates an alternate dispo-
sition, the same family will often contain regular and irregular
flowers; as, for example, in the Ranunculi, or else close to the
family of regular flowers will be found another family having no
essential difference, except the irregularity of its flowers. Thus the
Leguminosi may be considered as Rosaceæ, with irregular flowers, the
Fumariae as Papavera with irregular flowers, &c.

This law, however, is not without exceptions, as the Lobelïæ, the
Synantherus, the Aristolochiæ, have irregular flowers, although
their preflorescence is not imbricated, and the floral pieces are conse-
quently inserted at the same height.

M. Brogniart next attributes the irregularity of flowers to the dis-
position of the organs of plants to assume a different growth when
placed at different heights on their axis, a disposition which is also
manifested by leaves, and the existence of which the reporters consider
by no means improbable. The author concludes his memoir by
applying his theory to the abortion of the carpillis, or parts of the pistil, which he also imagines to be disposed spirally, and which would be reduced to three or two, according as the outward or inward part of the spiral is most disposed to increase in growth. The reporter remarks, that these last considerations are too conjectural, and must be regarded as more ingenious than solid; and adds, that the memoir has, in general, too great a tendency to hypothetical views, although it is but justice to acknowledge that the conjectures of M. Brogniart, which are, as usual, ingenious and interesting, are not allowed to usurp the place of positive facts, an abundance of which will be found to constitute the most solid part of the memoir. In conclusion, the reporters recommended the memoir to the warm approbation of the Academy.

Development of the parts of Plants.—On the 18th of July, M. Cassini read a report on a memoir by M. Barbe, entitled 'Observations sur l'impulsion qui provoque la saillie des germes radicaux adventifs, et sur quelques autres points de Physique Végétale.' The object of the memoir, which will form part of a large work hereafter, is to account for the fact previously observed by M. de Mirbel, that the adventitious radical germ always develope themselves in a horizontal direction; or, to speak more correctly, in a perpendicular or right-angular direction, with reference to the root from which they spring. M. Barbe has given them the new name of éradians, because they are under the influence of a powerful radiating force; and concludes that the sprouting of the adventitious radical germ cannot be produced by the ligneous fibre, because that could only act in its own direction, which is always parallel to the axis, but that it is operated on the points of the medullary radii, nearest to the liber or inner bark; and he is of opinion that the young roots are only expansions of these radii. In reply to the objection, that the roots are produced on the substance of some leaves, (as for instance, the cardamine pratensis.) M. Barbe states, that there are also medullary radii in the leaves; in proof of which, he cites the horizontal section of the petiole of the leaf of the orange-tree, as a proof of there being no connexion between the production of the adventitious roots and the development of the foliaceous buds. M. Barbe mentions the singular fact of a slip of willow having produced several roots, already arrived at a ligneous state, and even a new bed of wood and bark interposed between those which existed at the time of plantation, although the foliaceous buds were not at all developed. M. Barbe has also particularly examined the functions of the small spots or rents in the epidermis, which M. de Candolle calls lenticelles, and supposes to be buds of the roots; whereas M. Barbe maintains that, if they be not vegetable parasites of the class cryptogamia, (which, however, he is disposed to think they are,) they are merely eruptions of the feculent cellular tissue accumulated under the cuticle, and have no relation whatever with the production of the radical germs. The
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memoir also contains allusions to various other points, which are to be treated in M. Barbe's forthcoming work; but they are not developed with sufficient minuteness or clearness to admit of their being examined. The reporter, however, considered that M. Barbe deserved the approbation of the Academy, as an encouragement to continue his researches on a subject highly important to science.

Fossil Plants.—On the 18th of July, M. Beudant made a report to the Academy, on the publications and manuscript memoirs of M. Adolphe Brogniart, on this subject. M. Brogniart has already published five numbers of his work, containing thirty-one sheets of letter-press and seventy lithographic plates, representing a great number of fossil plants, compared with the analogous living plants, placed in juxtaposition with them. In studying fossil plants, a very different system from that in which a knowledge of living plants is obtained, must necessarily be pursued. The organs of fructification, which, in botany, supply the characteristics of the primary class, are almost invariably deficient in fossil plants; and the leaves and stems, which in living plants are usually the object of a very superficial study, are all that remain. M. Brogniart has, therefore, especially applied himself to examine minutely the organization of these parts, with a view of appreciating with exactness the relation of the characteristics to be drawn from them, with those derived from the organs which usually serve as grounds for classification. He has carefully examined the nerves of the leaves, and the different mode of insertion of these organs in each family; he has investigated the modifications which the different parts of the internal structure of plants can produce on their exterior, and has, in a word, applied himself to the most minute researches on every characteristic of living plants, which could by any possibility be preserved in those buried in the bosom of the earth. He has also endeavoured to ascertain the different modifications, which compression and the various modes and degrees of decomposition might produce in fossil plants; and has distinguished with the greatest precision the cases in which the family, the genus, and even the species of a plant, may be pronounced on with certainty, from those in which doubts may be entertained on some of these points, and it may, therefore, be only safe to decide on the genus, the family, or the class of a plant. By these means M. Brogniart has succeeded in distinguishing among fossil plants, species, genera and families, perfectly analogous to those of living plants; and has already disposed more than five hundred species of these plants in their natural order, and, by comparing them with living plants, enabled us to appreciate the immense distinctions existing between the ancient and modern botanical world. M. Brogniart has not stopped here, but has proceeded to examine the laws regulating the distribution of fossil plants in the different beds of the earth, and compared these with the laws of the geographical distribution of plants on the surface. He has ascertained with certainty,
that the vegetable remains found in the most ancient deposits of the globe are plants belonging specially to the families of the equisita, ferns, and lycopodia; that it is not until a higher series of formations about the mottled freestone (grès bigarré) that some coniferæ are found; the cycades are only found still higher, and the dicotyledonous plants do not appear until immediately after the chalk. The primary vegetation of the globe, therefore, consisted of vascular cryptogamiae, with about one fifteenth part of their number of monocotyledonous phanerogamiae, and these plants were of immense size; the horse-tails or equisita grew to ten feet in height and from five to six inches in diameter; the ferns from 40 to 50 feet in height, and the lycopodia from 60 to 70 feet. When the coniferæ begin to appear, the cryptogamiae become less numerous; the species are no longer the same, nor have the same magnitude. When the remains of cycades commence, the species of the cryptogamiae are again different, several genera have entirely disappeared, and the number of these plants, which in the first epoch bore a proportion to that of the monocotyledonous phanerogamiae of 14 to 1, is now only about equal to it: so that in the primitive floral world the cryptogamiae formed \( \frac{13}{15} \) of the whole number of plants; in the middle epoch they constituted only half, and the other half was composed of coniferæ and cycades; while in the present distribution of plants on the surface of the earth these families scarcely form one three-hundredth part of the whole number. When the dicotyledonous plants appear in the beds of the earth, their number becomes suddenly immense, and the cryptogamiae, which then belong to genera different from those found in the former beds, disappear almost entirely. The numerical relation of the different families with each other then becomes nearly similar to that of the plants now existing on the surface, and the most numerous species are those which have the strongest analogies with living plants. Hence it appears that the vegetation of the earth has greatly changed at different epochs, and has become more and more complex, so that the long lapse of time, during which all the deposits have been formed, is divided into various periods of unequal length, during each of which vegetation had peculiar characteristics uniform throughout the earth, and after each of which vegetation completely changed in its genera, its families, and even its classes, as well as in the numerical relation between the different species, until it ultimately arrived at a point nearly resembling that which now exists. It is also remarkable that the beds in which are found the remains of plants of any given period are separated from those containing the remains of plants of a different period by deposits entirely destitute of terrestrial plants; whence we may conclude that there were periods of repose, during which the surface of the earth was wholly unproductive, a circumstance which has an immediate relation with the differences observed between the various periods of vegetation, which indicates the sudden convulsions by which every thing then existing was destroyed, and

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which shows us at each change a new order of things doubtless in
harmony with the subsequent atmospheric circumstances. The im-
portance of these considerations, when united with the observations
recently made on the fossil remains of animals, and the researches
of M. Élie de Beaumont on the relative ages of the mountains of
crystallization, will be obvious to every geologist. M. Brogniart has
also gone into some curious researches on the comparison of the
laws regulating the different epochs of vegetation in the ancient
world with those now existing. The vegetable remains of the first
period have much greater resemblance to those now found near the
equator than to those of the temperate zone. In the torrid zone
we find much higher species of horse-tails, ferns, and lycopodia,
than are observed near the poles: many of them are even arbo-
resent there. But the size of the fossil plants of those families in
the first period of vegetation is much greater than even that of
those now existing at the equator, consequently the causes of de-
velopment were then much more intense than at present. On the
other hand, M. Brogniart remarks that these vascular cryptogamiae
formed \( \frac{2}{10} \)ths of the primordial vegetation, whereas they scarcely
constitute \( \frac{1}{10} \)th of the actual vegetation; there must, therefore, have
been some cause producing this preponderance. It had been before
remarked that these cryptogamiae attain the greatest size between the
tropics; but M. Brogniart is the first to call attention to the fact,
that their relative quantity is greatest in islands, and becomes more
considerable in proportion as the islands are smaller and at a dis-
tance from the continents, so much so that in some small islands
they are even now almost equal in number to the phanerogamiae:
whence we may conclude, that if the world were composed, not of
continents, but of small islands only, the cryptogamiae would be
equal or superior in number to the phanerogamiae. This observa-
tion is of great importance when considered in conjunction with the
geological facts which induce us to conclude that the existing contin-
ents were not always in their present state, but have been formed
piecemeal, and that probably, in the most remote ages, the earth con-
sisted only of islands and archipelagos in the midst of a vast sea.
M. Brogniart then shows us, that, in the subsequent periods, vegeta-
tion began to resemble that which we now see on the coasts and in
the large equatorial islands, both in the size of the plants, the families
to which they belong; and the numerical relation between these fami-
lies. Finally, he shows that, in the most recent period after the ap-
pearance of the dicotyledonous plants, the vegetation became analo-
gous to that now existing in large continents. These observations are
derived from the general examination of all M. Brogniart's printed
and manuscript labours. The work in course of publication, entitled
"L'Histoire des Végétaux Fossiles," will form a complete table of all
the fossil plants considered with a view to their classification, and their
analogy with the plants now existing. The five numbers which have
appeared, treat of the confervæ, the fuci, the equisetæ, the musci or
mosses, and some genera of the family of ferns. Each family is preceded by general considerations on the characteristics of the existing plants which it contains, on their anatomical organization, their generic divisions, and their distribution on the surface of the earth. The fossil plants of the same order are then compared with these, and the geological circumstances under which they are found minutely and accurately described. The anatomical structures of the families of the equiseta and ferns, particularly the organization of the petioli, and the characteristics of the stems of the latter, are, for the first time, fully and accurately described, and all the plates and tables of the localities in which the different fossils are found are remarkable for their scrupulous fidelity. In conclusion, the reporter observed that the philosophical spirit in which the researches of M. Brogniart had been carried on, and the importance of the facts which he had collected, as well in a geological as in a botanical point of view, specially merited the highest and most signal approbation of the Academy.

Structure of the Stems of Plants.—At the meeting of the 25th of July, MM. Desfontaines and Cassini made a report to the Academy on a memoir by M. Adolphe Brogniart, entitled 'Observations sur la structure et le mode d'accroissement des tiges dans quelques familles de plantes dicotyledones.' The object of this memoir is to supply the deficiency in science observed by M. de Candolle, who has remarked that the classification of plants can never be considered as truly according to nature, until the characteristics derived from the organs of vegetables can be made constantly concurrent with those derived from the organs of reproduction. M. Brogniart endeavours to attain this end by proving that several families of plants have distinctive and peculiar characteristics, observable in the structure of their stems. After alluding to the general difference arising from the fact of the plants being monocotyledons, or dicotyledons, the author remarks that, in the greater part of the ligneous dicotyledons, the fibrous fascicules, disposed in concentric circles, which form the successive beds of wood and liser, approach each other laterally, and are united at various distances; so that the radii, or rather the medullary plates which separate them, have their length frequently interrupted by reticulations. This disposition, although general, is not universal: thus, in the vine and the cimis, the medullary radii form very long continuous plates without interruption. M. Brogniart has made the same observation in all the menispermæ which he has been able to notice, as well as in the ligneous ranunculi, such as the elematis, in the aristolochia, and in the pipera. It might be hence concluded, that the structure belonged generally to creeping or climbing plants; but the author, not having found it in the Bignonia, periploia, honeysuckle, ivy, &c.; and having observed it in the berberæs, is induced to suppose it a characteristic, independent of the manner of growing, and having relation

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to the organization peculiar to certain families. Thus, therefore, the continuity of the medullary radii, from one articulation to another, may be considered one of the distinguishing characteristics of the plants above mentioned. With the exception of the *clematis* and *aristolochia*, which show it clearly at every age, this characteristic can only be observed in the young stems. M. Brogniart then remarks, that the greater part of climbing plants have the stem almost entirely composed of vessels of so large a bulk as to be visible to the naked eye on the transversal section, which appears full of holes, like a sieve. This characteristic, which is peculiar, not to the family, but to the mode of growth of the plant, is considered by the author as a necessary consequence of the small volume and great length of the stem, which bears numerous leaves of a large size, because these leaves occasion a profuse transpiration, and therefore require a structure of stem which will admit of a rapid passage of the sap, to supply the loss occasioned by the transpiration. M. Brogniart then details the remark which he has made on the internal organization of the vine, the *clematis*, and the *aristolochia*, the most important of which is the elliptical or oblong form of the section of the pith in the last named family. The author also establishes that, in the menisperme, the *liber*, or fibrous part of the bark, always remains in the same state that it was in during the first year, not increasing in any manner. Thus the *cambium*, which is interposed between the wood and the bark, instead of being divided as usual into two beds, the one of wood and the other of *liber*, is here entirely united to the ligneous body. He has also remarked that, in the internal structure of the stem of the menisperme, the ligneous body is divided into several concentric layers, perfectly distinct from, and independent of each other, each of which is the produce of several years, and that the new beds are frequently entirely wanting on one side of the stem, which is thus left naked. The *pipera* also offers some remarkable characteristics, particularly, 1st, the formation of new ligneous fascicules between those which primitively composed the ligneous ring surrounding the pith, by which the diameter of the medullary canal is increased, and 2ndly, the existence in the interior of the pith of fibrous fascicules analogous to those of the ligneous body, the number of which increases every year. M. Brogniart remarks, in conclusion, that comparative anatomy ought to be the basis of the classification of vegetables, as well as of that of animals; and that the true characteristics of the organization of a plant are rather to be found in the body of the stem which produces the leaves and flowers, than in those leaves and flowers which are but the offspring of, and dependent on the stem. The reporters, although not disposed to believe that the internal analysis of the organs can ever be of the same importance in botany as it is in zoology, agree with the author that it must always be a source of interesting and important investigation. The report concluded with the highest eulogium on the talents and industry of M. Brogniart, and the recommendation of the insertion of the essay
in the 'Recueil des Mémoires des Savans Etrangers,' which was ordered by the Academy accordingly.

**Fecundation of the Orchideæ and Cisti (Cistinus).**—On the 1st of August, Messrs. Cassini and Auguste de St. Hilaire made a report on a memoir by M. Adolphe Brogniart on the above subject, of which the following is an abstract. The object of the memoir is to explain the mode in which the pollen acts on the stigmata, and the manner in which the fecundating fluid passes thence to the ovary in these two families of plants, the reproductive organs of which are not formed in the usual manner, with a view to strengthening the theory formerly promulgated by him as to the general mode of fecundation of plants. The pollen of the orchis is agglomerated in divided and subdivided masses in such manner that the last groups are composed of three, four, or five spherical grains. When these masses fall on the stigma, some of the grains are separated from the rest, and fix themselves on that organ. Each of these grains soon produces a membranous tube, which penetrates into the stigmatic tissue, formed of utriculi, elongated, free, and only united by a viscous liquid.

In the epipactes, the pollen is pulverulent, formed of small aggregations of four spherical grains, which remain always united, and which, when they fall upon the stigma, give birth to very long tubular appendages which penetrate deeply into the stigmatic tissue.

The ovary of the Orchideæ offers internally a simple cavity, having three longitudinal projections, each divided into two laminae; these are the placentæ, which have on their edge an infinity of ovula, so disposed, that the opening through which the fecundating fluid ought, according to M. Brogniart's theory, to reach them, is diametrically opposite to the point by which they are attached to the plant. This appeared an astounding objection to the theory, but M. Brogniart explains it thus. The stigmatic tissue is continued in the axis of the column which constitutes the style; it is there, at the summit of the cavity of the ovary, divided into three faisceaux, each of which is subdivided into two filamentary bands, which are applied to the two laminae of each placenta, and as the separate filaments which form these bands are twisted or folded back in festoons, which penetrate between the ovula, and often appear to extend quite to the orifice at their extremity, the stigmatic tissue serves as a conductor to the fecundating fluid, which is thus enabled to attain the orifice at the extremity. The family of the cisti (cistinus) offers, from the ordinary position of the orifice of the ovula, the same objection to the theory of the fecundating fluid proceeding from the stigma to the interior of the ovary by means of that orifice; but M. Brogniart remarks, that when the orifice of the ovula is opposite to the point of junction with the plant, these ovula are placed on a very long umbilical cord, which is tortuous or bent back, so that the disengaged and open extremity of the ovulum is in contact with the sides of the ovary, of the partitions, or of
the placentæ, a fact which is quite sufficient to establish the possibility of the communication, as the stigmatic tissue, the conductor of the fecundating fluid, could very easily creep along the sides of the ovary until it came in contact with the orifice. And, in fact, it frequently happens, that in some Helianthema, the umbilical cords of which are not so regularly bent back as to bring all the ovula in contact with the placenta, a number of ovula, which have become abortions, are found in the fruit when ripe, their vestiges remaining dispersed among the perfect grains. The following facts, pointed out by M. Brogniart, are still more remarkable.

In the *Helianthemum leavipes et thymifolium*, the ovary contains six ovula, attached near the summit of its cavity to the three placentæ by cords so short as to be unable to bend back. But in this case, the orifice of the ovulum, instead of being, as in the other species, opposite to the point of junction, is almost close to it, and is prolonged in a small tube which is applied exactly to the base of the style at the point of termination of the extremity of the conducting tissue. In other species, such as the *Helianthemum Ægyptiacum et Niloticum*, where the numerous ovula are inserted on placentæ, which are parietal, not projecting, and supported by cords nearly straight, so that their open extremity cannot be brought into contact with any point of the internal sides of the ovary, M. Brogniart has discovered that the conducting tissue, which occupies the axis of the style, is prolonged downwards to the middle of the cavity of the ovary in a bundle, divided and subdivided into a number of fine and floating filaments, which can easily carry the fecundating fluid to the orifice of the ovula. From these facts, M. Brogniart concludes, that whatever may be the structure of the pollen, its seeds always produce a long membranous tube, which penetrates between the utriculi of the stigma to deposit the fecundating matter in its tissue, and that, notwithstanding any modifications in the structure of the ovary, the stigmatic tissue or conductor always penetrates to it, and is there disposed so as to be placed, by some means or other, in contact with that part of the ovulum which, being open, allows of the introduction of the fecundating matter.

On the truth of this theory, however perfectly it may appear to be supported, it is not the province of the reporters to pronounce; but the facts adduced have been verified by them, and found to be equally new, interesting, and exact; and the importance of these facts is sufficient, independent of any hypothesis, to entitle the memoir to be inserted in the ‘Recueil des Mémoires des Savans Etrangers.’

Abortions and Irregularities of Flowers.—On the 1st of August Messrs. Cassini and Mirbel made a report on the Memoir of M. de Jussieu on this subject, of which we gave an account in our last Number (page 133). After having analysed the memoir, the reporters remark, that by a singular chance M. Adolphe Brogniart had examined nearly the same question as M. de Jussieu at the same
time, and arrived at a similar result. (Vide Report, July 11th, on his Memoir, page 338.) While the memoir of M. de Jussieu was under the consideration of the reporters, he forwarded to them another and more detailed memoir, containing a great variety of interesting observations and illustrations of the family Malpighia. The ovula of this family are different in their mode of development from all those with which we are acquainted; they cannot, therefore, be referred to either of the three classes, Orthotropes, Anatropes, and Campulitropes, as they appear to participate of all these forms. The ovula of the Hippocastana are entirely anatrope. Two in each ovary, they present the remarkable characteristic, that in developing, one breaks out (se reenverse) from top to bottom, and the other from bottom to top. The ovula of the Acerinus are also anatrope, but not so decidedly so as those of the Hippocastana. The Camarea hir-sula, and the Camaria affinis, produce two kinds of flowers, the one conspicuously situated at the upper part of the stem, having four large petals, six well-conditioned stamens, and three fecond pistils; the other very small, hidden in the angle of the lower leaves, having no corolla, but one indehiscent stamen, no pollen, and two ovaries, generally without style or stigmata, yet producing good seeds like the ovaries of perfect flowers. The father of M. Adrien de Jussieu, in his Genera Plantarum, had divided the family Malpighia into two secondary groups, characterised the one by pulpy, the other by winged fruits; but M. Adrien de Jussieu remarks, that there are genera with capsular fruits, which belong to neither of these sections, and form a transition from one to the other. M. de Candolle had added a third group, under the name of Malpighia Monostyla; but the supposed single style is, according to M. Adrien de Jussieu, nothing but a bundle composed of several styles fastened together, and proceeding from an equal number of united ovaries; and the genus in question contains species, in some of which the styles are united quite to the summit, while in others, they are separated almost down to the base. The author, therefore, rejects both these divisions, and considering that the genera are only distinguished from each other by small and graduated differences in the degree of abortion, he prefers considering the family as a whole, without establishing any artificial division into tubes or groups. The memoir details the particulars of sixteen known and eight new genera, in which he has added to the 195 species before described, 90 hitherto unknown, or not supposed to belong to the family. The author avows his intention of re-arranging all the botanical families; and if immense materials, indefatigable research, and acute observation be sufficient to enable him to do so successfully, he is sure to triumph. If M. de Jussieu's theory of organization be open to some doubts, his efforts to dispel those doubts have been highly advantageous to science, as they have elicited a number of curious and authentic observations on the floral organs of the Malpighia and kindred families.
Family of the Chenopodia (Chenopodées).—At the same meeting, M. Auguste de St. Hilaire read a report on a memoir by M. Alfred Moquin on this family,—one of the least known in the vegetable world. This memoir, which is the first of a series, is devoted to the examination of the genus Sueda and the other Chenopodia most allied to that genus. The genus Sueda had been confounded with the Chiropodium and the Salsola, until Forskal proposed to class them in a separate group under the above name; but it has never, until now, been accurately described by any naturalist. The Sueda with ligneous or herbaceous stems, and fat and succulent leaves almost vermicular or cylindrical, grow on the borders of the sea or of lakes; they will always afford soda by incineration; but as the presence of this substance in the tissue is accidental, it will disappear when the plant is cultivated at a distance from salt water or marshes. M. Moquin enters into very long and minute descriptions of the various organs of the plant, and affords some remarkable explanations of anomalies in different species, particularly the existence of the perisperma in the Atriplex, the Beta, and the Chenopodium, and its non-existence in the Salsola, the Camphorosma, the Ainanbas, &c. ‘The species of liquor,’ says M. Moquin, ‘in the midst of which the embryo of the Salsola at first floated, becomes entirely absorbed by it. When this embryo has attained its full growth, it is larger or longer than that of the Chenopodia, which have seeds abundantly albuminous; it is more advanced, and has the tissue and colour of a little plant. Consequently a seed of Chenopodia, which has no perisperma, only differs from an albuminous seed of the same family, inasmuch as it has already absorbed its perispermic nourishment, and its embryo is rather more advanced in its growth. It results also from this observation, that the moment of maturity of seeds is not in all plants precisely that at which the embryos have attained the same degree of development. Thus a grain or seed of Sueda, having a spiral embryo, but without perisperma, is not analogous, as to its growth, to a grain of Anserina which has just left the parent stock; the latter at its maturity resembles a seed of Sueda, which is still at a certain distance from maturity.’ In the Chenopodées which have a perisperma the embryo is white; in those which have none it is greenish. The Sueda, however, are exceptions to this general rule, as their embryo is white, and they have no trace of perisperma. The reason of this, according to M. Moquin, is, that the Chenopodia which have a perisperma, generally have a double integument, the thick and crusty exterior of which allows no passage to the light, and the embryo therefore remains white; whereas the others have only a simple, membranous, thin tunic, the tissue of which allows the passage of the rays of light, which produce the green colour. The Sueda, although without perisperma, have an exterior crusty integument like the Anserina, and the embryo therefore does not become coloured.

The genus Shanginia (which, as well as the genus Schoberia, was
first distinguished as a separate genus by M. C. A. Meyer) forms a singular exception to the rest of the family of the Chenopodia in having a semi-inferre fruit. M. Moquin supposes the adherence of the pericarpium to be owing to an intermediate discus between the ovary and the calyx. The reporter, without rejecting this explanation, remarks that the consequence deduced is not a necessary one, as many plants present a large discus joined to the calyx, without having the ovary otherwise than free and disengaged.

The Sueda, the Schoberia, and the Shanginia form a little tribe in the family of the Chenopodia, the distinguishing characteristics of which are a white or whitish embryo, spirally turned, usually without perisperma, and always surrounded by a double integument, the exterior of which is crustaceous. The various plants comprising this tribe are described by M. Moquin with great accuracy and minuteness; but it is obviously impossible for us to follow him into this detail. The reporter observed that the work merited the full approbation of the Academy, and urged the author to continue his researches on the other tribes of the same family*.

Decoloration of Leaves; Vegetable Nutrition.—On the 8th of August, M. Dutrochet read an elaborate memoir on the above subjects, of which the following is an analysis. It is well known that when light is excluded from any of the vegetable kingdom the leaves lose their green colour, and become of a yellowish-white. The physical cause of this is the loss of carbon, which, when the action of the light no longer fixes it in the tissue of the plant, is poured out into the atmosphere in the shape of carbonic acid, and the plant, deprived of the substance to which it owes its green hue—the sign of life and health—languishes into a morbid paleness. But the loss of carbon is not the only cause of the change of colour; it is also produced by the exhaustion of the soil in which the plants are growing. Thus the leaves of a plant in a garden-pot will, if not watered with manured water, lose their green tint, and become white about three years after they have been allowed to remain in the same mould. Three years is mentioned as an average period, but the time will be greater or less according to the degree of nutritive principle and to the volume of the mould in the pot. An example illustrating this fact occurred a short time since. A gentleman, whose house was situated on a calcareous rock, dug large holes in the rock, filled them with earth, and planted peach trees. These trees had every atmospheric advantage; the fissures of the rock allowed free passage to the rain-water; and from their full exposure to the sun and light there could be no deficiency of carbonic acid; yet, after flourishing a few years, the leaves began gradually to change colour, and ultimately, when, from

* M. Moquin appears to adopt the term Chenopodia as indicating the whole family, and in opposition to Chenopodium, one of the genera of that family. The same remark applies to the Cistus, as opposed to the Cistus, and the Euphorbia, as opposed to the Euphorbias, in the preceding report.
the exhaustion of the nutritive principle of the earth in which they were planted, the suction of the roots no longer supplied any alimentary carbon, the trees produced only white or pale-yellow leaves.

The same phenomenon may be established by remarking in spring the difference of colour between grain growing in a rich soil, and that growing in a poor soil. The moment the sap which nourishes the plants cannot derive a sufficient quantity of carbon from the earth, the leaves become paler and paler, until the carbon is quite exhausted, and they are then produced quite white. In the decoloration of leaves from want of light, there may be plenty of carbon in the plant, but, instead of being fixed in the tissue, it is dispersed under the form of carbonic acid; while in that arising from the exhaustion of the soil, the carbon, which is the essential colouring principle, is wanting, and, therefore, the brightest rays of the sun produce no effect. Cold is a third cause of the change of colour in the leaves of plants; this results both from the obstacle opposed by the lowness of the temperature to the nutrition of the leaves, and also from the age of those organs. Those plants which have the greatest vigour of vegetation will always resist the longest the influence of the cold, which tends to suspend their nutrition, and, therefore, to change the colour of their leaves. This is remarkable in wheat and other grains; the cold of winter does not affect them, they grow up in the spring with green leaves; but if, afterwards, cold weather comes, the nutrition is suspended, and the leaves become yellow. If, however, in the same field, some parts have been better manured than others, and, consequently, have stronger nutritive principle, the grain growing on those parts will preserve the colour of the leaves long after that of the other parts has given way to the influence of the cold. Hence we find that a certain depression of the temperature occasions in plants, during their development, a suspension of the fixation of the alimentary carbon; and, consequently, (as they are constantly losing some of their carbon in the form of carbonic acid,) a change in the colour of the leaves, but that the effect of this lowness of temperature will be in a great measure resisted by plants which possess in a considerable degree strong principles of nutrition.

All these observations tend to prove that it is from the soil that plants principally derive the alimentary matter by which they exist; this alimentary matter is an extractive, soluble in water, existing in various proportions in the different vegetable earths. It is very abundant in the offal of animal and vegetable matter, whence dung and manure are formed. It appears from the experiments of M. de Saussure, that the oxygen of the atmosphere combines with the carbon of this extractive, and changes it into carbonic acid, which, being dissolved by the sap, is transported into the leaves, where it is decomposed by the action of the light; the carbon is fixed in the vegetable tissue, and contributes to form the green colouring matter, and the oxygen is discharged.

All carbon which is susceptible of being converted into carbonic
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acid at the ordinary temperature of the atmosphere, is adapted for the
nutrition of plants. This carbon is to be found in the extractive
matter which abounds in vegetable earth, and which is also found in
solution in all waters, even in the most apparently pure springs.
This carbon exists also in an organic matter which is as volatile as
the water in which it is dissolved. When water, charged with organic
substances, is distilled, water is obtained which is not pure,—it
contains a matter which, if not organic, is at least organizable.
This water, when exposed to the light, develops and produces the
green matter of Priestley, and the Vaucheria infusoria, which proves
that it contains alimentary matter. When the water contains a con-
siderable quantity of this matter it is sensible to the taste; but otherwise
its presence cannot be detected, as it is not affected by any chemical
re-agent. The volatility of this organizable matter proves that it must
exist in the water which is evaporated from the surface of the globe.
When this water falls again in the shape of rain, it meets, in its
course through the atmosphere, volatile emanations both animal and
vegetable, with which it becomes charged, and thus rain-water, dis-
tilled by Nature herself, falls charged with matters adapted for the
nutrition of plants. Thus we perceive that, although part of the
nourishment of plants is obtained from the atmosphere, it is as much
derived from the carbon contained in the rain and dews, as from the
absorption of the atmospheric carbonic acid. But it must be ad-
mitted that the principal aliment is derived from the soil through the
roots. Boyle's experiment on a willow branch which, in five years,
increased in weight one hundred and sixty-five pounds, while the
earth in the pot in which it was planted had only diminished two
ounces, does not prove that the plant was alimented solely or princi-
pally by the atmospheric carbon, because it had been from time to
time watered with water more or less charged with extractive sub-
stance. It must be remarked that when plants constantly impart the
detritus of their leaves to the soil in which they grow, they enrich it
with alimentary carbon instead of exhausting it, which proves that a
portion of their alimentary carbon is derived from the atmosphere, but
that portion would be insufficient for their aliment if unassisted by the
carbon introduced through the medium of the roots. It results from
this two-fold origin of the alimentary carbon, that the plant in its annual
fall of leaves, and in its decomposition after death, communicates to
the soil more carbon than it had received from it. Every year,
therefore, that a tree lives, it not only enriches the soil in which it
grows, but it acquires more powerful sources of aliment, because to
the atmospheric carbon, the source of which is always the same, it
adds the earthy carbon, the quantity of which is annually augmented.
This earthy carbon necessarily becomes exhausted in the fields,
when we remove the vegetables which they have produced; thence
the necessity of replacing this carbon, or rather this extractive, by
the addition of manure. This may also be effected, although with
less advantage, by growing herbaceous plants and burying them in
the soil under the plough; these plants, in their decomposition, return to the soil the carbon which they had borrowed from it, and also enrich it with that which they had derived from the atmosphere. It may appear paradoxical, but it is unquestionably true, that some soils furnish too much aliment to plants: thus wheat growing in a very rich soil, will have an exuberance of leaves, and the stems, borne down by their own weight, are bent towards the earth, by which the vegetation is suspended and no grain is produced. Even the stems which remain in an upright position produce very little grain; the superabundance of nutrition, producing in the plant the same effect as obesity in animals, considerably diminishes its generative power. The cause of this phenomenon must be sought in an examination of the mechanism of vegetable nutrition.

The alimentary carbon derived from the soil is converted into carbonic acid by the oxygen of the atmosphere, which the plant absorbs by means of its respiration; this carbonic acid is then deprived of its oxygen by the influence of the light, and it is then that the alimentary carbon, being set at liberty, assimilates itself with the plant which it nourishes. It follows, therefore, that if the extractive matter obtained from the soil be so great, that it cannot all be modified by the oxygen of the air, and assimilated by the light as abovementioned, the plant will be gorged with juices imperfectly prepared, which will be incapable of performing the most important act of organization, that of sexual generation. The nearer the stems are to each other, the more this effect on the leaves will be perceptible, since they mutually keep the light from each other, and the influence of light being indispensably necessary to the elaboration of the nutritive juices, many of the leaves, particularly the lower ones, become blanched and die. Count Chaptal, in his ‘Chimie appliquée à l’Agriculture,’ has noticed this phenomenon, and has also remarked, that when, by excess of manure, a soil has been raised to an exuberant state of nutrition, the produce will taste of the manure, which proves that the alimentary carbon has not been properly elaborated or prepared: thus the quality and flavour of grapes grown on a vine which has been abundantly manured, will be found far inferior to those grown on a vine which has been carefully and moderately manured. We often hear agriculturists remark, that fields of wheat, which have appeared luxuriant and flourishing in the spring, have belied their promise, and afforded but indifferent harvests; the cause of this is now evident to the physiologist,—the plants had derived too abundant a nutrition, and their functions, instead of being properly developed, were smothered and rendered inert. Hence, it is of the greatest importance, for practical agriculturists to study well the character of the soil with which they have to do, that they may avoid giving it too much, as well as too little artificial nutrition. It is well known to horticulturists, that trees which exhibit a great luxuriance of boughs and leaves, rarely, if ever, produce fruits or flowers; this is also a result of a superabundance of nutrition, but is produced in a different
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manner from that which has just been noticed with respect to wheat. In the latter, the organs of fructification appear, but produce no effect because they cannot perfectly perform their functions; in the trees, on the contrary, the organs of fructification do not appear at all. The flower-buds are, in fact, merely metamorphosed leaf-buds; in order to effect this metamorphosis, it is necessary that the buds should remain a certain time stationary. It is while the bud is in this state of apparent repose, that the formation of the organs of reproduction (which are merely leaves in another form) takes place. In those trees in which the vegetation is too vigorous, the sap is impelled so constantly and so powerfully towards all the buds, that they are never allowed the state of repose necessary to the conversion of the foliaceous organs into floral organs, and consequently are immediately developed into branches laden with leaves. This is the true cause of the absence of flowers in the trees of the tropics, which has been particularly remarked by M. Auguste de St. Hilaire, with respect to the forests in Brasil. This observation explains the means by which horticulturists are enabled to raise fruits out of their natural season, as cherries in January, grapes in April, &c. In order to effect this, it is only necessary to change the period at which the buds remain in that stationary position, which enables them to develop the germs of the floral organs. For this purpose the plants are kept during the winter in a hot-house, where their vegetation is very active; they are then taken out in the spring and exposed to the north, in a situation sheltered from the solar rays. This change of temperature suspends the vegetation, the plants lose their leaves, the buds become stationary, but being in a temperature favourable to the internal vegetative progress, they form the floral organs, which would never have been called into existence, had they continued in the hot-house. After two or three months of this aestival hybernation, the plant is replaced in the hot-house, the vegetation recommences; the flower-buds which have been elaborated during its repose become developed, and thus the time of efflorescence having been artificially changed, fruit is produced, which attains maturity at a season different from that which nature has assigned for its development.

The memoir was referred to a committee, consisting of Messrs. Mirbel, Cassini, and Auguste de St. Hilaire.

Night-blowing Flowers.—On the 8th of August M. Auguste St. Hilaire read a report on a memoir entitled 'Flore Nocturne,' by M. Viret. The object of this memoir was to explain the causes of the well-known phenomena of the closing or sleep of plants. M. Viret thus details his theory:—'My experiments have led me to the following conclusions. Cold and wet diminish the transpiration of plants. In that case the sap, instead of ascending towards the top in the branches of the leaves and flowers, as in the daytime, descends towards the roots. Hence the sap-vessels, or the upper parts, which,
in so many plants, are very thin and fragile, remain empty, and are compressed by their natural spring. This is the reason that so many compound and syngeneceous flowers, such as the malvacea, the convolvuli, &c., close during the night, and even when the sky is cloudy. When, on the contrary, the sun is radiant in the horizon, the heat and light soon recall an abundant sap into the branches, and the petals of the flowers open. Thus the flowers of the wild anemone, when closed by the cold, will, if brought into a warm place, or even if their peduncles are plunged into warm water, so as to occasion an ascent of the sap, unfold their petals, and resume their primitive vigour.

The same principle accounts for the opposite phenomenon observed in night-flowers: they are closed in the day, because the heat and light of the sun are too powerful for the frail texture of certain petals, and evaporate too much of the sap and nutritious juices which fill their vessels. But in the freshness of the evening, when the sap and juice, not being so powerfully evaporated, rest in greater quantity in the tissue of the plants, they dilate those vessels, and the flowers re-open. The reporter remarked that this theory is precisely similar to that of Adamson; but in order to admit that it is strictly true, it must be established that the nocturnal plants, which are represented as being liable to be too powerfully acted on by the solar rays, are always more frail and delicate than those diurnal ones which only flourish under the strong influence of the sun. This, however, is not the case, since the cistus, the aquatic ranunculi, the celemacea, the hydrocharices, and certain of the cruciform genus, which are diurnal plants, are flowers of extreme fragility and delicacy, and ought, therefore, according to the theory, to have their sap so completely evaporated, that their petals would be closed all day, and only open at night. The reporter, however, commended the industry of M. Viret, and recommended the Academy to request him to continue to communicate the result of his experiments and observations on the closings of the petals or flowers, as likely to tend to the establishment of a theory not liable to the objections raised to that now put forth. The report was adopted.

Self-fecundation.—At the same meeting, M. Dureau de Lamalle presented a piece of female hemp, which he believed to have been fecundated without having been subjected to the influence of any male hemp. He remarked that, although self-fecundation was a principle which could not in any case be admitted, he had seen reason to believe that some plants possessed the property belonging to some insects, of being fecundated for several generations in advance, by which means the later generations would present the appearance of fecundation without the intervention of a male; whence has arisen the idea of self-fecundation.

Flowers of the Resedæ.—On the 5th of September, M. Auguste
St. Hilaire presented a memoir on the flowers of the resedæ, of which the following is a summary:—After recalling the observation made by M. de Mirbel on the want of consistency in vegetable organization, and remarking that, in some genera, a particular class of organs will occupy precisely the same place as is occupied by a totally different class in a neighbouring genus, he observes that the family of the resedæ offers a remarkable example of this species of transpositions. After a general description of the flowers, he examines their parts in detail, and remarks that the petals in the bud are at first perfectly simple, singly trilobate, and composed only of a cellular tissue, which is more fully organized at the summit than at the base. He observes that they then become denticulated and laciniated; and perceiving the rudiments of a second petal appear at the base, he arrives at the conclusions that each petal of the reseda is composed of two opposed and soldered or connected petals; or rather that the corolla of the great part of the resedæ is composed of two verticilli immediately surrounding the pistil. It has been said by many authors, that the centre of the flowers of the resedæ contains a support, surmounted by a lateral discus, the stamina, and the ovary; but this is not the case. In the greater number of species, the support is hollowed at the summit, and forms a kind of calix, the top of which incloses the base of the ovary. The calix is formed of two verticilli, closely attached one over the other. The exterior verticillus is composed of nectarian scales, attached together, equal in number to those of the petals, and alternating with them, while the interior verticillus is formed of the fixed base of the stamina, really monadelphic. Sometimes the edges of all the nectarine scales are developed; but in general all but one become abortions; and in every case the alternation is preserved. The reseda luteola is, however, apparently an exception to this rule, as the edge of the only scale which is developed is in opposition to one of the petals. But as it is shown that the petal is composed of two petals fastened together, the alternation, in fact, still exists. From this observation, M. St. Hilaire concludes that, in the flower which is the prototype of the resedæ, the additions take place in the upper, and the suppressions in the lower part. He then proceeds to consider the staminal verticillus, first alone, and next in its relations with the petals. He shows that the movements which take place in the stamina of the resedæ do not result from the ordinary physical laws, but from a vital force which escapes our means of observation, and points out several analogous phenomena in other plants, particularly the darilla rugosa, in which the great divisions of the calyx close over the young pericarpium, allow it to ripen like the seed in a pod, open to allow its escape at maturity, and then close again. By an examination of the reseda alba, he proves that the number 10 is the type of the staminal verticilli of the reseda, and that this number, which presents, by turns, alternations and oppositions, is disguised in the different species by multiplications and unlinings.
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(dedoublement). From these examinations it results that the flower of the resedæ is composed—1st, of the verticillus of the calyx; 2nd, of a verticillus of petals, alternating with the calyx; 3rd, of a second row of petals, opposed to the first, and fastened to it; 4th, of a verticillus of nectarian scales, alternating with the double row of petals; 5th, of the stamina; and 6th, of the gynceum. Now the flower which is the prototype of the dicotyledons, presents—1st, a calyx; 2nd, a corolla, composed of as many parts, alternating with those of the calyx, as there are divisions in the latter; 3rd, stamina opposed to, and corresponding in number with the petals; 4th, the nectary; and 5th, the gynceum. We therefore find in the flower of the resedæ as many orders of parts as there are in the prototype or pattern-flower. And if, in order to arrive at an exact comparison, we regard these different orders of parts according to the rank which they occupy in the floral receptacle, we shall find that the second row of petals in the resedæ corresponds with the opposed stamina of the pattern flower, the nectarian scales with the alternating stamina, and the staminal verticillus of the resedæ with the nectary of the pattern-flower; thus proving that the extreme mobility of vegetable organization admits of different parts of the flower changing their places in kindred genera.

Plants of Chili.—On the same day Messrs. H. Cassini and Mirbel made the following report on a memoir by Adrien de Jussieu, entitled 'Observations sur quelques plantes du Chili.' The memoir consists of detached observations, which it is sufficient to notice where they present any feature of peculiar novelty. The genus Francoa, placed by M. de Candolle near the Rosaceœ, is placed by M. de Jussieu, in accordance with the opinion of M. Don, nearer to the saxifragœ. M. de Jussieu proposes to unite the genus Francoa with the genus Tetilla, in a small group bordering on, but sufficiently distinct from the saxifragœ, and to be called Francoacœa. He also proposes a new genus, to be called Ercilla, and to belong to the family of the menispermeœ. M. de Jussieu gives a more complete description of the genus Villaresia than had been given by its authors Ruis and Pavon; and without going the length of asserting that he has resolved the problem of its proper classification, he points out several striking points of analogy between the genus in question and the holly (ilex aquifolium). The genus Decostea, placed by M. Knutt in the suite of the Juglandes, is, from its striking resemblance to the Aucuba, attributed by M. de Jussieu, without hesitation, to the family of the Corni.

A very small plant of the order of the Onagianes, discovered by M. Gay de Draguignan in the mountains of the province of St. Jago, is made the foundation of a new genus, called by M. de Jussieu Gayophytum, in honour of the discoverer: its principal peculiarity is, that the number of cells (loges) of the ovary is only half of that of the petals.
Among the Euphorbiæ (Euphorbiacées) of Chili, M. de Jussieu remarks particularly the Croton lanceolatum of Cavanillas, in which he finds the type of a new genus, to which he has given the name of Chiropetalum, expressive of the peculiarity of its petals being divided into palmated straps. This genus, and those analogous to it, form a very natural little group in the tribe, remarkable for the colouring principle which is peculiar to them. The author also adds a new species to the genus Colliguaya, but considers it as scarcely varying from the Exacœcaria. He then describes a new kindred genus, which he calls Adenopatris, and thence takes occasion to offer some remarks on the groups of Hippomaniæ and Euphorbiæ, which he proposes to unite into one, in which case this section, or tribe, of the order of the Euphorbiæ (Euphorbiacées) would be distinguishable by the almost constant existence of a milky sap (suc laiteux), and by the extreme simplicity of the organs of fructification, which renders it necessary to have recourse to the original disposition (inflorescence) in order to find the generic characters. The memoir terminates with a list of the genera now admitted by the author in this tribe, which is too long for insertion, particularly as we have mentioned all the new ones suggested by M. de Jussieu. The report concluded by recommending the memoir to the approbation of the Academy.

Ligneous Fibres.—On the 19th September, Messrs. Mirbel, Desfontaines and Cassini, made the following report on a work by M. Paiteau, entitled 'Mémoires tendant à faire admettre au nombre des vérités démontrées la théorie de La Hire, touchant l'origine et la direction des fibres ligneux dans les végétaux.' In the memoirs of the Academy for the year 1708, La Hire maintained that the ligneous layers which occasion the increase of bulk in trees have a direction from top to bottom; that they proceed from the buds, of which they are the roots; and that, like other roots, they have a tendency to bury themselves in the earth. In fact the annual beds of the trunk have one end attached to the branches springing from the buds, and the other to the roots concealed in the earth, forming between these parts a necessary connecting link which cannot be broken without injuring both the buds and the roots. La Hire's idea, however, went much further; he imagined that each bud produces roots, which form with the roots of the other buds a sort of case between the old wood and the bark: this case, which is merely the annual bed, descends gradually from the top of the highest trees down to below the surface of the earth, where all the roots separate, and assume the form in which we find them. When this hypothesis is admitted, it is easy to explain how a swelling is formed at the upper part from a wound or ligature on the trunk of a tree; the roots of the buds, not being able to pass beyond that spot, collect into a mass, and strengthen and thicken instead of elongating. Then one of two things happen—either the swelling becomes sufficiently deve-
loped to admit of the communication between the upper and lower part of the trunk being sufficiently re-established, or the development is insufficient for that purpose: in the first case, the buds are saved, because their roots attain the earth; in the second, they are exposed to die of inanition, and the tree will perish with them, as they have no root. This hypothesis of La Hire excited so little attention, that when the same thing was asserted nearly a century afterwards by Du Petit Thouars, every one (including probably that learned naturalist himself) believed it an entirely new theory. For twenty-five years Du Petit Thouars defended it with a perseverance worthy of a better cause, but he gained no proselytes, as the experiments made by Duhamel and others demonstrated the fallacy of his reasoning; but a new supporter of it having come forward in the person of M. Poiteau, the committee nominated by the Academy have made the following fresh experiments:—If a large ring of bark be taken from the trunk of a sycamore maple, and replaced by a similar ring from the bark of a red maple entirely devoid of buds, the latter will graft itself as a cutting or graft would do, and below it a bed of red maple wood will speedily be developed. The grain of the wood will leave no doubt as to its nature. This ligneous production cannot be derived from the buds of the red maple, because the ring of bark was devoid of any; nor can it result from those of the sycamore, because they could only produce sycamore wood. It must, therefore, owe its origin to some other cause than the lengthening of the roots of the buds: and even if the bed of wood formed in the trunk of the sycamore below the ring of red maple bark were to be sycamore, and not red maple wood, still, as this bed of wood would be separated from the corresponding bed in the upper part of the trunk by the whole width of the ring of red maple bark, it is impossible to imagine that it could spring from the buds of the sycamore. Again, if a large ring of bark be taken from the trunk of a vigorous elm, or one of many other dicotyledonous trees, without being replaced by any thing, new beds of wood will be formed in the lower as well as in the upper part of the trunk, while no ligneous production will appear on the ring of wood left exposed by the removal of the bark; the formation, therefore, of the buds on the lower part of the trunk cannot be attributed to the development of the supposed roots of the buds, as they could not descend from the top of the tree to the earth through the exposed ring of wood without being perceived. The present memoir contains no new facts, but merely gives the theory the powerful support of M. Poiteau's opinion; the reporters, therefore, conclude, that, as the theory of La Hire is in direct contradiction to facts, the Academy cannot bestow its approbation on the memoir of M. Poiteau.

Generation of Plants.—At the same meeting, Messrs. Sylvestre, Mirbel and Cassini presented the following report on a memoir by M. Giroux de Busaringues, entitled 'Sur la Génération des Plantes et
The work is divided into two parts; the first of which contains the details of experiments made by the author on hemp, spinach, the *Lychoris dioica*, and wild sorrel, with a view of investigating a variety of phenomena in the generation of plants. M. Giroux has, with incredible patience, made his observations on 20,000 individual plants, 14,000 of which were hemp. In the hemp and spinach, he collected separately the seeds of the top, the middle, and the bottom of the ear, as well as those of the lowest branches and the thinnest stocks; in the *Lychoris dioica* those of the top and bottom of the trophosperma; in the sorrel those of the top and bottom of the ear: he then weighed 100 grains of hemp, taken at random from each of the three parts of the last, and found that, assuming the volume of the grains to be in proportion to their weight, those of the bottom part of the ear are the smallest, and those of the middle the largest. He then sowed, separately, each of the qualities of seed, according to the divisions above-mentioned. A short time after the germination, he reduced the number of the hemp to 5000, by suppressing many of the females, which would have injured the development of the others, and by the total removal of the males. Notwithstanding the absence of the latter, all the females which were preserved afforded an ample crop of fecund seed; but it cannot be concluded from this fact, that the fecundation took place without the intervention of male organs, because it is well known, that beneath the envelope of the female flowers of hemp, there are frequently stamina which cannot be removed without mutilating the adjacent organs; and although these stamina are generally malformed, there is nothing to prove that some among them may not enjoy the property of fecundation. A similar experiment, tried on the *Lychoris dioica*, with a similar result, is more conclusive, if M. Giroux be certain that every precaution of isolation was observed, and that all the flowers were either entirely devoid of stamina, or had their antherse completely removed before the emission of pollen; but he must assure us of this unequivocally before we can admit the conclusion. The following are the general results obtained by M. Giroux from the above and other experiments:—

1. The seeds taken from the summit, either of the ear or the trophosperma, have constantly produced more females than those taken from the lower part. 2. In hemp, those taken from the lower part produced more females than those taken from the middle. 3. The seeds taken from the thinnest stalks, both from hemp and spinach, produced more males. 4. The hemp seed of medium size produced more females. 5. In hemp, the size of the plants has been in proportion to that of the seeds. 6. The numerical relation between the male and female hemp, produced from seeds developed, some in presence of the males and others in their absence, did not offer any susceptible difference. In the second part, M. Giroux has extended, to a greater length than has ever been done by any preceding naturalist, his observations on the difference between the males and females of a variety
of monœci, polygamia; and dioeci, not only in their flowers, but their leaves, stems, branches, roots, &c. It would be useless to detail their minute differences of organization, as they can only be understood by a close inspection of the natural flower. M. Giroux imagined that there are special relations between the male flowers and the peripheric beds of the stem, and between the female flowers and the central beds; and with a view of ascertaining the reality and generality of the fact, he extended his observations to an immense number of plants, both endogenous and exogenous. From these observations he concludes, that when the vegetation of the superficial beds exhausts itself in forming leaves, the plant becomes adapted for the production of female flowers; and that when the central beds exhaust themselves in producing female flowers, the plant becomes adapted for the production of male flowers, and, therefore, that the distribution of the sexes in the monœci and polygamie depends on the relative state of these two orders of vegetation. The reporters consider this last theory as, at least, premature, but, on the whole, recommend the memoir to the approbation of the Academy.

Exogenous Plants.—On the 19th of September, a report was made by M. Auguste de St. Hilaire, in the name of himself and Messrs. Desfontaines and Mirbel, on M. Giroux de Busaringues' memoir on the evolution and growth of exogenous plants. The report commences by giving the following seventeen propositions, as the result of the arguments and experiments detailed in the memoir. 1. Plants are divided into beds (couches), more or less exterior, and more or less interior. 2. There are two causes of vegetation: the gas contained in the atmosphere, and the humidity existing in the soil. 3. The exterior beds, or layers (couches) of plants are, during their evolution, principally subjected to the influence of the atmosphere; the interior beds to that of the humidity of the soil. The development of both is in proportion to the relative predominance of these two influences. 4. At the point of junction of each of the leaves of the plant at its birth, these two causes of vegetation combine, and by that combination, produce an interior longitudinal fold, which embraces all the layers of the stem from the pith to the circumference, and it is from this fold that the buds derive their origin. 5. Each foliaceous organ of the bud is specially derived from one of the folds of the superficial beds of the stem. 6. The leaf itself produces the bark and a part of the interior bed. These interior parts form the nerves, which are duplicatures or folds united under a cortical envelope. 7. The size of a nerve is always in proportion to the number and volume of those which border on it; the size of the petioles to the number and volume of the principal nerves; the size of the stem to the number and volume of the petioles which it bears; the size of a principal stem to the number and volume of its vessels. 8. When no obstacle exists, every longitudinal fold formed in the stem continues to the root, and every
fold formed on the root is prolonged to the branches. 9. By this continuation from top to bottom, and from bottom to top, the folds determine the prolongation of the cellular tissue, whence afterwards result the vessels. 10. The evolution of a bud takes place in the same manner as that of a bulbous root, conformably to the order in which the leaves composing it are set on or jointed; the lowest leaves of the flower were the outermost in the bud, and the highest the most central. If, then, the stem of an exogenous annual be in imagination traced back to the plate or species of cone, which would be represented by that of the root, the innermost layers would answer to the highest verticilli, and the most superficial beds to the lowest verticilli. Although this proposition is not in accordance with the observation of former authors, the reporters consider it as highly interesting, and worthy of being carefully examined by physiologists. 11. The word verticillius must not be wholly considered in the sense in which it has been hitherto used by botanists; every association of lateral productions of the same nature may be considered as a verticillius, when those productions, if arranged in imagination in a single plane perpendicular to the axis of the stem would move round it without meeting. 12. If the number of branches of the stem of an annual, not taking into account the small upper branches, be divided by the number of the verticilli, the quotient will be the number of layers of the lower part of the stem. This interesting observation has been verified by the reporters upon several pieces of atriplx patula. 13. The only difference between an annual and a perennial plant is in the duration. The evolution of each bud takes place in the second year, in the same manner as that of the embryo took place in the preceding year. In the branches, as in the stem, the outermost beds correspond with the lowest verticilli, and the innermost with the highest. On the other hand, the innermost beds of the branch correspond with the most central zones of the stem, and the outer beds of the former with the superficial zones of the latter. 14. The outer edge of the sap (qubier) is always the same; the increase in size of each bed takes place in the interior by the intercalation of new fibres. Thus the development of the exogenous, or dicotyledon, is in each bed really analogous to that of the endogenous or monocotyledon. 15. The inner beds press the exterior ones outwards; the fibres of the inner beds become intercalated with those of the outer ones; and thus the plant augments in circumference. 16. In the bark, the most recent fibrous bundles have a tendency to intermingle also with those of anterior formation; they push them outwards and pass beyond them, on the side turned towards the centre; whence it follows that the inner surface of the bark is incessantly changing. 17. The increase in size does not result from the addition of a new body to a body already in existence, but from a centrifugal evolution of the latter; this evolution is operated upwards, along the length of the stem, by the influence of the roots;
and downwards by the influence of the leaves. It will be observed that this theory of M. Giroux differs from that of all former physiologists, although it has a very slight analogy to that of the late M. du Petit-Thouars. The memoir in which the theory is developed, although long, is not sufficiently so for the purpose, as each proposition might have furnished matter for a separate memoir, if supported by such facts and multiplied experiments, and illustrated by such plates as would be necessary to establish its truth. Such a work would have been an entire treatise of vegetable anatomy and physics, and would occupy more time than M. Giroux de Busaringues (whose main attention is devoted to agriculture, and with whom physiological studies are but an occasional recreation) could afford to bestow upon it; and he has, consequently, rather detailed than proved his opinions. This memoir must, therefore, be considered rather as affording materials for the exercise of the industry of other physiologists, than as establishing any new theory. In this point of view the reporters consider the work to merit the approbation of the Academy.

_African Plants._—On the 26th of September, M. Auguste de St. Hilaire read a report on the memoir by M. Vallot, entitled 'Notice sur plusieurs Végétaux mentionnés par les Voyageurs modernes qui ont parcouru l'Afrique Centrale.' The object of this memoir is to refer to their proper places in the scientific nomenclature the different plants mentioned under their vulgar names by various modern travellers in Central Africa. The utility of a work of this description is self-evident; the gigantic strides made by the science of botany during the last half century, while they have led to a most intimate acquaintance with the minutest properties of plants, have also been greatly instrumental in confining botanists to a technical style of writing, which renders their accounts unintelligible to the mass of readers. At the same time the descriptions given by unscientific travellers, although more picturesque, and therefore more generally interesting, are necessarily deficient in those particulars which render the discovery of importance to the scientific world. A work, therefore, which, by showing the relation between the two descriptions, enables the reader of the work of amusement, at once to refer to the work of science, must be of immense advantage; and M. Vallot, in the execution of his task, has proved himself to be possessed both of sagacity and information. M. de St. Hilaire, however, points out a few errors into which he has inadvertently fallen. Thus the _Ochrademus_ is a reseda, and has nothing in common with the _Sodaba decidua_ of Forskal. The white-barked _Euphorbium_ of Senegal has been already described by M. Adrien de Jussieu, under the name of _Anthostema_, and it was, therefore, unnecessary to form it into a new genus. M. Vallot is also probably mistaken in supposing the _Cauza_ of Caillé (which he is right in considering as a _Spindia_) to be the _Monbin_ of America; as M. Perottel, who has travelled both in America and in Senegal, found several
Spondiae in the latter country, but not the true Monbin. With the exception of these trifling errors, the work of M. Vallot is highly useful; and in recommending it to the approbation of the Academy, M. de St. Hilaire expressed his hope that the author would extend his researches in a similar manner to the plants of other recently-explored parts of the world.

Vegetation in Brazil.—On the 18th of July, M. Auguste St. Hilaire read a memoir containing a variety of interesting particulars respecting the primitive vegetation of the province of Minas Geraes, in Brazil. The primitive vegetation, which has entirely disappeared in Europe, still exists in a great part of Brazilian America, and in the province in question it is particularly remarkable. The whole country is divided into matos (woods), and campos (open country). The woods belong partly to the primitive vegetation, and partly to human industry. The latter has been exerted to replace the forests which have been burnt; the former consists of virgin forests, properly so called—the catingas, which lose their leaves every year, and the carrascos, a species of dwarf forests, the trees of which are only from three to five feet high. The province is divided throughout its whole length by a chain of mountains, which extends from south to north, and gives birth to a multitude of flowers. The western part is merely undulated, but the eastern is mountainous; the former is open, the latter wooded; and these two vegetable regions form two zoological regions almost equally distinct. The various shades of difference existing in these two principal regions are included within limits almost as exactly defined as the principal regions themselves; and when, starting from the sea, and commencing from about fifteen degrees south latitude, we direct our course towards the south-west, we traverse in succession the virgin forests, the catingas, the carrascos, and the campos, a sort of vegetable ladder, in which the plants gradually diminish in height, because the humidity of the soil and of the atmosphere experiences also a gradual diminution. As the zone of the forests is divided into several sub-regions, so also even that of the campos, or open country, presents two very distinct subdivisions; for in the southern part of the province the campos are composed only of herbs and underwood, while in the northern part tortuous and stunted trees are scattered at intervals among the pasture land. If the physical constitution of the province of Minas Geraes has exercised a great influence on the primitive vegetation, its effect has not been less on that which has resulted from the labours of man, and which may be called artificial. Thus in the forest regions, a fetid grass, called ‘seed herb’ (herbe à la graine), takes possession of all the ground formerly covered with trees; but this grass does not show itself in the campos at all, and in the northern region its existence is confined to the subdivision of the carrascos. The pasture lands formed by this grass are called, in the country, artificial; but the campos, which are called natural, must also have been modified
by the presence of man. Every year these natural pasture lands are set on fire to procure the cattle a fresher grass, and it is evident that a number of annual species must have been destroyed in their vegetation by these repeated conflagrations. A single burning is even sufficient to modify, in a most singular manner, the plants already existing. Scarcely has the grass of a campo naturæ been consumed before dwarf plants are seen to appear here and there among the ashes; these plants are merely abortions of species naturally much larger, and intended to blossom at a different period of the year. The most trifling labours of man have an effect on the vegetation, and in some deserts even the halting-place of the traveller is marked by the appearance of particular plants. The nature of the superficial bed of the soil, no doubt, influences the details of vegetation in these provinces, but that cannot occasion the existence of woods on the east of the great chain, and of pasture lands on the west. In the forest regions, the hills are very high, and are terminated by ridges; deep and narrow valleys separate these hills, which shade each other reciprocally; the effect of the wind is not felt in this country, and the numerous brooks by which it is watered contribute to develop its vegetation. When, on the contrary, the country is only undulated, and there is nothing to impede the course of the winds, when the earth is not refreshed by brooks, it is impossible that the vegetation can be vigorous, however good the soil may be naturally.

Goëthe a Botanist.—Among the works presented to the Academy on the 25th of July, was an Essay on the Analogies and Metamorphoses of Plants, by Goëthe. In handing this work to the Secretary, M. Géoffroy St. Hilaire stated that he was instructed by the author to present to the Academy the only copy in Paris, to which, in order to show his respect and esteem for the Society, he (Goëthe) had caused a French translation to be annexed. The name of Goëthe, so eminent in the world of fiction, is but little known to the public as connected with scientific researches; and M. St. Hilaire therefore deemed it expedient to accompany the work with a few observations on its nature and contents. One-third of the volume consists of a reprint of the aphorisms published by the author in 1790, under the title of 'Essai sur la Métamorphose des Plantes.' This work was disregarded at the time, and Goëthe was censured for having published it; but his only fault was, that he outstripped the age in which he lived, and published a work on plants half a century before there were any botanists who could read or understand it. The second part of the work is composed of additions to, and comments on, the first part; and the author there takes occasion to vindicate his claim to a place in the scientific world, by proving that great part of his existence has been devoted with passion and energy to the study of nature; and that he, therefore, is not to be considered merely as a philosopher wholly occupied
in depicting the internal history of man, or as a poet absorbed by
the fictions of scenic illusion. After 1810, the philosophic ideas of
Goethe were universally received; and amongst others, the illustrious
De Candolle developed them in his ‘Principes de la Symétrie et de
la Métamorphose des Plantes.’ Goethe removes the astonishment we
might feel at a poet, whose natural dispositions are generally sup-
posed to be adapted only for the appreciation of moral phenomena,
having been able to discern with so much precision the laws of the
development of the organs of plants, by detailing, in a minute and
interesting manner, the history and progress of his scientific studies.
In the third and last part, the author examines with much acuteness
the various ideas which have been published since the appearance of
his work upon the analogy of the parts of vegetables; his peculiar
susceptibility relative to the French doctrines is here unreservedly dis-
played, both in his exultation at the overthrow of what he charac-
terizes as a dictatorship, which had existed too long, and his regret
that some of his favourite ideas are not sufficiently encouraged.
M. Géoffroy concluded by remarking, that this work had been sent in
acknowledgment of an article written by him in the ‘Annales des
Sciences Naturelles,’ entitled ‘Sur les Écrits de Goethe, lui donnant
des droits au titre de Savant Naturaliste.’

CHEMISTRY.

On the Connexion between Chemical and Electrical Action.—On
the 11th of July, M. Becquerel read a memoir forming a continua-
tion of his attempts to trace the connexion between chemical and
electric action. The two subjects examined in this memoir are the
development of electricity by friction and phosphorescence. It has
been generally supposed that friction is produced by the reciprocal
interlacing of the rough parts of the surfaces brought into contact,
but from various experiments there is reason to believe that mole-
cular attraction is one of the causes of friction. This reaction of the
molecules on each other, by producing a derangement of their equi-
librium, must also disturb that of the electric forces, for it is now
unquestionable that electricity is disengaged whenever the molecules
of a body are displaced in any manner. Added to which, since
chemical action is one of the principal causes of disengagement, we
should examine how far the transitory alterations which the surfaces
of bodies undergo during friction, exercise an influence on the pro-
duction of the phenomena observed. And may not these phenomena,
which bear so strong a relation to those of heat, be owing, like them,
to vibratory motions of a particular species of that ethereal substance
which is supposed to be dispersed throughout all space? After
noticing some of the general effects of friction, M. Becquerel proceeds
to examine them in detail, as manifested in different substances.
When two plates of different metals are placed one at each end of
a copper wire, and brought into friction with each other, each of them
acquires an excess of contrary electricity, whence a current is imme-
Proceedings of the

diately produced. By trying the different metals in succession in this manner, a table is formed, in which each metal is negative as regards those which follow it. This order is precisely the same as that which is obtained for thermo-electric effects, in the closed circles composed of two of the same metals, when the temperature of one of the junctions is raised. It is demonstrated that the kind of electricity acquired by each plate is independent of the greater or less degree of friction which each of them undergoes; and, consequently, that the effect cannot be produced by the greater or less degree of heat produced by the friction upon each of the surfaces. Since the electric effects of friction are similar to those obtained by raising the temperature of one of the points of junction of two plates, it would appear that the heat produced by the friction is the cause of the phenomenon; while, on the other hand, it may be asked, does not the friction, by augmenting the power of attraction of the two bodies, heighten the electric effects which result from the action of this power, or does it not produce a particular concussion in the molecules of each body, the difference of which produces these effects? But if two plates be struck against each other without friction, no electric effect is produced; the two disengaged electricities recombine upon the surface of contact, and there is no current. Yet in this experiment there is a much greater concussion of the molecules of the two surfaces, and a much greater liberation of heat than in the slight friction of the plates against each other. We must, therefore, admit that if the displacement of the molecules disengages simultaneously heat and electricity, these two effects are independent of each other; it is even probable that the body which is the most heated, or the parts of which are the most displaced, is that which assumes the negative electricity. M. Becquerel then demonstrates that there ought not to be any electricity rendered free by the vibration of metallic cords, although there are electrical phenomena from molecule to molecule; and afterwards proceeds to examine the modifications produced in the phenomena by the reduction of one of the metals into filings, and by variations made in the temperature. When the filings of a metal are thrown upon a plate of the same metal, the latter acquires an excess of positive electricity, and the filings an excess of negative electricity. Generally, filings of metal have a tendency to negative electricity; but the filings of a positive metal will, notwithstanding this tendency, be positive with relation to metals of a more negative tendency. The author proves, by a variety of experiments, that these effects are produced, not by a difference in the action exercised on the metals by the air and water contained in them, nor by the heat which is disengaged, but by a difference in the mode of aggregation of the molecules of the surfaces, and consequently of their faculties of vibration.

The influence of the molecular state on this phenomenon may be still more strongly illustrated by means of an apparatus giving a rapid rotatory movement to the plate on which the filings are thrown.
Thus, either the peroxide of manganese, silver, or sulphate of iron, filed into very small particles and thrown on to a plate of zinc, tin, or gold in motion, assumes a negative electricity. Filings of zinc when thrown upon a plate of the same metal in motion exhibit no electricity, but become electric when the plate is in repose. Hence it appears that the swiftness of rotation augments the negative tendency of the zinc plate, probably by producing a concussion of all the molecules on the surface. There is great reason to suppose that the electric effects of decomposition and recomposition, operated during the concussion of the molecules, may furnish a clue to the cause of the magnetic phenomena observed by M. Arago in metallic plates when in motion. The general result of all M. Becquerel's experiments establishes that, when friction is produced between any two metals whatever, either in repose or in motion, that metal the parts of whose surface are most displaced becomes affected with negative electricity.

M. Becquerel also examines the effect of friction in bodies which are bad conductors of electricity; and although the variation of these effects, according to the state of the surfaces, renders the solution of the problem more difficult than when metals are employed, the relation between the phenomena in the two classes is plainly to be perceived. Thus fibrous substances are more strongly susceptible of electric affection than other bodies, because their particles are more easily displaced; for the same reason, heat augments the negative tendency of bodies. From these experiments M. Becquerel was easily led to consider the phenomenon of phosphorescence, and a great number of experiments have convinced him that electricity is disengaged whenever a change of equilibrium is operated in the molecules of bodies. This phenomenon he considers to consist in the separation of the two electricities, the composition of which produces, according to its greater or less rapidity, light, heat, chemical or magnetic effects.

Phosphorescence is produced by heat, light, percussion, the electric shock, certain chemical actions, and sometimes by the exposure to a high temperature, which occasions the body to lose its faculty of entering into combination with others. M. Becquerel proves that all these causes may disturb the equilibrium of the electric forces without occasioning free electricity to be disengaged. He has also proved that, when two bodies combine, that which acts the part of acid assumes, with respect to the other, a positive electricity, and that which acts the part of alkali assumes a negative electricity. In many cases these two electricities instantly recombine; but when the action is slow and the bodies are bad conductors, recomposition does not take place until the two electricities have both acquired a tension sufficient to enable them to overcome the obstacle opposed to their reunion by the want of conductibility. This is probably the cause of the phosphorescence produced in some slow chemical actions which take place spontaneously in the air, as in the earthy
sulphates, rotten wood or fish, &c. There are numerous other cases 
in which phosphorescence may be explained by the recomposition 
of the two electricities disengaged in consequence of a derangement in 
the molecules of bodies. On cleaving a crystal light is frequently 
observed, which is evidently electric. But may we not also attribu-
tate the phosphorescence produced by heat to a partial cleavage, 
or a displacement of the superposed plates? If certain bodies, 
after having been highly heated, lose the property of becoming 
phosphorescent, is it not because the heat has produced in the rela-
tive situation of their molecules a derangement which renders those 
regular displacements impossible? And is not the luminous ap-
pearance produced by percussion, and even that produced by 
grinding, when no free electricity is disengaged, to be attributed 
to another change in the position of the molecules, and consequently 
to an electric decomposition and recomposition? By a similar appli-
cation of the same principle, M. Becquerel accounts for the phos-
phorescence observed in some bases, such as zircon, which, when it 
is produced, deprives those bases of their faculty of combining with 
the acids. Finally, M. Becquerel considers that the phosphorescence 
produced by electric discharges, which is observed in bodies which 
are bad conductors, depends on a species of cleavage which gives 
rise to a decomposition and recomposition of electricity. This action 
is only successive on account of the bad conductibility of the bodies; 
and during the whole time that a portion of the two electricities 
which have become free remains engaged between the molecules of 
a body, that body has a luminous appearance.

**Bromide of Silicium: New Chemical Compound.** — On the 
26th of September, M. Serullas read a note on a new compound of 
bromine and silicium, which he calls bromide of silicium. This com-
ound is obtained by mixing lamp-black, pulverized sugar, and a 
sufficient quantity of oil to form a homogeneous paste, with silica, 
hydrated and desiccated to a certain point. This paste must be cal-
cined in a covered crucible. The quantity of carbon contained in the 
different substances employed should be above half the weight of the 
silica. The carbonaceous and spongy residue of the calcination is 
introduced in small fragments into a porcelain tube, at one of the 
extremities of which is fixed a small retort containing the bromine, 
and at the other a tube, which terminates in a globe or balloon, 
surrounded with ice, and having affixed to it a long tube, terminated 
by a narrow opening. The porcelain tube being made incandescent, 
the bromine is volatilised by slow degrees by means of heat; the 
bromide of silicium is produced and condensed in a liquid form in the 
tube and receiver. When the operation is terminated, the bromide 
should be re-distilled, in the manner pointed out by M. Oerstadt 
for the chloride of the same base, after having shaken it, in the 
same retort in which it is to be distilled, with mercury, in order to 
get rid of the excess of bromine; this produces a magma of greater
or less thickness, which appears scarcely to contain any liquid, although a considerable quantity may be obtained from it by distil-
lation. Bromide of silicium, when distilled, is nearly colourless; it emits thick vapours in the air. When cooled in a frigori
cific composition, it becomes solid at from 12 to 15 degrees below zero (—10° or — 5° F.); partaking, in this respect, of the property of bromine. It is raised to a state of ebullition at from 148° to 190° (300°—374° F.); its density is greater than that of sulphuric acid, for it falls rapidly through that liquid, in which it is decomposed but slowly, it not being until after the lapse of several days that it is entirely con-
verted into silica and bromine; the latter being the result of the re-
action of the sulphuric acid on the hydrobromic acid. Potassium
acts violently on bromide of silicium at a very slight elevation of
temperature, producing a loud detonation which constantly fractures
the tube. The following are the principal points of difference be-
tween the chloride and the bromide of silicium. 1. The chloride
boils at 50° C., the bromide not until 150°. 2. The chloride, which
sinks in water, has less density than sulphuric acid; it remains on
the surface, and is there decomposed into silica and hydrochloric
acid. The bromide, on the contrary, is, as we have before seen,
heavier than sulphuric acid. 3. Potassium suffers no sensible altera-
tion from chloride of silicium in a state of ebullition, whereas a very
slight heat is sufficient to produce a violent action of that metal on
the bromide; which may be accounted for thus: The potassium
becomes fused before the ebullition of the bromide; the chloride, on
the contrary, boils before the fusion of the potassium can take place.
Indeed, if potassium which, from having been exposed to the air, is
beginning to liquefy, be dropped into bromide of silicium, the deto-
nation will take place instantly. 4. The chloride of silicium may
be cooled below — 20° C., without losing its liquid form; whereas we
have seen that the bromide becomes solid at from 12° to 15° below
zero. The bromide of magnesium may be obtained in the same
manner, by a mixture of carbon and carbonate of magnesia, &c.;
but it is difficult to procure it pure, because it is not volatile, and
does not fuse under a red heat; then, as fast as it is formed, a part
is carried off by the gas into the tube and the ball, which are ob-
scured by it under the form of a greyish powder, a mixture of chlo-
ride of magnesium, magnesia, and carbon; and another part remains
at the bottom of the porcelain tube, and in the first part of the tube
which corresponds with it, under the form of a molten mass, more
or less pure, whitish, and crystalline. Bromide of magnesium has
a powerful attraction for the humidity of the atmosphere, and acts
strongly on water, producing detonation and development of heat.

Geology.

Pyrenean Chain.—On the 26th of September, M. Neboul read an
interesting series of 'Observations on the Structure of the Pyrenees,'
of which the following is an abstract. He stated, that in endeavouring to determine the direction of the Pyrenean axis, and its relation both with the direction of the inclined strata, and with the principal parts of which the entire chain is composed, he arrived at the following conclusions:—1. That the Pyrenees are not directed from E. S. E. to W. N. W., but at least fifteen degrees southward of this line. 2. That the direction of the strata is rarely parallel to this axis. 3. That the Pyrenees do not constitute a simple chain which may be supposed to have been formed at a single ejection. 4. That they exhibit the traces of various subterraneous evulsions, by which they may be supposed to have been produced. 5. That these evulsions, which appear to have succeeded each other during the long durations of the ancient periods, were, like those of the Alps, continued into a considerably advanced epoch of the tertiary period. Both Pliny and Ptolemy have fallen into an error, in fixing the western promontory of the Pyrenees at a spot called Aso, which D'Anville supposes to be the Punta de Figuerra, near the mouth of the Bidassoa, and Gmelin the Cape Machicaco; but neither of these points form the termination of the Pyrenean chain. This chain, to which the promontories in question are mere appendages, leaves them to the north, and extends to the confines of Galicia, as was observed by Strabo. This error has been very universally adopted, and thence the direction of the Pyrenean chain has been usually stated to be from Cape Creus to the Punta de Figuerra, two extreme points; one of which is situated south, and the other north of the true direction of the Pyrenean axis. This axis really commences in the east, at Cape Cerveres, the crest of which forms the best separation between the torrents directed towards the north, and those directed towards the south. Its western termination is more difficult to decide with certainty, because, on approaching the sea of Galicia, the chain forks out into two branches, one of which terminates at Cape Ortegal, and the other at Cape Finisterre; a line drawn from Cape Cerveres to the point where the separation takes place, and thence extended to the sea, would terminate between the two capes near Corunna, and the island of Sisarga. This direction, which alone fulfils the conditions prescribed for a geographic axis, differs only six or seven degrees from the parallel of the equator. It varies but little from the extreme sinuosities of the crest or ridge, formed by the two declivities of the chain, divides the mountainous region most equally between these two declivities, and most naturally unites the extremities with the centre; the most remarkable summits with the culminating points, whence proceed the principal fluvial currents, such as the Aude, the Arèîge, and the Garonne, in France; and the Ebro, the Douro, and the Minho, in Spain. A chain may have several geological axes, arising from the direction of special rocks, or other causes; but these axes must be partial, except where they are, by parallelism, confounded with the central and geographical axis, which is, by its nature, single and universal. A granite axis
does not appear to exist in the Pyrenees. The masses of granite form, as it were, large islands in the chain, which do not agree either with themselves or with the geographical axis. The western region of the French Pyrenees contains tracts of aphyte, having nearly the same direction as the chain from east to west; but they do not exist in the eastern region. The French Pyrenees, particularly those valleys through which flow the streams tributary to the Garonne and the Adour, contain a number of oblique ridges, which, as well as their strata, are directed towards W. N. W., and sometimes even towards the N. W., whence has arisen the great error of applying this law to the whole chain, and supposing the direction of strata to be parallel to the Pyrenean axis; whereas, in fact, that axis and the strata directed towards the W. N. W., cut each other in an angle of at least fifteen degrees. This error might have been avoided by observing that the rule above alluded to is by no means a general one; some of the ridges of the Pyrenees are directed towards the W.S.W., and the strata follow the same direction; that of Canigou, for instance, on the summit of which are seen gneiss and micaceous schistus, having the same direction as the protuberance of which they form the pinnacle. A few strata are also found having the same direction as the total chain from east to west. The sinuosities of the small ridges, their obliquity with respect to the central axis, and their junctions in one sinuous summit, prove that the Pyrenean chain was thrown up at different epochs. This fact, which is derived from the irregularities of detail in the central crest, is confirmed by the relation of its great and principal divisions. Independently of the small chains which may be traced on the two declivities, there are three principal and distinct chains, which contribute to form the long summit of the Pyrenees. The ridge or chain which overhangs the eastern region follows the direction from E. N. E. to W. S. W. It extends from the plain of Roussillon to that of Catalonia, skirting in France the right bank of the Est, and in Spain the left bank of the Segre. Its numerous summits attain heights of 1400 to 1500 toises; the Perigmal of Cerdagna is the most elevated point. The central crest, which is cut by this ridge between Mont Louis and Prati de Mallo, is much inferior to it in height. The valleys of the Est and the Segre form, at the foot of this chain, the only longitudinal section of this nature which exists in the whole Pyrenean chain. The great basin of Cerdagna (the largest in the Pyrenees) occupies the culminating point of the double valley, which appears to be situated in the linear direction of the summit, about 600 toises below the highest point. To the N. W. of this basin rises the second ridge, directed towards the W. ½ N.; its height is nearly 1500 toises, at the source of the eastern Arriege, and exceeds that elevation in the region of the western Arriege. It extends into the region of the Salat, and to the first branches of the valley of Etran; then gradually diminishes in height, and is lost amid the mountains of the French declivity,
This is the chain, the linear direction of which would, if prolonged, terminate near the mouth of the Bidassoa. The general summit of the Pyrenees passes suddenly from the ridge, to another more southerly, which is the principal, as it embraces in its almost parallel linear direction the most remarkable points of the chain. It may easily be seen that this ridge is not, as has been supposed by some geologists, united with the preceding by means of a winding fold; and that the two ridges only join by their inverse declivities in the basin of Beret, which, as well as the great basin of Cerdagna, was formerly a lake, the waters of which must have flowed simultaneously towards France, and towards Spain. We, therefore, see that the Pyrenean chain, simple as it is, is composed of several ridges, having different directions, both in their masses and in their strata; which proves the error of M. Elie de Beaumont, in supposing the Pyrenees to have been formed at a single ejection, by the application of his own principle, that eminences having different directions are the result of different evulsions. The Pyrenees contain numerous indications of rocks thrown up at various epochs. The most ancient of these is the presence of dry storax in the grauwaken of Maladetta, and in the deposits of anthracite of the intermediary formations. The period at which these ancient formations were thrown up cannot well be ascertained, but it is certain that when they were formed, the plants, the remains of which are buried in them, crowned the heights round their basins, which heights were already mountains. The only spot in which the sedimentary tertiary formation, not alluvial, and similar to that of the Herault, and the Apennines, is found in contact with the Pyrenean rocks, is where the Est empties itself into the plain; for from the borders of the sea of Gascony to the mouth of the Est, in the Mediterranean, the chain appears surrounded with alluvial formations; but at Nassaich, near Millas, the sands of the shelly deposit leave uncovered a large fragment of molanes, and of bluish sandy marl fixed to the Pyrenean quartz-rock. This curious fact is sufficient to destroy the hypothesis of the Pyrenees being anterior to the Alps, and would even, perhaps, authorise a contrary conclusion, as the glauconian deposits which, in the Pyrenees, occupy the central point of Mont Perdu, are only met with in the Alps on the eastern summits and at medium heights; such as the mountain of Fis, near Serrais, and that of Diableritz in the Lower Valais; and the molanes which, in the Pyrenees, rest immediately on the rocks of the central ridge, in the Alps, do not attain that ridge at all, but occupy only a part of the exterior chain, which, according to Saussure, belongs rather to the system of the Jura, than to that of the Alps of Mont Blanc. In conclusion, M. Nebaul remarks that as the greater part of mountainous systems (except the volcanic ones) have a great resemblance in the composition and the dispositions of their rocks, it is probable that their differences arise much more from the accidents of locality than from any general relations derived from the period of the commencement or completion of their formation.
This formation is the most striking and the most universal of the ancient geological periods. The indications of the throwing up of rocks occupy some epochs of the primitive, and all those of the secondary period; they are also frequently remarked during the tertiary period, when the great terrestrial evulsions which had produced the large chains of mountains began to be replaced by the volcanic eruptions and earthquakes, of which we are still witnesses.

**Medical Science.**

*Cure of Burns.*—On the 4th of July M. Magnin de Grandmont addressed a letter to the Academy, detailing several cases in confirmation of his theory of immersion in cold water being an infallible remedy in all cases of burns affecting only the *epidermis*, which, he remarks, are by far the most numerous class of burns.

*Cholera Morbus.*—On the 18th of July M. Magendie read a letter from M. Scipion Pinel, a surgeon at Warsaw, in which it is maintained that the cholera morbus affects principally the sympathetic nervous system, as is proved by the weakness produced in the general circulation, which is not accounted for by any sufficient affection of the heart, or the organs of circulation, and can only, therefore, be produced by a diseased state of the nerves, particularly the grand sympathetic or *trisplanchnic* nerve. He therefore proposes to give the disease the name of *trisplanchnic*. M. Pinel adds, in proof of the disease not being directly contagious, that he has infused into his own veins, not only the blood of a dying patient, but even the intestine mucus taken from a dead body. But he remarks, that when he remains more than a quarter of an hour in the room with the patients, he experiences a feeling of painful oppression in the stomach, in the direction of the vertebral column, which is removed by going into the open air. The treatment recommended by M. Pinel differs principally from that hitherto adopted, in prohibiting narcotics; but he agrees with other physicians in recommending warm drinks, and all other applications tending to restore heat to the surface of the body, and increase the circulation.

*Anatomical Plates.*—On the 25th of July, the Academy, on the recommendation of its reporter, M. Dumeril, bestowed its special approbation on the ‘Traité Complet de l’Anatomie de l’Homme,’ by MM. Bourgery and Jacob. This work is illustrated by 500 plates, executed with the greatest exactness.

*Lithotrit/.—On the 5th of August M. Civiale communicated to the Academy a case of lithotritry, in which the calculus was six inches in circumference. Great difficulty was experienced in fixing it in the instrument, although the orifice of the pincers was twenty-six lines in diameter. The operation was completed in fourteen visits.

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The first ten were employed in perforating, and the last four in reducing the stone into small fragments, which were passed off with the urine. No ill effects whatever attended or followed the operation, and the patient is perfectly recovered. The detritus, passed off with the urine, was exhibited to the Academy, (having been collected into small masses by means of a solution of gum,) and excited universal astonishment and admiration. The patient was present at the meeting.

Cure of Fever.—At the same meeting M. Magendie made a very favourable report on the use of powder of holly leaves, recommended by Dr. Rousseau as a cure for fever (vide page 148). The reporter stated that the new remedy had been tried in the hospitals in thirteen different cases of fever. The doses administered were from one to five gros per day, and in every case the patients were cured after about twenty days treatment. The effect of the holly is not so quick as that of the quinia and silicine, but it is a sure and excellent febrifuge. The only thing necessary to make it thoroughly useful, was to extract its essential properties, so as to avoid the necessity of administering it in such large quantities. We have already mentioned that this has been done; and M. Magendie concludes his report by stating that ilicine may now take its place with quinia and silicine in the list of febrifuges.

Cholera at Mecca.—On the 12th of September M. D'Arcet communicated to the Academy a letter which he had received from M. Mimaut, the consul-general of France, in Egypt, containing an account of a contagious disease, which had carried off at least 12,000 pilgrims at Mecca, and was still raging there with great violence. The individuals attacked fell down in the street, without any previous illness, and, after violent vomitings, died almost instantly. This visitation was at first considered to be the plague; but the Imans repelled this idea, on account of the promise of the Prophet, that pestilence should never visit the Holy City. They preferred attributing it partly to the want of soft water, which had existed for some time in Mecca, and partly to the vengeance of the Deity, at his holy house having been so long violated by the infidel drums and trumpets of the regiments in garrison at Mecca: the latter cause has been removed by the colonel of the regiment imposing silence on his band. There appears, however, every reason to believe that the disease is no other than the cholera; and the immense influx of pilgrims from every quarter, including Persia and India, added to the intense heat (31 deg. Reaumur), furnish sufficient causes for the propagation of the epidemic. During the three days devoted to religious ceremonies, previous to the Bairam, the whole body of pilgrims remain agglomerated in a dense mass, and do not move even when the rain descends upon them in torrents and numbers fall dead around them. During these three days the mortality was terrific,
and immediately afterwards, was still more increased by the feast of Mina, at which every Mussulman kills a sheep, the blood and entrails of which are left to rot on the public ways: thirty thousand of these animals were killed in one day, and the putrefaction resulting increased to such a degree the intensity of infection, that Mina was covered with corpses like a field of battle. The governor, Abdenbeg, who would not neglect his religious duties, went to Mina to assist in the sacrifice of sheep, and being attacked with the disease in the night, had ceased to exist before the morning. Annexed to this letter is the procès verbal of the post mortem examination of two of the corpses by European surgeons at Mecca; the symptoms are similar to those observed in cases of cholera elsewhere. This interesting communication was referred to the Cholera Morbus Committee.

**NATURAL PHILOSOPHY.**

**Polarized Light.**—On the 11th July M. Babinet communicated the result of some experiments which he had made relative to the unequal absorption of the two polarized rays of the coloured crystals which have a double refraction. He states that 'all the negative crystals, such as coloured spath, arragonite, tourmaline, &c., allow the ray, which undergoes the extraordinary refraction, to pass in excess. All the positive crystals, on the contrary, such as smoky quartz, the gypsum of Montmartre, &c., transmit the ordinary ray in great abundance.

**Vibration of Sound.**—On the 18th July M. Savart communicated the result of his experiments made with an instrument, invented by himself, for the purpose of ascertaining the greatest and least number of vibrations per second of which a sound may be composed so as to be perceptible to the human ear. He had previously ascertained that, in one extreme, sounds resulting from more than 40,000 simple oscillations per second, may be distinctly perceived; and he now stated, that, in the other, sounds may be produced by his machine, which are not only perceptible, but even intense, although composed of but eight vibrations per second. The lowest limit of perceptible sounds produced without the aid of his machine was thirty-two vibrations per second.

**Conduction of Sound by Water.**—On the 8th of August, a letter was read from M. Cagnard Latour, communicating an experiment which he had made with the instrument called the Syren. It is well known, that if the instrument be set in motion by a column of water of sufficient elevation, a sound resulting from the vibrations of the liquid itself is produced, even when the instrument is completely submerged. M. Latour ascertained, that by plunging himself into the water, and putting the syren in motion by injecting the liquid by
means of a pump held in his hands, the sound increased in intensity the moment his ears were submerged, although his distance from the instrument remained the same, thus proving that the hydraulic vibrations were directly transmitted to the auricular organs with more energy than when transmitted through the medium of the atmosphere. M. Latour also found, that the intensity of the sound did not vary materially in proportion to the depths to which he submerged himself; whence he concludes, that the augmentation of the pressure of the air contained in the ears did not operate on the phenomenon, but that it depended mainly on the immediate hydraulic communication.

Oscillations of the Pendulum in Air.—On the 22d of August, M. Poisson read a memoir on the simultaneous movements of a pendulum and the ambient air. The learned academician, after remarking that M. Bessel was right in asserting that some slight modifications must be made in the great law of nature laid down by Newton, that all molecules attract each other in the direct ratio of their mass, and the inverse ratio of the square of their distance, stated that he had repeated several of the experiments of M. Bessel. He put pendulums composed of substances of unequal weight into a state of oscillation, and ascertained that the variation in the weight occasioned the variation in the number of oscillations. He then, after a great number of experiments, all of which he verified by calculation, satisfied himself that the gradual diminution of the amplitude of the oscillations of the pendulum must be attributed to the pressure of the ambient air.

ZOOLOGY.

Silkworm.—On the 4th of July, M. Duméril read a report on a memoir by M. Lamare Picquot, relative to the Bombyx paphia of Asia. M. Lamare Picquot, while in India in 1829, had found, in the forests of Bengal which line the right bank of the Damoudore, to the west of Calcutta, some silkworm cocoons, the silk of which appeared to be of so excellent a quality, that he was induced to have the insect sought for, and succeeded in procuring several. This silkworm lives in the forests on a kind of wild Badamier (the Terminalia of botanists), which is very common. The Indians, who keep these worms, feed them on the leaves of the ordinary badamier, or with those of the Shamnus jujuba. The insect undergoes its last change on the return of spring, and comes out of the cocoon by a hole which it makes in the extremity. The female soon afterwards lays her eggs, and both male and female shortly die. The eggs come to maturity in about twenty to twenty-nine days, and ordinarily in the month of March. The worm at its birth is about a quarter of an inch long, yellow, with the head black and large. At its full growth, it is from three to four inches long. M. Lamare Picquot differs in
some particulars from Dr. William Roxburgh, who has given an account of this insect in the seventh volume of the Transactions of the Linnaean Society of London, as to the time of its various metamorphoses. The latter states, that the Bombyx paphia constructs its ball in the month of October, and that the perfect insect does not make its appearance until the July in the following year; so that its captivity would last some months. It is, however, certain, that the cocoons brought into France by M. Lamare Picquot, and placed in hothouses, were hatched in the spring. The reporter, however, remarks, that the appellation paphia strictly belongs only to the bombyx represented by Cramer under that name, plate 147 A and B, and 148 A; the species to which the same name has been applied by Linnaeus and Fabricius are so imperfectly described, that it is almost impossible to know what they really are. The Bombyx paphia always comes into the world during the night; its wings are entirely developed in two hours—the distance from tip to tip of those of the female is nearly five inches. The number of males generated at one time is but one-fifth of that of the females, so that one male impregnates several females. The desire of reproduction manifests itself almost immediately after the development of the wings, and is shown by a shrill buzzing noise, well known to the Indians, who attach the females to the branch of a tree by a silken thread tied round one of their legs, and in the morning they invariably find them with the males attached to them. The males are active, and take long flights; but the females are heavy, and fly but little. The colour of the female varies considerably, but that of the male is uniformly of a deep brick-red. The copulation lasts from twelve to nineteen hours, and the number of eggs varies from 500 to 700; they are white, and occupy the greater part of the abdominal cavity. The cocoon is of a very singular construction, and different from all those which are known to us. It is not fixed to the branch of a tree by a glutinous matter or by a silken thread, but the insect chooses a branch which is about half an inch in diameter, and forms a species of ring round it with a resinous matter issuing from its mouth; it then extends its work in a sort of pedicule, of about two inches long, in which it gradually encloses itself. The Indians, to preserve it from the birds, and to prevent the females from leaving the spot, cover the tree with a thick net. The silk which comes from these cocoons has a dark tint, which must be chemically removed before it can be dyed any other colour; it is much coarser than the silk of the common silkworm, but is stronger, and the stuffs made from it last a long time. It is also used to make nets and fishing-lines. M. Lamare Picquot imagines, that some of the trees now grown in France may probably be found to answer as a substitute for the badamier in furnishing food for these insects, and wishes them naturalized in France; but the reporter remarks, that although it would be easy to have the insects from India, it would be absolutely necessary to be provided with a nourishment for them—as otherwise, should M. Lamare Picquot
not be right in his conjecture as to the fitness of some of the French trees for that purpose, they would necessarily perish. In conclusion, the reporter, though bestowing great praise on M. Lamare Picquot's assiduous labour, appears rather to discourage the introduction of the *Bombyx mori*, on the ground, first, of the uncertainty of being able to nourish it; and secondly, that if the silk be useful, it would probably be as economical to get it in a raw state from India.

At the conclusion of this report, M. Chevreuil stated that he had been engaged in endeavouring to bleach the silk in question, but had not yet been able to succeed in doing so; although he had considerably diminished the intensity of the dark colour. He added, that it is undoubtedly chemically different from common silk, and would require to be carefully analysed before it was adopted as an article of commerce, since the chemical tests now used at the Custom-house to ascertain the purity of common silk would not produce the same effect on this. M. Chevreuil concluded by expressing an opinion that this silk is not a simple substance, but a combination of several.

*Apoplexy in Horses.*—On the 15th of August, M. Bouloy presented a memoir to the Academy, from which it appeared that apoplexy of the brain is in men; an observation in perfect and curious accordance with the relative degree of activity of those two organs in the two species of animals.

*Teeth of the Gnawing Mammiferæ.*—On the 11th of July, M. Geoffroy St. Hilaire read a memoir, the object of which was to prove that the front teeth of these mammiferæ, which have been hitherto called incisors, are, in fact, analogous to the canine teeth. For this purpose he entered into the history of the names given to the teeth in different animals. In the human anatomy, teeth were divided into three classes—the incisors, the canine, and the molares or grinders; and the same names were without difficulty applied to the families whose organization most resembled that of man, such as the quadrumanæ and the carnivorous. After these families, however, came animals which are digitated, but have but two sorts of teeth, and this circumstance became the characteristic of one of the great orders of mammiferæ, that of the *rongeurs*. There was not much time spent in inquiring in which class of teeth these animals were deficient. Teeth were observed placed in the front part of the jaw, as incisors are in man. The front teeth of the *rongeurs* were, therefore, immediately called incisors, which name some zoologists afterwards changed to *primores*—a term which had the double advantage of expressing that these teeth are the first which offer themselves to observation, and that they present a characteristic of the first order of zoological importance, inasmuch as their variation is always connected with a great number of others in the organization. The name of incisors was, therefore, given to these front teeth, merely because
that is the name of those teeth which, in the human mouth, present themselves the first in going from front to back. If the inverse course had been adopted, and the calculation made from the hinder part of the jaw, those teeth which, in the rongeurs, come after the molares, would, for the same reason, have been called canine. M. St. Hilaire's object is to ascertain which of the two appellations is intrinsically correct. It is evident that one class of teeth is entirely wanting, and this deficiency may be considered as the result of an atrophy. This atrophy must have existed either at the middle of the jaws, or one of the extremities; and M. Geoffroy conceives that the latter hypothesis is alone admissible, and that the atrophy must have occurred at the point at which the maxillary branch terminates. When the length of jaw gives sufficient room for the full development of the dental nerve, as in the dolphin, the lizard, the crocodile, &c., the dental, arterial, venous, and nervous branches are subdivided into clusters, of similar volume, and equally distributed. Then there are as many conic and symmetrically arranged teeth as there are subdivisions in the parent branches. There is then a regular formation in every point, both anteriorly and posteriorly; and it is of little or no consequence to what zoological class the animal belongs, since it is not the organic difference, but the room which exists for the development and distribution of the vascular and nervous branches, which makes the distinction. Hence we may conclude that there is nothing specially inherent in the nature of the dental operations to occasion the division of the teeth into the three classes of incisors, canine, and molares. The want of one class of teeth in the rongeurs is, therefore, owing to the want of room for development; the development began with the molares, which are, unquestionably, there; then proceeded with the canine teeth; but being there stopped for want of room, ceased, and the incisors are consequently wanting. There is no reason why we should admit the existence of the two extremes of the molares and the incisors, and suppose the absence of the intermediate class, the canine. The front teeth of the mammiferæ rongeurs should, therefore, as a matter of consistency, be called canine, and not incisors.

On the 18th, M. St. Hilaire read another memoir relating to the same subject; the object of which was to correct an error into which, the learned academian stated, that he considered himself to have fallen, in common with M. Cuvier, in 1795; when they published a memoir, in which the existence of the inter-maxillary bone was considered as furnishing a certain criterion for attributing to teeth affixed in it the character of incisors. This opinion M. St. Hilaire now considers erroneous:—1. Because it can only apply to the upper jaw, and, therefore, leaves the teeth of the lower jaw subject to an arbitrary classification; and, 2. because, so far from the inter-maxillary bone being merely intended to support the incisors, that bone exists in a great number of animals which have no teeth at all in the an-
terior part of the mouth. The inter-maxillary bone is principally in relation with the organs of taste and smelling; and, like most of the cranium bones, its principal purpose is to furnish partitions and assist in forming the cavities in which the organs of the senses are placed. It is true that these bones also furnish sockets for the teeth, but that is quite a secondary function. The teeth, stony and crystallizable substances, have a structure and system of formation which render them wholly unconnected with the structure and form of the osseous tissue. Deposited at first on the maxillary arcades, they do, it is true, hollow out a socket for themselves there; but this intercalation is wholly determined by the accident of proximity, and is not produced by any marked predilection for a particular bone. In those animals which have long jaws, the teeth, not meeting with any obstacle to their development, are regular in form and position all along the bone. This could not be the case in man, because the extreme development of the encephalos rendered a corresponding reduction in the face necessary. As, however, the nerves and vessels which pass through the jaws are not less numerous, and each of these bundles must terminate in a tooth, it follows that the number of the dental germs is not less considerable, but their arrangement is less regular. In the parts nearest the origin, these bundles are formed into groups of four, whence result the teeth with four fangs; further on they are only two and two, and the teeth have two fangs; while it is only towards the extremity that the germs are developed in an isolated manner and produce single teeth. It is only in those mammiferæ which have the cerebrum large and the face short, that we find those teeth with several fangs, which must be considered as being produced by dental germs, heaped, and, as it were, soldered together. These explanations M. St. Hilaire considers as strongly confirming his theory respecting the teeth of the mammiferæ ron-geurs, as above developed.

Snail's Eggs.—On the 15th of August, a letter was read from M. Turpin, containing some particulars of the microscopic analysis of the egg of the garden snail (*Helix hortensis*). When the exterior of this egg is viewed through a strong magnifying glass, the shining surface presents an infinity of white points, which appear, as it were, drowned in the soft mucous and transparent envelope of the egg. When an egg is crushed between two plates of glass, all the viscous and albuminous liquid which it contains is scattered abroad, and the torn membrane remains empty. If the whole be then viewed through a microscope having a two-hundred times magnifying power, a prodigious quantity of very beautiful pointed white and translucid rhomboidal crystals, regularly formed in their angles and sides, are clearly distinguished. These rhomboids are of unequal dimensions, the largest being about 100th part of a millimetre (.0004 of an inch). Some are single, and others grouped together by two, three, four, five, and six; all are fixed or glued against the interior surface of the envelope of
the egg. Among these rhomboids are found a few cubes, and some regular prisms with square bases. The number of these crystals is so considerable, that they may be considered as forming at least half the volume of the egg. The white external points above mentioned indicate the crystals which line the interior surface of the envelope. After these crystals have remained six days between glass plates, their form gradually alters, their angles become rounded, their beautiful white changes to a yellow hue, and they are in a great degree liquefied. These crystals were observed by M. Turpin in an egg laid by a snail on his table, and instantly examined with the microscope; whence he considers it probable that they are formed in the egg while it is yet in the ovary of the mother, in the same manner as similar crystallizations are formed in the cellular tissues of plants, particularly those of the genus Cactus. MM. Chevreuil and Cordier were requested to verify these remarks, and also to examine whether these crystallizations are peculiar to the garden snail, or whether they are common to every species of the genus Helix, and to the eggs of all the molluscs or molocogoanes; and also whether similar crystallizations could be obtained by submitting the albuminous liquid of the eggs of birds, reptiles, and fishes, to the action of electricity. We shall, of course, communicate the result of their investigations.

MISCELLANEOUS.

Human Nutrition.—On the 11th July a letter was read from M. Roulin, a young physician of eminent attainments, both medical and scientific, in which he vindicated the nutritive properties of gelatine, and pointed out the absolute necessity of salt entering into the regimen of animals upon whom the effect of different alimentary substances was to be tried. As a proof of the manner in which animal strength may be supported, he related the remarkable fact, that in travelling through some forests in Columbia, in 1825, he and his guides, being entirely without provisions, were compelled to eat five pair of sandals (made of untanned leather, softened by the dampness of the forests) and a deer-skin apron, which they roasted and masticated. In the latter operation, two hours were occupied in getting through the third part of the sole of a sandal. This singular aliment supported their strength; and though the journey, which was to have lasted only two days, occupied fourteen, they arrived at its termination in good health. They occasionally ate the core of the palm-trees, but found that it sustained their strength much less than a piece of the roasted leather.

New Compressing Pump.—On the 18th July a letter was read from M. Thilorier, announcing that M. Perrot had recently applied his new system of compressing gases to an engine of war, by means of which it throws 200 balls per minute. This machine principally differs from that of Perkins by the use of the elastic force of air instead of that of steam, by which a considerable saving of expense
is obtained. In the ordinary pumps, the force required to overcome the resistance of the piston is as the number of atmospheres; whereas in M. Thilorier's new pump, the force required is only as the square root or cube root of the atmospheres, according as the gas has been submitted to two or three successive compressions. Hence by the new pump a single man may, in a given time, perform as much work as a steam-engine of thirty-horse power applied to the old pump.

Climate of Asia.—On the 18th of July, M. de Humboldt communicated to the Academy some very curious observations on the relation subsisting between the temperature of the soil and the phenomenon of the preservation of the soft parts of antediluvian animals. The first basis of climatology is the precise knowledge of the inequalities of the surface of a continent. Without this knowledge, we should attribute to the elevation of the soil what is, in fact, the effect of other causes exercising their influence on the low regions (in a surface which has the same inflexion as the surface of the ocean) upon the inflexion of the isothermal lines. In advancing from the north-east of Europe to the north of Asia, beyond the forty-sixth or fiftieth degree of latitude, we find at once a diminution in the mean temperature of the year, and a more unequal distribution of this temperature among the different seasons. Europe, with its sinuous shape, is but a peninsular prolongation of Asia, as Brittany (renowned for its mild winters and unoppressive summers) is of the rest of France. The predominant winds received by Europe are the west winds, which to the western and central parts are sea breezes, that is to say, currents which have been in contact with a mass of water, the temperature of which, at the surface between 45 and 50 degrees of latitude, is never, even in January, below 9° Centigrade. Europe enjoys the influence of the large terrestrial tropical zone of Africa and Arabia, which becomes heated by the solar irradiation in a far different manner from that which would be the case with a surface of water similarly situated, and which, by means of the ascending currents, pours out masses of hot air on the countries situated more to the north. The small and unequal development of Europe towards the north, and its oblique direction from south-west to north-east, are advantages which have not hitherto been sufficiently appreciated in considering it with respect to its general configuration, and as a western prolongation of Asia. Being thus placed opposite to the gulf which the warm waters of the gulf-stream open in the polar ices, its coasts are (at least in the two-thirds which are western, that is, the part properly peninsular) bathed by a free sea; for, in the one-third which is eastern, where it widens in joining Asia, it partakes of the character of the climate of that continent. The continent of Asia extends, from east to west, beyond the parallel of 70 degrees, over a space thirteen times as long as Europe. Its northern coasts, throughout, touch not only the winter boundary of
the polar ices, but, except in a few points, and during a very short period of the year, their summer limits also. The north winds, the force of which in the open plains is not moderated by any chain of mountains to the west of the meridian of the lake Baikal as far as the 52nd degree of latitude, and to the west of the meridian of Bolor, as far as the 40th degree, pass over a field of ice covered with snow, which prolongs, as it were, the continent even to the pole; on the other side, Asia offers to the influence of the solar irradiation but a very small portion of country situated under the torrid zone between the meridians which bound its eastern and western extremities. The equator passes only through a few islands, Sumatra, Borneo, Celebes, and Gilololo; during the whole remainder of its vast extent, the equinoctial line cuts only the ocean: whence it results that the continental part of Asia under the temperate zone does not enjoy the effect of ascending currents similar to those which the position of Africa renders so advantageous to Europe. There are also other causes which tend to increase the frigidity of Asia; these are—1st, Its position with respect to Europe, which gives the latter all the western coasts, always under the temperate zones, much warmer than the eastern ones; 2nd, The form of its outlines, which, to the north of the parallel of 35°, present neither gulfs nor peninsular prolongations of any consequence; 3d, The form of its surface, which, in one part, chains of mountains intercepting the approach of the south winds over a great extent of country, and in another, a series of high platforms lying in a direction from south-west to north-east, which, accumulating and preserving snow even in the midst of summer, act, by means of descending currents, on the countries which they bound or traverse, and thus lower their temperature. These contrasts between Europe and Asia present a summary of the causes which act simultaneously on the inflexions of the isothermal lines between the different seasons, and which are particularly perceptible to the east of the meridian of Petersburgh, where the continent of Europe joins Northern Asia in a width of 20 degrees of latitude. The east of Europe and the whole of Asia, to the north of the parallel of 35 degrees, have a climate eminently continental, as distinguished from the climate of the isles and the western coasts; they have, both from their form and their position with respect to the west and south-west winds, a climate of excess analogous to that of the United States of America, that is to say, very hot summers succeeding very severe winters. At Astracan M. de Humboldt has seen grapes as fine and as ripe as in Italy or the Canaries; although in the same spot, and even much more to the south, at Kislar, which is in the same latitude as Avignon, the thermometer (Centigrade) often descends in winter 28 and 30 degrees below zero. A more profound knowledge of the laws regulating the temperature of the earth in Asia, may produce a modification of the ideas entertained respecting the circumstances which have attended the last terrestrial revolutions. Thus, when it was known that the bones of
animals, the analogous species of which now exist only in the tropical regions, are found still covered with the flesh in the diluvium of the plains in the north of Siberia, at the mouth of the Lena, and on the banks of the Velhoui, between 72 and 64 degrees of north latitude, it was immediately supposed that a sudden refrigeration of the temperature had, at some period, been operated in those countries; but this phenomenon appears now susceptible of being more easily explained by the cold which, as M. de Humboldt has ascertained recently on the spot, exists in the earth, even in the midst of summer, at a depth of five or six feet. When at noon, in the months of July and August, the air had a temperature of from 25 to 30 degrees, M. de Humboldt found, between 54 and 58 degrees of latitude, four wells of small depth, which had not the slightest remains of ice on their borders, but the temperature of which varied from 2° 6' to 1° 4' above zero. M. Erman found, on the road from Tobolsk to Jakoutsk, in the latitude of 56°, springs at a temperature of 0° 7'' and 3° 8' above zero, when the atmosphere was at 24°; but beyond the parallel of 62° in the steppes, and even in the parallel of 60° in places not very elevated, the soil remains frozen at a depth of from twelve to fifteen feet. At Bogoslowsk, in the middle of summer, M. de Humboldt found, at a depth of six feet, in a turfy soil, but slightly shaded by trees, a bed of congealed earth 9½ feet thick, traversed by small fillets of ice, and containing groups of crystal of solid water, like a porphyritic rock. At Jakoutsk (latitude 62°) the subterranean ice is a general and perpetual phenomenon, notwithstanding the high temperature of the atmosphere in July and August; and it may easily be conceived, that from this parallel to that of the mouth of the Lena, 72° N. latitude, the thickness of this bed of congealed earth must rapidly augment.

These facts being established, it may also be remarked, that tropical animals, tigers precisely similar to those of India, are still seen in Siberia. Several tigers, of an enormous size, have been killed near the celebrated silver mine of Schlangenberg. Other animals, which we now consider as peculiar to the torrid zone, have, doubtless, as well as the bamboos, the ferns, the palm trees, and the coral lithophyton, existed in the north of the ancient continent. This was, probably, under the influence of the internal heat of the earth, which in the most northern regions communicated with the atmospheric air through the crevices of the oxydized crust. As the atmosphere became chilled by the interruption of this communication, when the crevices were successively obstructed by interposed rocks, or other solid matters, the distribution of climate gradually became almost entirely dependent on the solar irradiation, and the animal and vegetable tribes, whose organization required an equal temperature of a more elevated degree, became gradually extinct. Some of the most hardy among the animals doubtless retired towards the south, and lived some time longer in regions nearer to the tropics; others, such as the lions of ancient Greece, the royal tiger of Dzungaria, the panthera
iri and of Siberia, were enabled, by their organization and the effects of habit, to naturalize themselves in the climate of the centre of the temperate zone; some species even were enabled to inhabit the regions still more to the north, as M. Cuvier supposes was the case with the thick-haired *pachydermis*. Now if, during a Siberian summer, one of the last revolutions of the globe destroyed those elephants and rhinoceroses whose species is now lost, and which may be supposed to have been wandering at that season of the year towards the banks of the Velhoui and the mouth of the Lena, their bodies would find there, at the depth of a few feet, thick beds of congealed earth capable of preserving them from putrefaction. Slight convulsions, crevices of the soil, much less than those which we have seen in our days on the plain of Quito and the Indian Archipelago, would be sufficient to effect this embedding and preservation of the soft parts of those animals. The supposition of a sudden refrigeration appears, therefore, wholly unnecessary. It must not be forgotten, that the tiger, which we are in the habit of calling an animal of the torrid zone, now exists in Asia, from the extremity of Hindostan to Tarbagataï, the upper Irtychi, and the steppes of the Kirghises—an extent of forty degrees of latitude; and even sometimes in summer makes excursions one hundred leagues further to the north. Individuals of this species arriving in the north-east of Siberia, as far as the parallels of from 62° to 65°, might, by the effect of convulsion or crumbling of the earth, or other circumstances by no means very extraordinary, offer, in the present state of the Asiatic climates, phenomena of preservation very similar to those of the mammoth of Mr. Adams, and the rhinoceros of the Velhoui.

**Preservation from Shipwreck.**—On the 22d of August, M. Monnin presented a memoir on this subject. He proposes to fix round the vessels in stormy weather, large bladders made of the hides of oxen or horses, and filled with air, which would sustain the vessel and prevent its sinking, even when filled with water. He also proposes to diminish the dangers arising from vessels striking against rocks, by placing impermeable mattresses of hair or old linen between the coppering and the wood of the vessel.

**Atmospheric Phenomena.**—At the same meeting a letter was read from M. Jean Dufour, communicating a phenomenon observed at St. Serir (Candu). On the 20th instant, about five o'clock in the afternoon, the sun appeared round and white like a moon; that is to say, it emitted no apparent rays, and could be steadfastly regarded without dazzling or in any manner affecting the eyes. An hour afterwards it appeared of a pale blue colour, but still destitute of rays; and the horizon, at its setting, was of a deep red, such as is frequently observed after a very hot day. A kind of mist, at a considerable distance from the earth, and of trifling density, was uniformly spread in the upper regions of the atmosphere, and veiled the
sun. The thermometer marked 25 (Reaumur), and the barometer 27 p. 4 l. The wind was easterly and the weather calm; the heat had not the stifling character peculiar to stormy weather, nor was there any sign of thunder. During the day the objects exposed to the direct rays of the sun had been observed to assume a bluish tint. M. Arago remarked, that from letters which he had received from Perpignan and Bordeaux, it appears that the same atmospheric phenomena were observed throughout the south of France. At subsequent meetings various letters were received, which proved that the same phenomenon was visible in Italy, and several other parts of Europe. A similar phenomenon was mentioned by M. Roulin, as having occurred a few years since in South America. After the conversation on this subject had terminated, M. Géoffroy St. Hilaire read a letter which he had received from M. Lambert, of Cullomiers, stating that at five o'clock in the morning of the 16th instant, the servant of M. Fimmerman, of the Château de Moral, saw a considerable volume of flame, unaccompanied by smoke, issue from the ground at the foot of an old and large pear-tree. The same phenomenon was witnessed on the same spot a few days afterwards by another servant, and by M. Fimmerman himself. The spot from which the flame issued, presented the appearance of a hole similar to that occasioned by the passage of gas which has forced its way after fire has exhausted a portion of inflammable matter. The château is situated at the base of a long chain of mountains, the height of which is about one hundred and fifty toises.

New Work by M. de Humboldt.—On the 12th of September, M. de Humboldt presented a new work to the Academy, entitled ' Fragmens Asiatiques.' The following is an analysis of its contents:—General sketch of his voyage in Central Asia. Notice on the discovery of diamonds on the western declivity of the Oural. Account of the quantity of gold and platina obtained from the Oural, between 1814 and 1830, presenting a total of 24,000 kilogrammes; and an average, during the later years, of 15,800 kilogrammes of gold, and 1700 of platina, mixed with osmium and iridium, per annum. Several routes in Central Asia (from the southern frontier of Siberia, to Kachkar, Yorkend, Ak-Sou, and Eachmis), collected at Semipotatensk, on the borders of Chinese Dyongaire, from a comparison of the accounts of various native Asiatic travellers. A notice of the astronomical position of several places in the south of Siberia, and of the position of the Chinese post of Rhonimalokhou, to the north of Lake Dyayzan. A series of observations of magnetic inclinations (the mean inclination of two needles) made during the journey. Considerations on the mountainous systems of Central Asia; the great depression of the soil around the Caspian Sea and the Lake Aral, determined by barometrical observations; situation of volcanoes which have emitted streams of lava at a distance of between three hundred and four hundred leagues from the sea. Notice of the fires
and saltworks of Bakau, recently visited by M. de Lenz. Geographical additions by M. Klapproth, on the limits of the Atlas, after the Chinese authors, and of the volcanic phenomena of Central Asia. Artesian and fire wells of the Chinese, at a depth of one thousand eight hundred feet, (perforated in a manner not yet used in Europe, by raising a beam or rammer with a cord, used by the Chinese from the most remote periods,) of hydrogen gas, both portable and brought by pipes from great distances for lighting, and for the evaporation of salt-waters. Ancient use in China of combustible bricks made of pounded pit-coal. Summary of volcanic phenomena considered in the most general point of view as the effect of the action of the interior fluid of a planet on its solid and oxydized exterior crust. Remarks on the progress of radiation at the surface, and on the interruption of the communications with the interior, which advance a state in which the relation of position with a central body (the sun) alone determines the difference in climate. Considerations on the temperature and the hygrometrical state of the atmosphere of the north of Asia. Effect of the subterranean ices on the preservation of the soft parts of animals, and remarks on the inutility of the geological hypothesis of an instantaneous refrigeration. General reflections on the causes of the inflections of the isothermic lines upon the numerical data of the distribution of heat on the surface of the soil, in the sea and in the air; upon a mode of arranging the mean results and examining the disturbing causes which are at first insulated, but afterwards accumulated on each other in such a manner as to disclose empirical laws.

Statistics of Human Generation.—On the 29th of September, M. Mathieu read a report on a memoir by M. Giroux de Busaringues, containing a statistical account of the marriages, and the births of infants of both sexes, in France, classed in months. M. Giroux imagines the reproduction of man to be subjected to the same laws as that of domestic animals, and that whatever tends to increase the motive power of the man, or to diminish that of the woman, promotes the procreation of male children, and vice versa; so that a man may render himself more or less apt to procreate boys or girls, according as he addicts himself to exercises productive of muscular force, or to slothfulness, to sobriety, or to intemperance. M. Giroux's facts are drawn from the official returns of every part of France, for ten years, commencing from 1817. He finds that, with respect to the number of marriages, the months are thus to be classed,—February, January, November, June, May, July, October, April, September, December, August, and March. This depends on the periods of religious festivals, and on those of rural labours, marriages being rare during those periods and most numerous in the months immediately preceding them. Thus March, the month in which Lent falls, is last on the list; while the preceding month, February, is first. The births of legitimate children are thus distributed,—February,
March, January, April, November, September, December, October, May, August, June, July: counting backwards for nine months, we have the following order as that of the conceptions,—May, June, April, July, February, December, March, January, August, November, September, October. It will be observed, that the months in which the greatest number of conceptions take place are not those in which there are the greatest number of marriages, which proves that conception rarely takes place in the first month of marriage; on an average it attains its maximum about two or three months afterwards. The greatest number of conceptions are in spring and summer, and the smallest number in autumn and winter; but M. Giroux imagines that this is not so much owing to the direct influence of the seasons, as to the fact of each season bringing periodically the recurrence of labours and habits more or less favourable to the procreative power. Thus it appears that the greatest number of conceptions are in those months in which the strength of man is developed by exercise and moderate labour; and the smallest number at the period of the dispersion and emigration of the rural population, and of the recurrence of more fatiguing labour. The greatest number of boys are born in January and June, the greatest number of girls in December and July; whence the maximum of the conception of the former is in April and September, and of the latter in March and October. M. Giroux is of opinion that the female sex predominates in first conceptions. The greatest number of natural children are born in the months of February, March, January, and April, and are, consequently, conceived in the months of May, June, April, and July; the same as has been observed of legitimate children. But the smallest number of births of natural children is in the months of July, August, September, and October; and, consequently, the smallest number of conceptions is in the months of October, November, December, and January, being two months further advanced in the cold season than the minimum of conception of legitimate children. The greatest number of boys among natural children are born in January and July, and of girls in November and December; consequently, the most numerous conceptions of the former are in April and October, and of the latter in February and March. M. Giroux has confirmed these general calculations, by applying them to each department separately, and finds the results nearly similar. The reporter concluded by recommending M. Giroux to persevere in his researches after new facts to verify and confirm the results at which he has arrived.

Wooden Houses.—On the 19th September, M. Navier made a report on a memoir by M. Blom, a colonel of engineers in Sweden, relative to moveable wooden houses constructed by him. A report on a former memoir of M. Blom had been made last year by MM. Navier, de Prony, and Girard, in which it was suggested that the invention, although well adapted for cold countries, would be found
open to objection in warmer climates. The present memoir of M. Blom has for its object the removal of this difficulty. He remarks that, with respect to the variation of temperature and the hygrometric state of the atmospheric air, it is hardly probable that the houses would ever be exposed to greater variations than in Sweden, where the thermometer of Reaumur in July generally marks 20°, and sometimes 24°, above zero, and falls in winter to the same number of degrees below zero. In the latter case, the temperature of the interior of the houses is preserved, by stoves, at from 12° to 19° above zero. M. Blom points out particularly the advantage derived from the construction of the wooden walls, both on account of their small thickness and their small conducting power: thus, in thaws, every part of the edifice promptly assumes the highest temperature of the atmosphere, and there is not that precipitation of water which is always observed on the surface of stone walls, and which produces a very injurious humidity. It is well known that the Swedish ships remain a long time in hot countries without being at all injured by the heat of the sun, although the surfaces of the decks are not covered with coating; and it appears that the moveable houses of M. Blom have been used for four years in the Swedish colony of St. Bartholomew, and no complaints have been made of their having sustained injury either from the heat or the hurricanes which prevail there. These houses are warmed by portable stoves, also invented by M. Blom, and they are insured against fire at the ordinary premium, the offices having been satisfied that, while they are not more liable to conflagration than ordinary houses built of the same kind of wood, they have the advantage of being easily removed out of the reach of danger. Every part of these houses is moveable, so that, with very little expense, the form and position of the rooms may be changed at pleasure. In countries subject to earthquakes, these edifices are particularly desirable, as, independently of the facility of removal, they are much less likely to be destroyed or overthrown than constructions in stone. But it is in new colonies that their advantage would be most sensibly felt. Wherever it may appear to the settler desirable to fix his residence, his habitation is ready, and may be removed elsewhere when circumstances may render a change desirable. Large public edifices, such as hospitals, barracks, prisons, &c., may in like manner be easily transported wherever a change or increase in the territory may render it desirable to remove the seat of government. As far as the mode of construction can be understood from the drawings appended to M. Blom’s memoir, it appears that the walls are formed of thick planks, two together, and united at their joints by keys. The planks in the outer row, which are the thickest, are placed upright or vertically; those in the inner row are placed horizontally. Between the two rows is interposed a species of pasteboard, impregnated with bituminous substances. The joints of the principal pieces, which form the angles of the roofs and floors, are secured by buttons; the angles of the prime Image.
panels which form the walls and partitions are united and secured in the same manner. The author states that he intends to send a house to France; and it is very desirable that he should do so, as it is impossible to form an accurate idea of the details of the construction from drawings. It appears that success in constructing these houses depends in a great measure on the excellence of the Swedish wood, as well as on the care taken in selecting and preparing the wood proper for the different parts of the edifice, and the nicety and precision of the work; other architects may, therefore, at first find considerable difficulty in constructing them. It is unquestionable that the mode of building employed by M. Blom is immeasurably superior to that hitherto in use in Sweden and Russia. In conclusion, the reporter strongly recommended M. Blom to the approbation of the Academy, as having invented and brought to perfection a new and useful branch of industry, which it would be desirable to have known in France.

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—MECHANICAL SCIENCE.

1. ON THE FLEXURE AND FORCE OF CERTAIN WOODS.

Experiments have been made by Mr. Brown, at Newport (Rhode Island), U.S., on the resistance to flexure belonging to three fir woods,—*pin du lorr* (*Pinus strobus*), spruce fir (*Abies nigr*e), and southern fir (*Pinus australis, Pinus longifolia*): these were as the numbers 120, 132, 193. The third kind, therefore, deserves the preference and choice of those planters who, especially in large plantations, prefer real utility to beauty. The *pin du lorr* is considered worthy of its place in parks; and, for the improvement of forests, the southern fir may be mingled with the other fir-trees; but whether it will be more valuable than the Scotch fir (*Pinus sylvestris*) or the Corsican fir, for size, straightness, and strength, can only be ascertained by similar experiments to those above, and observations upon its growth. It may probably be a very valuable addition from America to the firs already known in Europe*.  

2. DOUBLE IMAGES OF OBJECTS SEEN THROUGH THE AIR.

M. Rozet has frequently observed double images of objects seen through the air; he compares them to the images formed by doubly refracting spar, and goes so far as to say that the atmosphere has the property of

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occasionally giving two images, nearly as Iceland spar. During his residence in Africa, the same phenomenon was presented in a very remarkable manner at various times, particularly at the camp of Staonelli, on the 27th June, 1830. At 10 o'clock, A.M., the sky was very clear, and the thermometer at 21° R. (60° F.) On looking at the line of battle, formed before the camp, there were distinctly two images. The extraordinary image was not so strong as the other, but yet perfectly distinct from it; it was raised about a fourth of the height of the objects (query, men?) and deviated very slightly in a lateral direction. The same effect occurred with isolated men. Many Algerine tents in the hands of the French had on their summits spheres of tinned iron, surmounted by a crescent. On all these spheres was seen a second, tangential to the principal one; so that it seemed at first as if there were two.

Whether the two images were repetitions of each other in the same direction, as is the case with Iceland spar; or whether there was an inversion of any part of the images, as happens in all ordinary atmospheric refractions, M. Rozet has not mentioned, although the distinction is a very important one to the analogy referred to between the action of the air and doubly refracting bodies. The observations were read to the Royal Academy of Sciences at Paris, on the 20th June*.

§ II.—CHEMICAL SCIENCE.

1. On the Rapid Production of Steam by Heated Metals.

Some highly interesting and practical experiments have been made by Professor Johnson, of Philadelphia, on the quantity of steam evolved, and time required by heated metals. There is every reason to believe that explosions have often happened, especially on board vessels, by the water being either splashed or returning over parts of the boiler which have been highly heated; and in the arrangement of the boilers in the American steam-boats this is especially likely to be the case. Hence it becomes important to know what power of suddenly raising steam from boiling water such heated iron would possess; and this was done, in the experiments, by plunging the metal into a certain portion of weighed water at 212°, contained in a vessel, itself guarded by a coat of green-baize, cotton, &c., so as to prevent loss of heat, and attached to a scale-beam. The vessel could hold 28 1/2 lbs. of water at 60°; when 14 lbs. of boiling water were put in, it required 14 hours for the temperature to sink to 115°, the temperature of the place being 80°.

When used, 15 lbs. of water were put in the vessel suspended to

* Revue Encyc., i. p. 613.
the scale-beam, and the water and vessel raised to 212° by heaters. On making the experiments, the hot metal was introduced either at once or more gradually, covered over with a perforated cover, and the metal always withdrawn upon the cessation of ebullition; the loss of weight was then ascertained. As it was difficult to ascertain temperatures above the boiling point of mercury, a barely red heat in daylight was chosen as a standard of comparison between the different metals and different masses of the same metal; and as it is probable that the heated parts of boilers are seldom raised above a dull red heat, it was thought that, for practical as well as theoretical purposes, that point would be most interesting and important. The experiments to determine the period of greatest activity show that, just below the point of visible redness in daylight, the greatest quantity of steam is generated in a given number of instants; such, at least, is the case when the experiment is performed under ordinary atmospheric pressure, and this point, therefore, has been termed the comparable temperature.

The following is a table of experiments with rolled iron boiler-plate, 25 1/2 inches long by 7 1/2 broad and 7/10ths thick, exposing 395 square inches of surface, and rolled into an open coil. The water was at 212°, the barometer at 29.9 inches: the room was from 80° to 85°.

<table>
<thead>
<tr>
<th>Weight of Metal in oz. avoird.</th>
<th>Time in Seconds</th>
<th>Oz. avoird. ft. Steam per oz. avoird. Metal</th>
<th>Oz. Metal for one oz. of Steam</th>
<th>Observed Heat in Daylight</th>
</tr>
</thead>
<tbody>
<tr>
<td>144</td>
<td>40</td>
<td>10.75</td>
<td>.0746</td>
<td>13.395</td>
</tr>
<tr>
<td>144.25</td>
<td>90</td>
<td>16</td>
<td>.1169</td>
<td>9.016</td>
</tr>
<tr>
<td>144.25</td>
<td>90</td>
<td>16</td>
<td>.1109</td>
<td>9.016</td>
</tr>
<tr>
<td>144.125</td>
<td>90</td>
<td>16</td>
<td>.1110</td>
<td>9.008</td>
</tr>
<tr>
<td>144.125</td>
<td>90</td>
<td>16</td>
<td>.1110</td>
<td>9.008</td>
</tr>
<tr>
<td>144</td>
<td>70</td>
<td>16.5</td>
<td>.1145</td>
<td>8.727</td>
</tr>
<tr>
<td>144</td>
<td>150</td>
<td>19.75</td>
<td>.1371</td>
<td>7.291</td>
</tr>
<tr>
<td>144</td>
<td>120</td>
<td>20</td>
<td>.1386</td>
<td>7.2125</td>
</tr>
<tr>
<td>144</td>
<td>90</td>
<td>21</td>
<td>.1458</td>
<td>6.857</td>
</tr>
<tr>
<td>144</td>
<td>90</td>
<td>22.5</td>
<td>.1562</td>
<td>6.400</td>
</tr>
</tbody>
</table>

The coincidence of the second, third, fourth, and fifth experiments is remarkable, and proves that, at the temperature of comparison, 9 lbs. of wrought-iron will generate 1 lb. of steam under atmospheric pressure. In after experiments it was found that, but for the caution taken to avoid waste and error, this effect might have been produced in 25 or 30 seconds instead of the times noted.

A second series of experiments were then made with wrought-iron cylinders 6 inches long, 1.7 in diameter, having a surface of 38 square inches, including the hook; the water being, as before, at 212°.
Experiments five, six, seven, eight, and nine correspond closely with two, three, four, and five of the last table, and show that, at the comparative temperature, iron in this form, as well as in plate, produces 1 lb. of steam for 9 lbs. of metal.

By a third series of experiments, with cast-iron, it was found that, at the comparative temperature, it could generate more steam than wrought-iron,—8 1/2 lbs. nearly being sufficient to produce 1 lb. of steam. This effect is, perhaps, attributable to a difference in the specific heat of the two substances, which may well exist, considering the difference of their composition*

2. Discharge of Lightning over a Large Surface.

The following interesting electrical account is given by Mr. Bryant, clerk in the State Prison at Charles Town, Massachusetts: 'Yesterday,' July 30, 1829, 'we had a severe shock of lightning at the prison. It rained in torrents, and a dense mass of highly-charged clouds spent their embosomed electricity on and about us. I was looking out of my office window to discover the direction in which the clouds were moving, when a flash, accompanied by a rustling noise like that of small shot thrown upon stiff paper, and a feeling as if all the energy of my muscles was at once withdrawn, and an almost insuperable inclination to fall back on the floor, convinced me that I had been struck with lightning. But I only tottered back a few steps, and recovered myself immediately. On leaving my office to inquire what mischief had been done, I learned of the officers that almost all of them, as well as many of the convicts, had been affected like myself. My office is in the brick building directly south, and in front of the prison, about three hundred and sixty yards from the north wall of the prison yard. Between the office and the prison building is a large yard, perhaps one hun-

* Silliman's Journal, xix. 292.
dred and fifty feet wide, the length enclosed by the wall is four
hundred and eighty feet, the width three hundred and sixty feet.
The prison has three conductors on it, about equidistant from each
other, say eighteen feet. The lightning passed down each of these
without any injury to the building, except starting a few slates near
the ridge post. Now what appears singular in this case is, that no
person out of nearly three hundred officers and convicts was in the least
injured, although almost every one was more or less affected by it.
Nearly all of these persons had either a steeled hammer, a musket
with bayonet fixed, or some metallic utensil in their hands. Within
a yard of my situation is an armory with thirty guns and as many
steel-pointed pikes, the points and bayonets pointing up. I can
account for our escape only by supposing that the fluid was attracted
by so many different objects on all parts of the building, and all
over the yard, that it divided itself just before it reached us, and
passed off in such small quantities as almost to lose its effects. It
is singular that men standing five hundred feet distant from me
should be affected in the same degree. I suppose that one hundred
tons of iron are exposed on the different buildings in grates, doors,
pillars, &c. &c. One of the officers had a saw in his hand, which,
he says, seemed to be "light red fire." Another was stooping and
picking up nails from the floor, and the instant after the flash found
himself standing bolt upright, with his hands tightly clenched to-
gether. The effects of this shock were felt over a surface of one
hundred and seventy-two thousand five hundred feet in nearly the
same degree, without any permanent injury being sustained*.

and Pressure.

M. Becquerel has lately examined the changes which take place in
the electric state of bodies by the action of heat, contact, pressure, &c.
of which the following is an abstract:—It has been long known that
if a body, which receives positive electricity by friction with another,
be raised to a high temperature, it gradually loses this power, and at
a certain temperature receives negative electricity.

Iceland spar, which is positive when rubbed with any substance,
becomes negative when its temperature is sufficiently raised. The
author expected to find the same property in different metals re-
garded as electro-positive and electro-negative. He expected that
caloric, by separating the atoms of metals, would produce similar
effects with cleavage in crystallized bodies. When the plates of mica,
or sulphate of lime, are suddenly separated by cleavage, the parts
thus separated are found to be in opposite electric states. If the
parts thus separated be again pressed together and suddenly sepa-
rated, they are again found in the same electric states, and the
difference in those states is more marked as the temperature is

* Silliman's Journal, xvii. 193.
raised. By means of a torsion-balance, and a press for compressing bodies, the author has arrived at some curious results. He found that the excess of electricity acquired by each body was proportional to the degree of pressure to which it was submitted. If, for example, a thin plate of mica and a thin disc of cork be pressed together with a certain force, and then separated, and the electric tension ascertained, and if the experiment be repeated with a double pressure, the electric tension will also be double. The author found, by submitting the discs, first to a great pressure, and without separating them, to a smaller, that the effects of the great pressure remained for some time; and when the discs were separated, they were found to possess a higher electric tension than that which belonged to the least pressure. These remarks only apply to bad conductors of electricity.

The author has demonstrated that heat has no effect on free electricity, but has a considerable effect on the neutral fluid naturally belonging to metallic bodies. If one end of a metallic rod is heated, that extremity manifests positive electricity, whilst the other end exhibits the negative state. The author compares the series of decompositions and recompositions of the two elements of the neutral fluid along the metallic rod, to the transmission of heat from one molecule to another by conduction. He then shows how the electricity thus developed by heat may be removed, and the opposite state rendered evident. Having rolled the end of a platina wire in a spiral form, and placed it on the cap of a gold cup electrometer, (metallic contact being prevented,) he applied the flame of a spirit lamp to the spiral which projected over the cap of the instrument, till it was raised to a red heat. The lamp being removed, and the spiral touched with a piece of moist paper on a heated rod of glass, the positive electricity was removed, and the gold leaves diverged with negative electricity. M. Dessaignes discovered long ago, that if the end of a plate of silver be heated, and the two ends brought in contact with the nerves and muscles of a frog, that contractions were produced. Mr. Ritchie has shown, in the Philosophical Transactions, that a powerful electro-magnetic effect could be produced by two pieces of the same metal, having one of the ends raised to a high temperature; but this experiment of the author is the only one in which electric tension, capable of causing divergence in conductors by heat alone, has been observed. The author tried experiments with the more oxidable metals, and found them to possess the same property, though in a less degree, and that in bismuth, tin, and antimony, the effects were scarcely sensible. The second part of this memoir contains theoretical views of the electric and chemical theory, which the curious in such matters can examine in the original.

4. CRYSTALLIZATION OF PERCHLORIC ACID. (Serullas.)

Solution of perchloric acid was concentrated by evaporation until
it evolved white vapours in some abundance; it was then mixed with four or five times its volume of concentrated sulphuric acid, and distilled in a small retort, to which a receiver had been adapted. The mixture became coloured, and when boiling, evolved chlorine and oxygen; but a small quantity of the perchloric acid escaped decomposition, rose in vapours, and condensed in the receiver, which was purposely cooled. Thus distilled, this acid is solid, it contains no sulphuric acid; exposed to air, it rapidly attracts water, and at the same time evolves dense white fumes. When melted and poured into water, each drop hisses like hot iron. It melts at 45° C. (113° F.) It appears in two forms, either massive, or in long prisms, apparently quadrangular, and terminated by dihedral summits: the latter form is, without doubt, that which contains the minimum of water, and is consequently the most volatile.

Several precautions are required in procuring with certainty the crystallized acid. The sulphuric and perchloric acid are to be introduced successively by a long tube into a small retort not tubulated, the neck of which is to be introduced (without a cork) into a bent tube contracted at the extremity. On applying heat, the liquid boils, and is by little fire preserved in that condition; a portion of the acid soon flows slowly over and solidifies in the tube, which it is sufficient to cool with water. The process must be stopped before the mixture is discoloured, and as soon as a drop of liquid passes over and retains its fluid state, otherwise water will distil and redissolve the crystals forming the liquid and non-fuming acid. For the same reason, only small quantities should be operated on at once, not surpassing eight or ten grammes (140 gr.) of perchloric acid.

Perchloric acid may be concentrated in a retort, by care, almost like sulphuric acid: the first portions are merely water. It has thus been carried to a s. g. of 1.65, and may perhaps be urged further. In this state it evolves some vapour in the air: it boils at 200° C. (392° F.) If, when boiling, dry paper be held in its vapour close to the aperture of the vessel, it inflames vividly. Of this strength ten parts exposed to air attracted 1.8 parts of water in twenty-four hours, and in ten days had attracted 8 parts.*

5. Strength Test for Bleaching Powder.

The necessity of having a means of ascertaining the chlorine strength of bleaching powder has been felt so strongly, that many persons have turned their attention to the discovery of an unexceptionable process for the purpose; and the use of sulphate of indigo, of salts of manganese, and of the chlorometer apparatus of Gay Lussac, is consequently well known to all who are concerned in the use of that chemical production. M. Marozeau, amongst others, has sought to obviate the objections belonging to all the processes

known, and has described, as the result of his exertions, a new process founded on the use of mercurial salts. Let muriatic acid be added to a solution of protonitrate of mercury, in quantity more than sufficient to precipitate all the mercury as calomel; then let a solution of chloride of lime be added: the chlorine set at liberty by the excess of acid will react on the calomel, will convert it into corrosive sublimate, which, dissolving, the solution will become perfectly clear and transparent again, if enough chloride of lime has been added.

This effect, when produced by known solutions of mercury and bleaching powder, and with the attention required to obtain a complete chemical action, is said by M. Marozeau to furnish a very excellent method of ascertaining the strength of bleaching powder: for by agitation of the liquids, all the calomel at first formed may be converted into corrosive sublimate, and dissolved before the slightest odour of chlorine is sensible in the residual liquor. He uses the chlorometer of M. Gay Lussac, but inverts the office of the pipette or fixed measure of bulk: instead of using it to measure out the bulk of solution of chlorine to be tried, it is employed to measure out a fixed quantity of the test solution of nitrate of mercury, and the graduated jar is used to ascertain the quantity of solution of chloride required to convert the calomel when formed into corrosive sublimate.

The strengths of the solutions of nitrate of mercury and bleaching powder to be tried, are made to conform to the dimensions of the instruments constituting Gay Lussac’s chlorometer. The proof-liquor is procured by boiling mercury in excess in dilute nitric acid, continuing the ebullition until no deutonitrate remains in solution. The strength is adjusted in two ways, either by preparing a solution of chloride of lime with a known quantity of chlorine, and then trying it against the test solution as yet unadjusted, and diluting the latter until it agrees with this known solution,—or by ascertaining how much of the test liquor is required to precipitate the whole of the chlorine in a known solution of common salt. For, as the quantity of chlorine in common salt required to convert the mercury in a given quantity of test solution into calomel, is exactly equal to that required afterwards from chloride of lime to convert the calomel so formed into corrosive sublimate, it is easy to make a known solution of salt, and to dilute the test liquor, until a given quantity of it will exactly precipitate a measure of that saline solution; and such test liquor will, by the process recommended, show what quantity of the solution of bleaching powder contains the same proportion of chlorine as the standard solution of salt thus referred to.

M. Marozeau then gives minute instructions for the use of this process, intended for those who, not possessing much chemical knowledge, still have to apply the instrument; and he states that, having used it very constantly, it has afforded him highly satisfactory results.*

* Ann. de Chim., xlvi. 400.
6. Preparation of Iodic Acid.

The following is a method of preparing this acid, devised and recommended by A. Connell, A.M.:—The vessel employed was a rather large and tall flask, into which fifty grains of iodine and an ounce of fuming nitric acid were put; the acid was made to boil, and as soon as any iodine sublimed and condensed on the sides of the vessel, it was washed back again into the liquid by agitation. After the process had been continued some time, a precipitation of white crystalline grains was observed to take place, and the operation of boiling and washing back the sublimed iodine was continued until the free iodine had, to a great extent, disappeared. The whole was then decanted into a shallow basin, and evaporated to dryness.

Any free iodine which had remained was soon dissipated by the heat. The residue of the evaporation consisted of whitish crystalline grains, which were iodic acid, retaining a little nitric acid, from which they appeared to be freed by one or two solutions in water and re-evaporations, when they lost most of their crystalline appearance, and became a whitish deliquescent mass, occasionally with a light purplish tint, from a tendency to decomposition by the heat of evaporation. Where no particular precautions were taken to prevent loss in the state of vapour, and where the process was not continued until the entire disappearance of iodine, the quantity of acid obtained approached that of the iodine employed; a larger proportion of iodine might probably be used with the same quantity of acid *.


The necessity of marking the linen of hospitals, &c., in a perfect and durable manner, so as to resist the action of alkalies, soap, &c., is so important as to have induced M. Henry to examine the methods in use, and endeavour to replace them by a better. The sulphate and muriate of manganese, the sulphate and acetate of iron, nitrate of silver, acetate of alumine and iron, and acetate of lead, mixed with gum or indigo, or ink, have been used for the purpose; but all either require previous or subsequent operations of some nicety, as immersion in carbonated alkalies or hydro-sulphurets, or else such degree of care as to be inexpedient in the hands of the women or persons to whom the duty generally devolves.

The following is the process which M. Henry ultimately recommends as the very best. Take one part, by weight, of iron filings, and three parts of vinegar, or acetic acid of s. g. 1056. Mix the filings with half the vinegar, and agitate it continually. As it thickens, add the rest of the vinegar, and also one part of water. Then apply heat to assist the action, and when all the iron is dissolved, add three parts of sulphate of iron, and one part of gum arabic, previously dissolved in four parts of water. These are to be

* Jameson's Jour., 1831. p. 72.
mixed well at a gentle heat, and will yield twelve parts of the preparation.

When to be used, the linen is to be spread on a table, and the preparation applied by means of a hair brush, and stencil plates of copper*.

8. NEW APPLICATIONS OF ARTIFICIAL ULTRAMARINE.

It is well known that a few years since M. Guimet discovered a process of manufacturing ultramarine from its proximate elements, and without the use of lapis lazuli. He has latterly described a great extension of this, his manufacture, in a letter to M. Gay Lussac. A paper manufacturer wished to apply this ultramarine in place of smalt to the coloration of his papers, and was, in consequence, supplied with a sufficient quantity to make a large experiment. The latter paper made had as good a tint as that coloured with smalt, and was more uniform, but it was found that, in producing this effect, the 1 lb. of ultramarine, because of its extreme division and intense colour, was as effectual as 10 lb. of the finest smalts. After this 200 lb. of ultramarine were sold to the paper-makers of Lyons at the price of 20 francs per lb., and proved to be more economical than smalt. In consequence, M. Guimet has very much extended his manufacture, and is able to sell ultramarine for these uses at the price of 16 francs per lb.

The ultramarine for painters requires a particular purification, as well as careful selection from all that is manufactured. The price for the finest quality is 60 francs the lb. The second quality is 20 francs the lb.

Besides its use in paper-making, the manufacturers of calicoes, muslins, &c., &c., are beginning to use it, and M. Guimet expresses a hope, that shortly France will be entirely independent of other countries for the blues required for these uses*.

9. REDUCTION OF TITANIUM.—(Liebig.)

Recently prepared chloride of titanium and ammonia (of Rose) is to be introduced into a glass tube, two or three feet long and half an inch in diameter, so as, without being pressed, to occupy the half of it. The tube is to be placed horizontally in a furnace, and attached to an apparatus, by means of which, ammoniacal gas, dried by passing over caustic potassa, may be supplied. The vacant parts of the tube are first to be heated whilst a little ammonia is passed in: gradually the part containing the salt is to be heated and the temperature raised until the tube softens. The chloride is entirely reduced, and the tube being opened, when cold the metal is taken out in the form of a dark blue powder, or in plates, having the lustre of copper. If exposed to the air before it is cold, it will inflame and burn into titanic acid.

* Jour. de Pharm., 1831, p. 388. † Ann. de Chimie, xlvi. 431.
When the sublimed chloride is used, the metal is often in very beautiful crystalline groups.

The parts of the tube which are last heated are frequently plugged up with muriate of ammonia: it is useful, therefore, to introduce a smaller tube, and clear away the muriate from time to time which attaches to it.

Perhaps tungsten and molybdenum may be thus reduced; but all similar trials upon silica and alumina have failed.*

10. Preparation of Metallic Cromium.—(Liebig.)

When ammoniacal gas is passed, in a similar manner to the above, over the chloride of chromium and ammonia, heated to redness in a glass tube, metallic chromium is obtained as a black pulverulent metallic powder, assuming lustre when burnished, inflaming at a red heat, and burning into a brown powder.

When ammoniacal gas is passed over the chloride of chromium, the combination occurs with the disengagement of light, the vessel is filled with a purplish red flame, which continues until the chloride is saturated. When the chloride of chromium is heated in ammoniacal gas, the metal is also obtained, but is then of a brown chocolate colour instead of black.

When in the preparation of chloride of chromium the neutral solution of muriate of chromium is evaporated, a green mass is obtained, which evolves no water at temperatures even a little above 212° F. But at temperatures of 400° or 500° F., it begins to smell, and losing water, becomes a brilliant spongy crystalline peach-coloured mass, perfectly fixed in the fire. The conversion of a muriate into a chloride cannot be so convincingly shown on any other substance.

When the chloride is heated in the air, a very beautiful oxide of chrome, as to colour, is obtained, fit for porcelain works.

When chloride of chromium is heated in sulphuretted hydrogen, a crystalline brilliant black sulphuret of chromium is procured.

Metallic chromium, prepared as described, burns, if calcined, in the air, but does not become of a green colour: the oxide thus procured has not been examined, to ascertain whether it be the same with, or a different oxide from the green one †.

11. Analysis of some Mercurial Salts.

Mr. Phillips has recently examined and analysed some of the salts of mercury, especially some sulphates and carbonates. When two parts of mercury and three of sulphuric acid are heated for a short time, some protosulphate of mercury is formed, but on continuing the heat, almost the whole becomes bipersulphate. This, being put into water, is decomposed, and the yellow precipitate, formerly called turpeth mineral, thrown down. When 200 parts of the bipersulphate were put into water, 141.1 parts, and then by heat

* Ann. de Chimie, xliv. 108.  † Ibid., p. 110.
8.4 parts, more of the yellow precipitate were obtained. On analysing this precipitate, the mean of experiments gave sulphuric acid 12.6, and peroxide of mercury 87.5 per cent.; from which it would appear that the salt is a subpersulphate, constituted of—

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<table>
<thead>
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<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>3 atoms sulphuric acid</td>
<td>. . .</td>
<td>120 or 12.2</td>
</tr>
<tr>
<td>4 peroxide</td>
<td>. . .</td>
<td>864</td>
</tr>
<tr>
<td></td>
<td></td>
<td>984</td>
</tr>
</tbody>
</table>

The acid and oxide remaining in the solution have been supposed to constitute a peculiar supersalt; but when 4 atoms of bipersulphate of mercury are acted on by water, 3 atoms of acid and 4 of oxide are precipitated, whilst 5 of acid remain in solution; this dissolving a part of the bipersulphate prevents decomposition of the whole, and the quantity remaining in solution depends, to a certain extent, upon the quantity of water used.

The carbonates of mercury were then examined. Carbonate of potassa, added to protonitrate of mercury, produces at first a yellow, but, when in excess, a black precipitate. The yellow precipitate dissolves in acid without effervescence, and was a subprotonitrate; the black precipitate, when dried by exposure to air, was only black oxide.

By adding carbonate of potassa to pernitrate of mercury, a precipitate with an ochre yellow colour was obtained, which being dried by exposure to air, and then dissolved in nitric acid, lost 4.4 per cent. of carbonic acid; the solution, decomposed by soda, gave 96.1 of peroxide of mercury. The salt is therefore a dipercarbonate, consisting of—

<p>| | | |</p>
<table>
<thead>
<tr>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>2 atoms peroxide</td>
<td>. . .</td>
<td>432 or 95.2</td>
</tr>
<tr>
<td>1 carbonic acid</td>
<td>. . .</td>
<td>22</td>
</tr>
<tr>
<td></td>
<td></td>
<td>454</td>
</tr>
</tbody>
</table>


These compounds may be prepared either by precipitating proto or persalts of mercury by iodide of potassium, or by triturating iodine and mercury together. The former has many inconveniences in consequence of the effects produced by excess of either precipitate, or by changes effected on the iodide itself by different circumstances influential during its formation. M. Berthemot, therefore, very much prefers the latter, which, a little modified according to his suggestion, affords very excellent iodide of mercury.

The protiodide consists of single proportionals of the elements, consequently, according to Berzelius, of mercury 1265.822 and iodine 789.145, or per cent. of 61.6 and 38.4. These weights are therefore to be taken, put into a mortar with a flat bottom, and triturated together. The mixture soon takes a reddish colour, and upon adding a few drops of the very strongest alcohol, and continuing to

* Phil. Mag. N. S., x., 205.
triturate it, will become yellowish green, and all the mercury and iodine will have disappeared. The alcohol will be almost entirely evaporated, and a very fine protiodide will have been produced. In this operation there is formed at first deutiodide mixed with iodine and mercury; the alcohol, by dividing the deutiodide and dissolving the iodine, so as to form a very concentrated solution, brings the whole into mutual contact, and very promptly determines the combination of the two bodies.

When 100 parts of the deutiodide, and 44.5 of metallic mercury are triturated with a little alcohol, protiodide is also readily formed.

Deutiodide of mercury consists of 1 proportional of mercury to 2 of iodine, or of mercury 1265.822, and iodine 1578.29, or per cent. 44.51 and 55.49. These proportions being mixed are to be triturated as before, but the alcohol must be added drop by drop, and the trituration continued a few moments between each addition. The addition of too much alcohol renders the action so strong, that much heat is evolved, the mixture even fuses, and the iodine is vapourised and lost. This process succeeds very well, and gives a compound of known and definite composition, although the appearance is not so beautiful, nor the colour so fine, as that prepared by precipitation. This is due, without doubt, to the particular state of aggregation and arrangement which the particles acquire in precipitation. But when both are made to undergo sublimation, they become alike in every respect.*

13. ON BORATE OF SILVER.—(Rose.)

When a concentrated solution of borax (either fused or crystalized) is mingled with a moderately strong solution of nitrate of silver, a white precipitate of borate of silver falls. Whichever way the solutions are mingled the same effect takes place. When water is gradually added to this precipitate, it dissolves entirely, like most of the precipitates produced by alkaline borates, for very few appear to be insoluble in water. Water produces no change in the constitution of this substance; light renders it violet or black. When washed as well as may be, it was analysed, and its constitution appeared to be—

<table>
<thead>
<tr>
<th>Oxide of silver</th>
<th>76.9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Boracic acid</td>
<td>23.1</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>100.0</td>
</tr>
</tbody>
</table>

Here the acid contains only thrice the oxygen of the oxide, but in borax the acid contains six times the oxygen of the base. If, therefore, borax be a neutral salt, this argentiferous compound is a subsalt.

When a saturated solution of borax is diluted thirty or forty times, so that the water present shall be enough to dissolve any borate of silver formed, one of the solutions being in excess and the two being

* Journ. de. Pharm. 1831, 456.
poured indifferently to each other, a precipitate is obtained quite different from the borate of silver. It is brown, insoluble in water, and when well washed proves to be oxide of silver; by heat it loses 9 per cent. of oxygen and water.

Thus, whilst a concentrated solution of borax produces a subsalt from a solution of silver, the effect of the boracic acid is entirely lost by mere dilution, and the solution acts, with respect to silver salts, as pure alkali. This effect of water cannot be compared to that upon bismuth salts or verdigris, for the subborate of silver is entirely soluble in water.

Solution of borate of potass acts in the same manner. Borate of ammonia in strong solution gives, with nitrate of silver, a white soluble precipitate, and, when diluted, it produces no precipitation. Sulphate of silver acts in the same manner as the nitrate*.

14. IGNITING PLATINA.

Dr. Hare says, 'I find that if asbestos or charcoal be soaked under an exhausted receiver in muriate of platina, then dried in an evaporating oven for twenty-four hours, and afterwards ignited, the property of ignition in the gaseous elements of water is acquired.'

15. ON ORGANIC ANALYSIS, AND ON THE CONSTITUENTS OF VARIOUS ORGANIZED BODIES.

MM. Henry and Plisson have been very earnestly employed for some time past in endeavouring to improve the means of analysing organic bodies, so as to obtain accurate estimates of the proportions of their elements, and for this purpose have exerted themselves to give the element under consideration, whether oxygen, hydrogen, carbon, or azote, a gaseous form, conceiving that the probable errors in the mensuration of gases are far smaller than those likely to occur in the estimation of weights. Their processes have been published in several memoirs in the Journal de Pharmacie, and elsewhere, and they have finally given an account of the results of many analyses obtained by their methods, which it is our intention to abstract or transcribe.

The substances were used in the purest possible state; for they were—i. Prepared with great care and crystallized many times. ii. They were calcined to ascertain the absence of inorganic matter. iii. They were carefully dried at 212° F. iv. The mixtures of the principles and oxide of copper were always preceeded in the tube by an extensive portion of pure oxide of copper, heated powerfully, so as to decompose the carburetted hydrogen and oil formed upon the first action of heat on the destructible matter. v. The tube and compound were always placed in the most favourable circumstances for the avoidance of errors.

* Ann. de Chimie, xlvi, 319,
† Silliman's Jour., xx. 160.
Carbon—Has always been estimated in the state of carbonic acid; and when heat has been applied and the oxide of copper has done its duty, pure oxygen has been passed through the tube to burn what carbon might remain in contact with the reduced copper: this precaution appears very necessary; 100 of carbonic acid was considered as containing 27.6508 of carbon.

Hydrogen—Was occasionally obtained in the gaseous state, by bringing the water produced in the analysis in contact with the alloy of antimony and potassa, first cold, and then heated; but as upon various trials the process of estimating its quantity from the weight of water produced was found to be exact, it was generally adopted.

Azote.—This principle was obtained in the gaseous state by passing the nitrous oxides and acid over heated sulphuret of barium (sulphate of baryta heated with charcoal); the oxygen was taken away and the nitrogen sent forward into the receiver; but that no hyponitrite of baryta might be formed from nitrous acid, some pieces of metallic copper were interposed between the oxide of copper and the sulphuret of barium; this would convert any nitrous acid into nitric oxide, and prevent the source of error referred to.

The following are the proportions of the elements contained in many substances, each being in a very pure and dry state.

<table>
<thead>
<tr>
<th></th>
<th>Carbon</th>
<th>Hydrogen</th>
<th>Oxygen</th>
<th>Nitrogen</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Mannite</td>
<td>0.38770</td>
<td>0.08467</td>
<td>0.52743</td>
</tr>
<tr>
<td>2</td>
<td>Cantharidine</td>
<td>0.68560</td>
<td>0.08432</td>
<td>0.13152</td>
</tr>
<tr>
<td>3</td>
<td>Piperine</td>
<td>0.76100</td>
<td>0.10274</td>
<td>0.13626</td>
</tr>
<tr>
<td>4</td>
<td>Caryophylline</td>
<td>0.81920</td>
<td>0.12250</td>
<td>0.05730</td>
</tr>
<tr>
<td>5</td>
<td>Aurade</td>
<td>0.88760</td>
<td>0.150892</td>
<td>0.011508</td>
</tr>
<tr>
<td>6</td>
<td>Amyrine</td>
<td>0.8104000</td>
<td>0.1047368</td>
<td>0.0848632</td>
</tr>
<tr>
<td>7</td>
<td>Ceroxyline</td>
<td>0.83200</td>
<td>0.11050</td>
<td>0.05750</td>
</tr>
<tr>
<td>8</td>
<td>Arbre à brai</td>
<td>0.797250</td>
<td>0.106512</td>
<td>0.096203</td>
</tr>
<tr>
<td>9</td>
<td>Alouchi</td>
<td>0.8264000</td>
<td>0.1100624</td>
<td>0.0635376</td>
</tr>
<tr>
<td>10</td>
<td>Copal</td>
<td>C. balsam</td>
<td>0.8925</td>
<td>0.1046</td>
</tr>
<tr>
<td>11</td>
<td>Black mustard</td>
<td>0.5328000</td>
<td>0.111840</td>
<td>0.094416</td>
</tr>
<tr>
<td>13</td>
<td>Morphia</td>
<td>0.7052000</td>
<td>0.079884</td>
<td>0.167056</td>
</tr>
<tr>
<td>14</td>
<td>Quinia</td>
<td>0.7455200</td>
<td>0.084322</td>
<td>0.087212</td>
</tr>
<tr>
<td>15</td>
<td>Cinchonia</td>
<td>0.7888000</td>
<td>0.085760</td>
<td>0.028918</td>
</tr>
<tr>
<td>16</td>
<td>Strychnia</td>
<td>0.7640000</td>
<td>0.078784</td>
<td>0.082190</td>
</tr>
<tr>
<td>17</td>
<td>Brucia</td>
<td>0.7048000</td>
<td>0.078108</td>
<td>0.149154</td>
</tr>
</tbody>
</table>

Aurade (5) was discovered in the volatile oil of orange flames by one of the authors. 6, 7, 8, and 9 are subresins discovered by M. Bonastre, and described in the Journals some time since. M. Bonastre supplied those which were analysed. There is reason to fear, from the oxygen in the volatile oil of bitter almonds, that the air had acted on it, although great care was taken to prevent the contact.

* Sulphur 0.111792.
With regard to the vege-to-alkalies 13, 14, 15, 16, and 17, every possible care was taken to obtain them in a state of purity, and especially free the one from another. The occurrence of several of them in conjunction rendered the latter precaution exceedingly necessary, and the authors suggest that probably the discordant results already published arise from some such mixture.

16. Results obtained from the Seed of the Mango.

The mango tree (Mangifera Indica, L.) has been transported from the East Indies to St. Domingo, and the other neighbouring islands, where it is now exceedingly abundant. In consequence of which its products may now find useful applications; to forward which purpose M. Arequin has devoted his attention to the analysis of the seed. The fruit is a fine mass of pulp, very agreeable in the estimation of some, and the seed or grain lies in the middle, having the form of a kidney, and inclosed in a parchment-like integument.

The mango pulp contains much crystallizable sugar, and also citric acid and gum.

The mango-seed is remarkable for the large quantity of gallic acid present, and for the presence also of stearic acid, and for the useful state of its starch. When a seed is cut with a knife, it gives a deep blue colour to the latter; when touched with persulphate of iron, it acquires a fine blue colour, both effects due to the gallic acid present.

Five pounds and a half of the seeds being worked upon, by various digestions in water, alcohol, &c, and subsequent evaporations gave above eight ounces and a half of crystallized gallic acid.

When the pulp of the seeds had been exhausted by water, it was acted upon by alcohol, and a substance obtained by evaporation from the alcoholic solutions, which crystallized, and had the following properties: it was perfectly white; was insipid and inodorous; it fused at 70° C. (158° Fahrenheit); on cooling, it crystallized in mingling long acicular forms; it dissolved in strong boiling alcohol, in all proportions, the solution on cooling yielding mammellated groups of acicular crystals; it is insoluble in water; it reddens moistened litmus paper; its solution in weak alcohol reddens infusion of litmus; it is quite soluble in oils and fatty bodies; it unites to salifiable bases, forming well characterized salts (soaps); when made into a taper, it burns like wax, with a fine white flame. This substance has all the physical and chemical characters of stearic acid, which therefore exists, ready formed, in the vegetable kingdom. Its quantity was rather more than two ounces.

When the pulp, thus far exhausted, was treated with ether, a fatty matter was obtained from it, fusing at 30° C. (86° Fahrenheit); soluble in hot ether to any extent; insoluble in rectified alcohol; liquefying in the mouth like cocoa butter; when formed into a candle,

* Journ. de Pharm., 1831, p. 437.
burning like tallow; having the consistence of tallow, and being of the same nature as the butter of cocoa. The powdered grain treated with water yields a small portion of this butter in a very pure and fresh state. The quantity obtained from the original quantity of seed was one ounce and a half.

After all these operations the starch was separated by washing in water; its quantity amounted to $32\frac{1}{2}$ oz., or rather more than half the weight of the dried seeds. When the recent seeds were worked with for starch, 1 lb. always yielded about 6 oz. of starch, and by drying lost about 6 oz. of water.

Besides these substances the following were also obtained;—lignine above 5 oz.; gum $2\frac{1}{2}$ oz.; tannin 200 grs., nearly; brown resin 200 grs.; green resin 144 grs., and a little vegetable albumen.

M. Arequin then describes processes for obtaining gallic acid from the mango seed, either with or without the use of alcohol, and for the preparation of ink with this substance instead of galls. If obtained in abundance, the seeds may be very useful for these and analogous purposes.

17. On Lactic Acid.

Berzelius has exerted his talents in the explication and clearance of the doubts which have existed relative to the existence of lactic acid, independent of the acetic acid. Gmelin, on distilling fluids containing lactic acid, obtained a product which feebly reddened litmus paper, and which, saturated by baryta and the solution evaporated, gave a white pellicle, which, when touched with sulphuric acid, evolved the odour of acetic acid.

Berzelius repeated the experiment, and obtained the same result, but the acid odour never occurred unless muriatic acid was present in the results of the distillation. When pure lactic acid with water was distilled and the product evaporated, it reddened litmus paper, because a little of the acid had been carried over with the vapours, but tartaric acid was found to pass over in the same manner. In fact, it is extremely difficult to prevent a little of the contents of the retorts, in the finely divided state assumed, from passing over: but when the product of lactic acid was distilled a second time, not a trace of acid passed over. If acetic acid had been present it would have distilled.

The question then arose whether lactic acid was a compound of acetic acid and an animal matter analogous to the sulpho-vinates. All attempts to prove this in the affirmative failed. Lactic acid was heated up to the point at which extractive matter would become brown, and then a current of ammonia passed over it, but no acetate of ammonia passed forward in vapour, though that must have occurred if acetic acid had been present.

The following are the methods adopted by Berzelius to obtain pure lactic acid. The acid alcoholic extract obtained from the liquids of

* Journ, de Pharm., 1831, p. 421.
milk or meat is to be dissolved in strong alcohol and mixed with a strong alcoholic solution of tartaric acid, whilst any precipitate falls: being left for twenty-four hours in a cold place, the double tartrate separates. The solution being evaporated, the extract is to be dissolved in water, and well pulverised carbonate of lead added as long as it dissolves, and till the solution has a sweet taste; it is then to be acted upon by animal charcoal, and afterwards by sulphuretted hydrogen, to remove the lead. The liquid is to be evaporated that all sulphuretted hydrogen may be expelled; and then, mixed with hydrated protoxide of tin recently prepared, well washed and still moist, it is to be left several days in contact, with agitation at intervals. The sublactate of tin produced, when well washed, is to be decomposed by sulphuretted hydrogen, and thus the purest possible lactic acid has been obtained. In this way much acid remains in the solution and is virtually lost. Whether this is another acid, resulting from the partial decomposition of that under purification, or whether it forms a soluble perlactate of tin, is uncertain; but the liquid from over the protoxide of tin, when acted on by sulphuretted hydrogen, gives bisulphuret of tin.

Another process is to saturate the acid alcoholic extract with carbonate of potassa or soda; evaporate and heat the mass obtained in a sand bath till it fuses, becomes brown and evolves the odour of urine, and ultimately of herring, or roast meat: to dissolve in water, act by animal charcoal till colourless; filter, evaporate to dryness; dissolve in alcohol; decompose by tartaric acid; remove the excess of tartaric acid by carbonate of lead; precipitate the lead by sulphuretted hydrogen, and evaporate. In this way colourless acid is obtained, but it contains extractive matter, and is less pure than the former.

Lactic acid is colourless, inodorous; it possesses a sharp taste, rapidly diminishing on the addition of water till the taste is nearly lost. When evaporated at 212° F., until it loses nothing more, it becomes thick so as to flow with difficulty or even to be a soft solid. It attracts moisture from the atmosphere. When strongly heated, it becomes brown, boils slightly, evolves a suffocating odour like that of heated oxalic acid, blackens, swells, and leaves a bulky charcoal. It dissolves readily in alcohol and in small quantities in ether.

Many of the salts are uncrystallizable, or gummy; those of potassa, ammonia, magnesia, and zinc crystallized. They dissolve in alcohol, though sometimes with difficulty, especially if there be excess of base present.*

18. On Camphor and Camphoric Acid.—M. J. Liebge.

The accounts given of the camphorates by R. Brandes and Bouillon-Lagrange are extremely different. These differences are, by M. Liebge, referred to these chemists having used two different kinds of camphoric acid. Camphor, acted upon by concentrated nitric acid,

* Ann. de Chimie, xlvi. 420.
fuses into a yellow liquid; by long digestion this disappears, and the acid liquor, on cooling, deposits a large quantity of opaque white crystals, which when boiled with water communicate an odour of camphor to it. This is the camphoric acid of Bouillon, and it yields salts either slightly soluble or insoluble.

These crystals are a chemical combination of camphor and camphoric acid, and may be prepared at once by dissolving camphor in camphoric acid fused at a gentle heat. If they be acted on a second time by strong nitric acid, crystals more transparent are obtained, which give exactly the soluble salts described by Brandes. Even this acid was found by M. Liebeg not to be quite pure, and therefore it was again heated with nitric acid until the crystals obtained, when boiled with water, gave no odour of camphor to its vapour.

Camphorate of lead was prepared with this acid and decomposed by sulphuric acid; from 1105 parts of the camphorate were obtained 760 of the sulphate, the equivalent number of the acid is therefore 135.67. On analysing the camphorate of lead by oxide of copper for the composition of camphoric acid, it came out as follows:

<table>
<thead>
<tr>
<th>Calculation</th>
<th>Experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>10 atoms of carbon 76.4370</td>
<td>56.29</td>
</tr>
<tr>
<td>15 ,, hydrogen 9.3597</td>
<td>6.89</td>
</tr>
<tr>
<td>5 ,, oxygen 50.0000</td>
<td>36.82</td>
</tr>
<tr>
<td>135.7967 100.00</td>
<td>100.00</td>
</tr>
</tbody>
</table>

Many chemists, ‘amongst which I counted myself,’ says M. Liebeg, calculate that 2 volumes of hydrogen are equal to an atom, whilst others consider 1 volume as an atom. Camphoric acid seems to leave no doubt on this subject: it contains 15 atoms of single volumes, and only \( \frac{7}{2} \) of double volumes. Now \( \frac{7}{2} \) atoms could only arise from an error in analysis, but every possible care was taken to avoid these: on the contrary, 15 atoms is in the simplest ratio with the other bodies present.

When camphor is acted on by nitric acid, there is no effervescence, and no carbonic acid gas is disengaged. It would seem as if the camphor was merely oxidized. M. Liebeg, therefore, analysed camphor to see if the mere addition of oxygen would give the constitution of camphoric acid above determined. His best experiments (of which he still speaks cautiously) gave

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Carbon</td>
<td>81.763</td>
<td>1 atom</td>
</tr>
<tr>
<td>Hydrogen</td>
<td>9.702</td>
<td>18 atoms</td>
</tr>
<tr>
<td>Oxygen</td>
<td>8.535</td>
<td>12 &quot;</td>
</tr>
</tbody>
</table>

The hydrogen and oxygen are, therefore, in the same ratio as in camphoric acid, and if it be supposed that, during the action of nitric acid on camphor, 5 atoms of oxygen be given, the acid will result.
Hence camphor appears to act like a simple body, and there is no other case of a similar kind amongst organic bodies, except with indigo.

19. Action of Heat on Acetate of Lead.—(Matteuci.)
When acetate of lead is heated it fuses at 135° F., and boils at 212°. During the ebullition it loses its three proportionals of water; and solidifies nearly at the same temperature. When the temperature is still further raised fusion re-occurs at 536° F., the liquid boils for some time, becomes brownish, and again solidifies into a dull white uncrystalline mass, which is a trisacetate of lead. During the operation, acetic acid, with a little pyroacetic spirit, comes over, but at a later period only pyroacetic spirit, with abundance of carbonic acid, is obtained.

The pyroacetic spirit being analysed was found to contain hydrogen 6.4039; carbon 59.56; and oxygen 33.7361 per cent., to 3:5:1 volume of these substances. This composition may be represented by 1 proportion of acetic acid 1 of water, and a substance composed of 6 proportions of carbon and 2 of hydrogen. Pyroacetic spirit soon begins to decompose, and when exposed only a few minutes to the air, becomes acid and milky; acetic acid and an oleaginous body (in appearance) are produced. When heated in contact with potassa or lime, acetates are formed and the oleaginous substance evolved. With chlorine it produces muriatic and acetic acids, and the same oleaginous body. The oleaginous principle has an aromatic odour, and by exposure to air becomes green; it is insoluble in water, but soluble in alcohol. It contains no chlorine, but is a binary compound of carbon and hydrogen.

§ III. NATURAL HISTORY.

1. Industry of Birds.
Dr. Steel, who resides near the mineral springs of Saratoga, has observed, that the river swallow (Hirundo riparia) can, when necessary, vary the construction of its nest. When it finds steep sandy banks, it excavates them, forming holes for its nest to which its enemies cannot obtain access. But when such a bank is wanting, it approaches the habitations of men, and though less familiar than the window swallow, it attaches its nest to granaries, cart houses, and similar places. Then it is obliged to build instead of excavate, and it does in fact gather materials and put them together. It appears too that this species has not essentially the habits indicated by its name: it can live and thrive wherever it can find subsistence, security, and society; (for isolated families or solitary nests do not occur.)

small colony, which in 1828 fixed itself at Saratoga, increased so rapidly, that in 1830 there were several hundred nests.

These animals, therefore, have innate faculties, to which the name of instinct may be given, and acquired faculties, variable according to circumstances: they may create feeble arts (according to Buffon’s expression), and lose them as rapidly as they were acquired, if circumstances be not favourable to the continuance of their practice.

2. On the rapid Flight of Insects.

In passing along the Manchester and Liverpool railway at a speed of about twenty-four miles an hour, ascertained by a stop-watch, I observed one of the smaller humble-bees, I think the *Apis subinterrupta*, flying for a considerable distance, and keeping pace with the train, apparently without the slightest effort; in fact, the little traveller was going at a rate far more rapid than ours, for its accompaniment was not in a straight line, but in that well known zigzag mode of flight, observable when these insects are hovering from flower to flower in search of food. Several house blue-bottle and horse-flies were also repeated visitors: our rapid motion seemed to have no manner of effect upon them, for, when it suited their purpose, they darted onwards for a few feet or yards, or balanced themselves steadily over any given point, though in an instant, whenever their efforts relaxed, or they thought it expedient to part company, they were far away in our rear. I should observe, moreover, that the wind at the time was blowing obliquely against us with a current of such strength, that I occasionally had some difficulty in keeping my hat on. Under all circumstances, therefore, of the wind’s opposition and their irregular motion, I considered that the locomotive powers of these insects could not be well less than from thirty to forty miles an hour. Compared with the beautifully arranged muscular powers of these minute beings in the creation, how insignificant are those which science, with all its advantages, has hitherto been able to accomplish by mechanical means!—D. T.

[It is to be presumed that these insects were not in any way sheltered by the train, i.e. were not to leeward of or behind any advancing part, for then they would find no difficulty in keeping pace with the engines. But it would have been better if the fact had been distinctly noticed and stated.—Ed.]

3. Useful Astringent in Cases of Mercurial Salivation.

(M. Virey.)

It often occurs that in cases of mercurial treatment the various astringents of tea, myrrh, bark, borax, alum, and even nitric acid, are insufficient to arrest the flow of saliva, in consequence of which injurious results have occurred, very difficult to remedy. It has

* Rev. Ency., l. p. 173. \[ Phil. Mag. N.S., x. p. 150. \]
been found in the United States, that the decoction of the bark of a sumach (Rhus glabra) has, in this respect, remarkable powers. Care must be taken not to confound this species with the Rhus vernix, which very much resembles it, for the latter is acrid, and, according to Dr. Fahnestock, produces injurious effects. In the genus of sumach there are aromatic as well as poisonous species. Of the former kind are the Rhus suaveolens, and Rhus aromaticum, both of North America*.

4. Relief for the Tooth-ache.

The following account is by Dr. Ryan, who himself testifies to the efficacy of the remedy recommended. 'Like many of our best remedies, that which I proceed to notice (for the tooth-ache) was discovered by accident. A gentleman who attends my lectures (Mr. Myers, of Newington Causeway) had frequently applied sulphuric acid to his tooth with some relief, but on one occasion, he, in a moment of confusion, took down the next bottle to his remedy, which contained nitric acid: to his great surprise he experienced immediate relief and without the slightest pain. Since that period he has not suffered from tooth-ache, though three years have now elapsed. During the last winter he informed me of the success of this remedy, which induced me to try it while labouring under the most intense pain from tooth-ache. The effect was immediate, and no pain whatever was induced. I have since used it in numerous cases, and invariably with complete success. In some instances the disease does not return for days, or weeks, and in others not for months.'

The best mode of employing it is by means of lint wrapped round a probe and moistened with the acid, which is then to be slowly applied to the cavity of the tooth, care being taken not to touch the other teeth, the gums, or the cheeks. On withdrawing the probe and inquiring how the patient feels, the usual reply is, 'The pain is entirely gone.' The mouth is next to be washed with tepid water. The acid should be gradually applied to the whole cavity of the tooth, or otherwise a second application will be required before complete relief will be obtained.

This remedy may be used when the gum and cheek are inflamed so as to preclude the possibility of extraction. In cases where the diseased fang remains, and when the caries faces the adjacent tooth, it obviates the necessity of extraction in all cases of hollow teeth, which all practitioners declare to be desirable if possible, and it enables the dentist to perform the operation of 'stopping or filling teeth' much sooner than he can otherwise accomplish. In a word, it will alleviate a vast deal of human suffering and supersede a most painful operation. It does not accelerate the decay of the tooth to which it is applied†.

5. Poisoning by the (sebacic) Acid of Goose-Grease.

On the 2d of April, 1829, Dr. Siedler was called to attend MM. H——, and their children. On his arrival he found the two brothers H——, one aged thirty-one, the second twenty-eight years, and the two children of the first, one a girl æt. four, the other a boy æt. two and a half, all presenting the following symptoms,—cold sweat, anxiety, vertigo, general paleness and prostration of strength, eyes sunken, and pupils dilated; burning pain was felt in the lower part of the belly, increased by pressure; violent vomiting succeeded by ardent thirst, for which the patients had drunk large quantities of milk, which was thrown up without producing any effect; tongue dry, involuntary discharge of urine and feces.

The eldest brother was insensible for six minutes; his respiration was scarcely visible, his pulse imperceptible, and the heart's action exceedingly weak. The second brother had vomited blood several times, but he experienced less abdominal pain than the other. In the little boy the globes of the eyes were turned upwards, the lips livid, and the pulse scarcely sensible. Lastly, the symptoms in the little girl were the mildest of all. M. Siedler suspected at once that these accidents were occasioned by the use of a certain quantity of goose-grease, which had been employed in the preparation of some meat, of which the four patients had eaten shortly before the symptoms began. An emulsion, containing hyoscyamus, was prescribed, and on the 9th of April all had recovered.

The vomited matters were subjected to chemical analysis: they were strongly acid, but contained no metallic poison: but the following facts induced Dr. Siedler to attribute the illness to the effect of sebacic acid. The lady of the house had made use of goose-grease to dress some veal, and all the persons who partook of the dish fell quickly sick. The lady herself, who had barely tasted it, felt it so disagreeable, that she took no more. None of the grease which was suspected to have caused the accident remained for examination, the pot which contained it having been entirely emptied and cleaned out; but on examining the same kind of grease contained in three other pots, it was found to exhale a strong repulsive odour, and it reddened strongly blue paper tinged by turnsole. Three ounces of this grease were given to a vigorous, well-formed dog: an hour after, his extremities became violently convulsed; he cried piteously, he refused to eat, his eyes were suffused, pupils dilated, skin cold, and arterial pulsations scarcely perceptible. In this state he continued for thirty hours, after which he slowly recovered*.


We see peculiar appearances in weak and morbidly sensitive eyes before the breaking out of a violent storm, showing the positive influence of an atmosphere which has now attained its maximum of

* Hufeland's Journal.
electricity, and I am acquainted with several persons who are able, from a certain premonitory feeling of their weak eyes, to predict a thunder-storm, infallibly, a considerable time beforehand. As every experienced oculist is convinced of the bad effects of an atmosphere of this kind, he will never extract the cataract during the approach of a thunder-storm.

### 7. Native Country of the Potatoe.

The following observations are made upon this highly interesting subject by Mr. Cruickshanks, in Dr. Hooker's Botanical Miscellany.

Mr. Lambert, in the tenth volume of Brande's Journal, and in the appendix to his splendid work on the genus *Pinus*, has collected many valuable facts which prove that the potatoe is found wild in several parts of America, and among others in Chili and Peru. Don Jose Pavon, in a letter to Mr. Lambert, says, 'The *Solanum tuberosum* grows wild in the environs of Lima, and fourteen leagues from Lima on the coast; and I myself have found it in the kingdom of Chili,'—and Mr. Lambert adds, 'I have lately received from Mr. Pavon very fine wild specimens of *Solanum tuberosum*, collected by himself in Peru.' There is also a note from Mr. Lambert on the same subject, in the third volume of the *New Edin. Phil. Journ.*, with an extract from a letter of Mr. Caldcleugh, who sent tubers of the wild plant, some years ago, from Chili to the Horticultural Society.

But it is frequently objected, that in some of those countries where the *potatoe* is found wild, it may, like many other species met with in that state in America, be an *introduced*, not an *indigenous* plant. There are, however, many reasons for believing that it is really indigenous in Chili, and that wild specimens found there have not been accidentally propagated from any cultivated variety. In that country it is generally found in steep, rocky places, where it could never have been cultivated, and where its accidental introduction is almost impossible. It is very common about Valparaiso, and I have noticed it along the coast for fifteen leagues to the northward of that port; much farther it may extend north or south, I know not. It chiefly inhabits the cliffs and hills near the sea, and I do not recollect to have seen it at more than two or three leagues from the coast. But there is one peculiarity in the wild plant that I have never seen noticed in print, that its flowers are always *pure white*, free from the purple tint so common in the cultivated varieties, and this, I think, is a strong evidence of its native origin. Another proof may be drawn from the fact, that while it is often met with in mountainous places, remote from cultivated ground, it is not seen in the immediate neighbourhood of the fields and gardens where it is planted, unless a *stream of water run through the ground*, which may carry tubers to uncultivated spots.

Having observed the distribution of this and other plants through the agency of the streams employed for irrigating the land, I am led to think, that the wild specimens found near Lima may have had

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similar origin. If they occurred in the valley, this is more than probable, as almost the whole of the land is either cultivated by irrigation, or the uncultivated spots are overflowed when the river is swelled by the rains in the interior. I remember a curious instance of this sort of vegetable colonization. In the vineyards of Chili, it is customary, in order to economise the land, to sow lucerne among the vines, to the great injury of the latter, as it prevents the ground from being ploughed or hoed. An intelligent landowner, who had travelled in France, and observed the beneficial effects of turning up and manuring the land, determined to adopt the same system in a large vineyard he was planting near Santiago, and gave orders to his mayor domo not to sow lucerne seed in it as usual. On visiting his estate some months afterwards, he was astonished to find the land covered with young plants of the forbidden pasture, although none had been sown; and on investigating the matter, it was found that the stream which irrigated his grounds passed first through several lucerne fields in another part of the valley, from which it had carried and disseminated seed over the whole vineyard.

Humboldt, who has bestowed such unwearyed attention on the subject of plants cultivated in the New World, (but whose work was published previous to that of Mr. Lambert,) denies that the potatoe is indigenous to Peru. In his Essai Politique sur le Royaume de la Nouvelle Espagne, he says, 'J'observe d'abord, pour ne consigner ici que des faits exacts, que la pomme de terre n'est pas indigène au Pérou, et qu'elle ne se trouve pas nulle part sauvage dans la partie de la Cordillère qui est située sous les tropiques. Nous avons, M. Bonpland et moi, herborisé sur le dos et sur la pente des Andes, depuis les 5° nord, jusqu'aux 12° sud; nous avons pris des informations chez des personnes qui ont examiné cette chaîne de montagnes colosales jusqu'à La Paz et à Oruro, et nous sommes sûrs que dans cette vaste étendue de terreil ne végète spontanément aucune espèce de Solanée à racines nourrissantes. MM. Ruiz et Pavon, dont l'autorité est d'un grand poids, disent avoir trouvé la pomme de terre dans les terrains cultivés, in cultis, et non dans les forêts et sur le dos des montagnes,' page 400. The last paragraph, however, is at variance with the letter of Don José to Mr. Lambert, and more appears to be inferred from what Ruiz and Pavon say on the subject in the Flora Peruviana, than those authors intended. The passage in that work, after the description of the Solanum tuberosum, is as follows:—Habitat in Peruviae et Chilensis regni cultis, et in collibus Chancay, ad Jeguan et Pasamayo præedia.' If they had only found it in cultivated land, the first part of this passage would have been sufficient; but the context leaves it to be understood that that circumstance does not apply to its locality at Chancay.

Chancay is a town on the coast of Peru, which gives its name to the surrounding district or jurisdiction; in which the estates of Jeguan and Pasamayo are situated, and it is doubtless the place alluded to in Don José's letter, being about the distance he mentions north of Lima. There is a great extent of cultivated land in the neighbour-
hood, irrigated from the river of Pasamayo, (called also the river of Chancay,) but Ruiz and Pavon say, they found the plant in the hills, where, as I have before observed, there is no cultivation. As nothing, however, is stated of the nature of the hills, nor of the height at which the plant occurs above the valley, there is still room to suspect that it may have been accidentally introduced, and, indeed, the Indians formerly brought water upon the land from a considerable distance, at a much greater elevation than any that is irrigated at the present day.

Upon the whole, it may be safely concluded that this important vegetable is really indigenous to Chili; but with respect to Peru, some further evidence appears necessary to remove all doubt on the subject. The question can only be decided by ascertaining the exact situations in which the plants present themselves at Lima and Chancay, especially with respect to land that is or has been cultivated. It would be interesting, too, to know the colour of the flowers.

8. New Forms of Cellular Tissue.

Dr. J. E. Purkinje has ascertained that the tissue which constitutes the lining of the case of an anther is of a peculiar kind. It consists generally of vesicular cellular tissue, the membranous walls of which are marked with spiral fibres, coiled up in the inside from the one end to the other; sometimes the fibres are placed like a number of ribs passing from the base to the apex, without any trace of a spiral direction. Occasionally the membrane of the tissue disappears and fibres only remain; and in some cases the whole of the tissue is reduced to a number of points sticking up from the inside of the anther. It is probable that these and similar observations will throw much light upon the analogy that exists between cellular and vascular tissue.


From the researches of Dr. L. W. T. Bischoff of Bonn it appears that the spiral vessels of plants contain no fluid, but serve exclusively to convey air, and that this air has from 7 to 8 per cent. more oxygen than the atmosphere. He found by very delicate and repeated experiments, that the air from the spiral vessels of malva arborea contained 27.9 per cent. of oxygen, and cucurbita pepo, at two different times, 29.8 and 27.9 per cent. From the researches of the same author, it seems that the spiral vessel is a delicate pellucid membranous tube, within which a spiral fibre is generated; that when the whole surface of the tube is filled up by the coils of the spiral fibre, a true spiral vessel is the result, and that spurious spiral vessels, or ducts, with all their modifications, are formed by dislocations or separations of the spires within the membrane. Dotted ducts, for instance, are, according to Dr. Bischoff, caused by the separation of the spiral fibre into minute points.
Foreign and Miscellaneous Intelligence.

10. Cryptogamia in Molasses.—Virey.

Van Dyk and Van Beck of Utrecht have observed a black substance in molasses which rapidly extends and enlarges, and is, according to them, a cryptogamous plant identical with the conferva mucoroides of Agardh (Syst. Algarum, Lund., 1824); or more recently, the syncollesia mucoroides (in alge conferveoidae fungine.) The genus syncollesia of Nees is thus characterised by Agardh: globuli minutilissimi, in fila repentia, caespitosa, coadunati, leviter inundati. The Dutch authors make a species of that mentioned above, syncollesia sacchari; different from the hyphomycet us of Martius, the aleuryoma granulosum of Martius, and the sporotrichum densum and S. vitellinum of Link, all of which are observed in molasses.

These mouldy productions appear to be due to the impure waters with which the sugar moulds are washed; lime water kills them.*

11. Algerine Sirocco.

When the south wind blows at Algiers, the temperature rises rapidly 5° or even 10° C. (9° or 18° F.) On the 17th of September, 1830, the thermometer rose to 39° C. (104° F.) in the shade. It was then as if all were in a furnace, and men and animals found respiration difficult. Captain Boissel, who superintended the works on that day in the suburb of Babazon, remarked that drunken men fell senseless; those who were not so drunk resisted the effect a little, but at last fell; and those who had drunk only a little too much suffered from very violent head-ache and were obliged to sit down †.


Storms are not very common in the climate of Algiers, nevertheless several occurred (towards the end of 1830), breaking upon Mount Atlas. On the 8th of October, in the evening, the air towards the south was powerfully charged with electricity, the whole horizon seemed on fire, and the thunder was continual. At the same moment a strong white light was seen at the extremities of all the poles of the pavilions which were within Algiers, or in the neighbouring forts, and which continued for half an hour. Artillery officers who were promenading bare-headed on the terrace of fort Babazon were very much astonished to feel their hair rise upright on their heads, and to observe, with those which were visible, a small star of light at the extremity of each. When they raised their hands in the air, stars formed at the extremities of their fingers; these disappeared when the hands were lowered. During the whole time of this storm every one felt nervous sensations, and great lassitude over the whole body, but especially in the legs. These effects were observed, and are described by M. Royet ‡.

* Journ. de Pharm., 1831, p. 393. † Revue Ency., l, p. 619. ‡ Ibid.