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AND

THE ARTS.

APRIL, 1813.

ARTICLE I.

Experiments on the comparative strength of Men and Horses, applicable to the Movement of Machines. By M. Schulze*.

Those who have had occasion to construct machines intended to be moved by men or animals, are sufficiently aware how important it is to be acquainted with the quantity of power that can be attributed to either of them, in order to estimate with accuracy the effect which it is proposed to obtain from the machine. It is well known, that the arrangement of the whole depends entirely on the ratio of the velocity of the motive force to the resistance. This was the reason that long ago induced experimentalists to take the trouble of determining the strength as well as the velocity exerted by men and animals, when they are made to move machinery; and the results they obtained, which have been commonly made use of in computing the effect of machines, are, that men exert from twenty-seven to thirty pounds, with a velocity of from one and a half to two feet per second; and that a horse has about seven times more strength than a man, with a velocity of from four to six feet per second.

These are the data which we have been obliged to use when-For anlas of

* Memoirs of the Royal Academy of Sciences of Berlin for 1783.
Euler for determining the effects of machines moved with different velocities, &c., it became necessary to compute the effect of a machine moved by men or horses. It is evident that the force must be diminished when the velocity is increased, and vice versa: but we are not yet certain of the method of finding the ratio of the diminution or augmentation of this force to the velocity. Euler has given us two different formulæ to compute this ratio: but no one has hitherto attempted to verify by experiment which of them is to be preferred, although they differ very considerably from each other. If we put \( P \) for the absolute force which takes place when we simply consider equilibrium, \( C \) the absolute velocity which takes place when the man or animal moves freely, and without being overcome by the resistance, \( p \) the relative force, and \( c \) the corresponding velocity, we have by the first of these formulæ,

\[
b = P \left(1 - \frac{c}{C}\right)^2;
\]

whereas the second gives us

\[
p = P \left(1 - \frac{c^2}{C^2}\right).
\]

As I am obliged now more than ever to attend to a number of machines, and to compute their effect, it therefore concerns me very much to know exactly in what manner to estimate, compare, and fix the strength and velocity of men and animals, which are used for moving various machines, proper for different purposes.

With this view I made, with considerable care, the experiments I am now about to detail, which of course would have been very expensive, had I not had some facilities which other persons may not possess.

To make the experiments on human strength, I took promiscuously twenty men of different sizes and constitutions, whom I measured and weighed; the result of which is given in the following table:

<table>
<thead>
<tr>
<th>Order</th>
<th>Size:</th>
<th>Weight:</th>
<th>Order</th>
<th>Size:</th>
<th>Weight:</th>
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</thead>
<tbody>
<tr>
<td>1</td>
<td>5’ 3”</td>
<td>122</td>
<td>11</td>
<td>5’ 9”</td>
<td>132</td>
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<td>2</td>
<td>5’ 2”</td>
<td>134</td>
<td>12</td>
<td>5’ 1”</td>
<td>157</td>
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<tr>
<td>3</td>
<td>5’ 7”</td>
<td>165</td>
<td>13</td>
<td>5’ 3”</td>
<td>175</td>
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<tr>
<td>4</td>
<td>5’ 5”</td>
<td>131</td>
<td>14</td>
<td>5’ 4”</td>
<td>117</td>
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<tr>
<td>5</td>
<td>5’ 11”</td>
<td>177</td>
<td>15</td>
<td>5’ 10”</td>
<td>192</td>
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<tr>
<td>6</td>
<td>6’ 0”</td>
<td>158</td>
<td>16</td>
<td>5’ 0”</td>
<td>133</td>
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<tr>
<td>7</td>
<td>5’ 8”</td>
<td>180</td>
<td>17</td>
<td>4’ 11”</td>
<td>147</td>
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<tr>
<td>8</td>
<td>5’ 2”</td>
<td>117</td>
<td>18</td>
<td>5’ 3”</td>
<td>124</td>
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<td>9</td>
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<td>19</td>
<td>5’ 6”</td>
<td>163</td>
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<tr>
<td>10</td>
<td>5’ 0”</td>
<td>126</td>
<td>20</td>
<td>5’ 10”</td>
<td>181</td>
</tr>
</tbody>
</table>
STRENGTH OF MEN AND HORSES.

To find the strength that each of these men might exert to raise a weight vertically, I made the following experiments:

I took various weights, increasing by 10lbs. from 150lbs. up to 250lbs. All these weights were of lead, having circular and equal bases. To use them with success in the proposed experiments, I had at the same time a kind of bench made, in the middle of which was a hole of the same size as the base of my weights: this hole was shut by a circular cover, which effected this purpose when pressed against the bench, but at other times was kept at about the distance of a foot and a half above the bench, by means of a spring and some iron bars. To prevent the weight with which this cover was loaded during the experiment, from forcing down the cover lower than the level of the surface of the bench, I had several grooves made in the four iron bars, which sustained the cover at any height at which it might arrive by the pressure of the springs, as soon as the pressure of the weight ceased.

After having laid the 150lbs. on the cover, and the other weights in succession, increasing by 10lbs. up to 250lbs. I made the following experiments with the men whose size and weight are given above, by making them lift up the weights as vertically as possible all at once, and by observing the height to which they were able to lift them. The following table gives the heights observed for the different weights marked at the head of the table.

<table>
<thead>
<tr>
<th></th>
<th>150</th>
<th>160</th>
<th>190</th>
<th>200</th>
<th>210</th>
<th>220</th>
<th>230</th>
<th>240</th>
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<tbody>
<tr>
<td>1</td>
<td>7&quot; 9&quot;</td>
<td>6&quot; 4&quot;</td>
<td>4&quot; 11&quot;</td>
<td>4&quot; 11&quot;</td>
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<td>1&quot; 11&quot;</td>
<td>1&quot; 7&quot;</td>
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<tr>
<td>2</td>
<td>7 10</td>
<td>6 6</td>
<td>5 7</td>
<td>4 7</td>
<td>5 11</td>
<td>2 5</td>
<td>0 5</td>
<td>1&quot; 7&quot;</td>
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<tr>
<td>3</td>
<td>7 9 7 3 6 5 9 4 11</td>
<td>4 0 3 0</td>
<td>1&quot; 7&quot;</td>
<td>6&quot; 3&quot;</td>
<td>3 8</td>
<td>3 1</td>
<td>1&quot; 4&quot;</td>
<td>0 1</td>
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<tr>
<td>4</td>
<td>8 3 7 6 7 2 5 10</td>
<td>5 3</td>
<td>4 7</td>
<td>4 0</td>
<td>3 8</td>
<td>3 1</td>
<td>1&quot; 4&quot;</td>
<td>0 1</td>
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<tr>
<td>5 12</td>
<td>4 11</td>
<td>9 7 8 5</td>
<td>7 10</td>
<td>7 1 5 10</td>
<td>4 7</td>
<td>3 2</td>
<td>1 3</td>
<td>0 1</td>
</tr>
<tr>
<td>6 14</td>
<td>5 14</td>
<td>13 5 12 8</td>
<td>11 5</td>
<td>10 1 8 6</td>
<td>6 6</td>
<td>4 1</td>
<td>0 1</td>
<td>0 1</td>
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<td>11 11 3 10 5 9 3</td>
<td>8 1</td>
<td>6 9 5 3</td>
<td>3 8</td>
<td>1 11</td>
<td>0 2</td>
<td>0 1</td>
<td>0 1</td>
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<tr>
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<td>9 4 8 11</td>
<td>8 1 6 11 5 10</td>
<td>5 1 3 2</td>
<td>1 0</td>
<td></td>
<td></td>
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<tr>
<td>9 10</td>
<td>9 5 8 3 7 1 5 6</td>
<td>4 1</td>
<td>2 9 1 3</td>
<td>3 8</td>
<td>1 11</td>
<td>0 2</td>
<td>0 1</td>
<td>0 1</td>
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<tr>
<td>10 8 1 6 5 4 7 3 9 2 5 1 7 0 4</td>
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</table>

This table proves to us, that the size of the men employed has considerable influence on the height to which they severally brought the same weight. We find also by this, that the height diminishes in a much more...
considerable ratio than the weight increases; and we may therefore conclude, that it is advantageous to employ large men when it becomes necessary to draw vertically from below upwards; and, on the contrary, it is more advantageous to employ men of considerable weight, when it is required to lift up loads by means of a pulley, about which a cord passes, which the workmen draw in a vertical direction, from above downwards. To find the absolute strength of these men in a horizontal direction, I took the following method:

Having fixed over an open pit a brass pulley, extremely well made, of fifteen inches diameter, whose axis, made of well-polished steel, to diminish the friction, was three-fourths of an inch in diameter; I passed over this pulley a silk cord worked with care, to give it both the necessary strength and flexibility. One of the ends of this cord carried a hook to hang a weight to it, which hung vertically in the pit, whilst the other end was held by one of the twenty men, who, in the first order of the following experiments, made it pass above his shoulders; instead of which, in the second, he simply held it by his hands.

I had taken the precaution to construct this in such a manner, that the pulley might be raised or lowered at pleasure, in order to keep the end of the cord held by the man always in a horizontal direction, according as the man was tall or short, and exerted his strength in any given direction.

I had made the necessary arrangements, so as to be able to load successively the basin of a balance which I had attached to the hook at the end of the cord which descended into the pit, whilst the man who held the other end of the cord employed all his strength without advancing or retracting a single inch.

The following table gives the weights placed in the basin when the workmen were obliged to give up, having no longer sufficient strength to sustain the pressure occasioned by the weight. To proceed with certainty, I increased the weight each time by five pounds, beginning from 60, and intervals of time, having always precisely a space of ten seconds between them. The result of these observations, repeated several days in succession, is contained in the following table:

When
STRENGTH OF MEN AND HORSES.

When the cord passed over the shoulders of the workmen:

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</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>95</td>
<td>6</td>
<td>100</td>
<td>11</td>
<td>95</td>
<td>16</td>
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<td>7</td>
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<td>17</td>
<td>100</td>
</tr>
<tr>
<td>3</td>
<td>110</td>
<td>8</td>
<td>105</td>
<td>13</td>
<td>110</td>
<td>18</td>
<td>90</td>
</tr>
<tr>
<td>4</td>
<td>100</td>
<td>9</td>
<td>95</td>
<td>14</td>
<td>90</td>
<td>19</td>
<td>110</td>
</tr>
<tr>
<td>5</td>
<td>105</td>
<td>10</td>
<td>90</td>
<td>15</td>
<td>110</td>
<td>20</td>
<td>105</td>
</tr>
</tbody>
</table>

When the cord was simply held before the man:

<table>
<thead>
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<th></th>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>90</td>
<td>6</td>
<td>100</td>
<td>11</td>
<td>90</td>
<td>16</td>
<td>90</td>
</tr>
<tr>
<td>2</td>
<td>105</td>
<td>7</td>
<td>110</td>
<td>12</td>
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<td>17</td>
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<tr>
<td>3</td>
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<td>8</td>
<td>100</td>
<td>13</td>
<td>100</td>
<td>18</td>
<td>85</td>
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<tr>
<td>4</td>
<td>90</td>
<td>9</td>
<td>90</td>
<td>14</td>
<td>85</td>
<td>19</td>
<td>100</td>
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<tr>
<td>5</td>
<td>95</td>
<td>10</td>
<td>85</td>
<td>15</td>
<td>105</td>
<td>20</td>
<td>100</td>
</tr>
</tbody>
</table>

These two tables show, that men have less power in drawing a cord before them than when they make it pass over their shoulders: it shows us also that the largest men have not also the greatest strength to hold, or to draw in a horizontal direction by means of a cord. To obtain the absolute velocity of these twenty men, I proceeded as follows:

Having measured very exactly a distance of 12,000 Rhineland feet, in a plain nearly level, I caused these twenty men to march with a good pace, but without running, and so as to continue during the space of four or five hours. The following is the time employed in describing this space, with the velocity resulting from each of them.

<table>
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<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>40'18</td>
<td>4'94</td>
<td>8</td>
<td>40'9</td>
<td>4'99</td>
<td>15</td>
<td>30'17</td>
<td>3'51</td>
</tr>
<tr>
<td>2</td>
<td>41'12</td>
<td>4'85</td>
<td>9</td>
<td>40'20</td>
<td>4'96</td>
<td>16</td>
<td>41'26</td>
<td>4'82</td>
</tr>
<tr>
<td>3</td>
<td>39'8</td>
<td>5'55</td>
<td>10</td>
<td>40'51</td>
<td>4'90</td>
<td>17</td>
<td>42'25</td>
<td>4'71</td>
</tr>
<tr>
<td>4</td>
<td>39'40</td>
<td>5'04</td>
<td>11</td>
<td>56'17</td>
<td>5'51</td>
<td>13</td>
<td>40'19</td>
<td>4'98</td>
</tr>
<tr>
<td>5</td>
<td>34'19</td>
<td>5'83</td>
<td>12</td>
<td>38'11</td>
<td>5'24</td>
<td>19</td>
<td>39'57</td>
<td>5'01</td>
</tr>
<tr>
<td>6</td>
<td>35'11</td>
<td>5'68</td>
<td>13</td>
<td>38'5</td>
<td>5'25</td>
<td>20</td>
<td>37'51</td>
<td>5'29</td>
</tr>
<tr>
<td>7</td>
<td>38'7</td>
<td>5'23</td>
<td>14</td>
<td>37'1</td>
<td>5'40</td>
<td></td>
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</tbody>
</table>

It is necessary to mention, with regard to these experiments, that I took care to place, at certain distances, persons in whom I could place confidence, in order to observe whether these men marched uniformly and sufficiently quick without running.

Having
Having thus obtained, not only the absolute force, but the absolute velocity also, of several men, I took the following method to determine their relative force.

I had made use of a machine composed of two large cylinders of very hard marble, which turned round a vertical cylinder of wood, and moved by a horse, which described in its march a circle of ten Rhineland feet. This machine appeared to me the most proper to make the following experiments, which serve to determine the relative strength that the men had employed to move this machine, and which I use hereafter to determine which of Euler's two formulæ ought to be preferred.

To obtain this relative force, I took here the same pulley which served me in the preceding experiments, by applying a cord to the vertical cylinder of wood, and attaching to the other end of this cord, which entered into an open pit, a sufficient weight to give successively to the machine different velocities.

Having applied in this manner a weight of 215 lbs. the machine acquired a motion which, after being reduced to an uniform motion, taking into account the acceleration of the weight of the friction, and of the stiffness of the cord, gave 2.41 feet velocity; and having applied in the same manner a weight of 220 lbs. the resulting uniform motion gave a velocity of 2.47 feet. I only mention these two limits, because they serve as a comparison with what immediately follows. I began these experiments with a weight of 100 lbs. and increased it by five every time, from that number up to 400 lbs.

I made this machine move by the seven first of my workmen, placing them in such a way, that their direction remained almost always perpendicular to the arm on which was attached the cord which passed over their shoulders in an almost horizontal direction.

Thus situated, they made 281 turns with this machine in two hours, which gave for their relative velocity \( c = 2.45 \) feet per second. We have also the absolute force, or \( P \), from these seven men by the above table \( = 730 \) lbs. and their absolute velocity, or \( C = 5.30 \) feet.

Therefore, by substituting these values in the first formula, we
we find the relative force $p = 205$ lbs. which agrees very well
with what we have just found above.

If instead of this first formula, the second be taken, it gives
$p = 153$ lbs. which is far too little.

By this it is evident, that the first of Euler's two formulæ is
to be preferred in all respects. I have also made a great num-
ber of combinations, and I almost always found the same
effect.

Dividing the 205 lbs. which we have just found, by seven,
the number of workmen, we get 29 lbs. for the relative force,
with 2.45 feet relative velocity for each man, which is rather
more than the values commonly adopted in the computation
of machinery. A number of other observations on different
machines, which I intend to relate another time, have given
me the same result; that is to say, we must value the mean veloc.
per
human strength at 29 or 30, with a velocity of 2 1/4 feet per
second.

To obtain the ratio of the strength of a horse to that of a
man, I had the same machine moved by a horse, without altering
any thing; and I found by ten different horses which I
used successively, that a horse makes 603 turns in two hours
instead of 281; therefore, by supposing the static motion of
a horse seven times greater than that of a man, we find that
the former has 5.3 feet per second of velocity.

By this it is evident, that the effect of a horse is fourteen
Horses exceed
men 14 times
in drawing.
times greater than that of a man, or, which amounts to the
same thing, fourteen men must be used instead of one horse.
Hence it appears, that it is much more advantageous to employ
horses than men in moving machines, if other reasons did not
require us to prefer men.

I have also made a number of other interesting observations
on horses and oxen, which are likewise used in moving ma-
chines; but as I am now waiting for observations of this kind,
which other persons are making according to my plan, I shall
reserve them for another memoir.
II.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. 

By Professor J. Berzelius.

(Continued from p. 166.)

After this general review of the changes which appear to be necessary in the theory of chemistry, I shall have the honour to present to the academy the results of some experiments upon the combinations of various metals with oxygen and with sulphur, made with the intention partly of determining their composition with greater precision, in order to refute certain incorrect notions respecting their nature, and partly to ascertain the electro-chemical nature of those metals, as well as the place they ought to occupy in the system among the other combustibles. Much remains yet to be done on this subject, because the field to be explored is so extensive, that each individual step appears relatively of small magnitude.

My researches have been made upon the oxides of tin, tellurium, gold, platina, palladium, lead, zinc, and manganese; and at my request the following metallic oxides have been analysed, namely, those of cerium by M. de Hisinger; those of nickel and cobalt by M. Rathoff; that of bismuth by M. Lagerhielm; and those of mercury by M. Sefftroud; and these chemists solicit the honour to publish their works in the Memoirs of the Academy.

Ammonium? I should also have wished to add to these experiments that of the production of an amalgam of ammonium produced an anhydrous ammoniacal salt; and though in my experiments on that subject, an amalgam of kalium has produced an amalgam of ammonium in the subcarbonate of ammonium, prepared with the carbonic acid gas and dried ammoniacal gas, I shall not venture to present the same to the Accademy as a well-determined result, because I have not yet had an opportunity of examining to what degree I may have succeeded in operating with materials perfectly deprived of water. It is,
Nevertheless, clear, that the success of an experiment of this nature would be decisive as to the nature of ammonium.

I. Concerning the Oxides of Antimonium.

Notwithstanding the labours of chemists have, perhaps, been more frequently employed upon this metal than upon any other, we have hitherto possessed but few data respecting the number and the nature of its oxides; and the information given in elementary works is often contradictory the one to the other. The chemists who have operated the most successfully upon these oxides are, MM. Proust, Thenard, and Bucholz. Thenard, guided by the principle of unlimited combinations advanced by the illustrious Berthollet, found that antimony produced six different oxides, that is to say, one black, one chestnut brown, one greyish white and fusible, one white and not fusible, one orange, and one yellow, in which the quantity of oxygen differed no more than one or two per cent. Proust, on the contrary, found no more than two oxides, of which he has determined the composition with considerable accuracy; and Bucholz, who repeated the experiments of Thenard with the intention of examining them, could find only two degrees of oxidation precisely the same which Proust had described. The author have found as many as four, which it is incontestible that has found four. Thenard saw, though he did not well distinguish them from the mechanical mixtures of different degrees of oxidation, which are very frequently obtained, and though he has given no other distinctive character than the colour, which is so often fallacious.

I must observe, that the antimony employed in all my experiments was purified in the following manner: I reduced it to powder, and mixed it with the white oxide of antimony, which I then exposed to fire till the mixture was fused. If the fused oxide, which flowed above the metallic bottom, was found to be coloured after cooling, I repeated the same operation.

1. Suboxidum stibicum is formed when the metal is exposed for a long time to the action of a humid and warm atmosphere. It forms an extremely thin coat of a blackish grey colour, which prevents all further action of the atmosphere upon the parts so covered. In order to obtain this suboxide in larger
larger quantities, a piece of antimony, fused in a tube of glass, to give it a convenient form, was employed as the positive conductor in the decomposition of pure water by a voltaic pile of fifty pair. The antimony produced oxygen gas in extremely small bubbles, but, at the same time, it became covered with a grey pellicle which became almost black when the metal was dried in the air. That part of the antimony which was covered by the cork had preserved its metallic brilliancy, and the difference between the suboxided surface and clear metal, was very marked. But as even in this experiment the suboxide did not appear visibly to increase as soon as the pellicle was formed, I employed antimony reduced to powder as the positive conductor, and touched at the bottom by the point of the platina wire. This point produced oxygen gas, which, from time to time, rose through the powder, and this last began to be covered with a lighter and bluish powder. After some days this powder had increased so much as to be capable of being separated from the metal by means of levigation. This becomes nearly black by drying, and, when rubbed with a polished bloodstone, did not give the smallest trace of metallic brilliancy. When thrown into muriatic acid, this fluid emitted a slight smell of hydrogen, and a few instants afterwards, metallic particles were seen swimming in the acid, and were more easily precipitated by soda than the powder before the action of the acid. The suboxide of antimony, therefore, possesses a property common to most of the suboxides, of being decomposed by the action of acids, by concentrating the oxygen upon part of the metal to produce a base combinable with the acid, and reducing the other part to the metallic state.

I have not been able to produce this suboxide in a sufficient quantity to analyse it; but I shall hereafter shew how it is possible to find its composition by calculation with some degree of probability.

2. Oxidum stibiosum. The characters of this are very well known from the experiments of Proust and Thenard. It has a dirty white colour, is slightly soluble in water, is easily fused by a cherry red heat into a yellowish fluid. The mass, when cold, is crystallized in the manner of asbestos, but the groups of crystals cross in every direction, and it is not difficult to break their continuity.
a. In order to determine the quantities in the composition of this oxide, I digested ten grammes of antimony with nitric acid until they were completely corroded. I then mixed the liquid with much water, and washed the precipitate with water until that fluid came off without being capable of reddening tournesol. The oxide thus obtained weighed, when dry, 12.005 grammes; and, in order to drive off all water, I exposed it in a glass capsule to an heat not as high as ignition, but it took fire on a sudden, and continued to burn like fungus (or tinder) at the same time subliming in a thick white smoke, of which part was condensed on the sides of the glass. The powder, by this means, became as white as snow, and weighed 12.3 grammes.

b. As this analytical method did not appear very good, I mixed in a small glass retort ten grammes of muriatic hydrargyrificus (corrosive sublimate) in powder, with twenty grammes of powdered antimony. The atmospheric air of the retort having been expelled by hydrogen gas, and a small receiver filled with hydrogen gas being also applied, I gently heated the mixture until the muriatic stibious (butter of antimony) came over, and lastly I heated the body of the retort red hot, to distil over the mercury amalgamated with that part of the antimony which had been added in excess. The quantity of 16.98 grammes of antimony remained in the retort. Consequently ten grammes of muriatic hydrargyrificus had been decomposed by 3.02 grammes of antimony. But 100 parts of this salt contain 5.75 of oxygen combinable with other metals. 100 parts of antimony had, therefore, been combined with thirteen parts of oxygen.

I repeated this experiment several times without having ever obtained results perfectly equal, but varying, as for example, 19.35 or 19.68 parts of oxygen for 100 of antimony. The causes of error in this experiment may be several. For instance, it is possible that the mercury may be so adherent to the antimony as not to be separated but at a temperature which would also carry over a little of the latter; and it is also possible that a small quantity of mercurial muriate may arise before decomposition along with the vapors of the muriate of antimony. It is, therefore, probable, that these experiments may have given the quantity of oxygen rather too great.
Indirect method of deducing the components of an oxide.

Sulphuret of antimony was made by heating antimony and cinnabar.

Cause of inaccuracy.

Inference that 18·6 parts oxygen and 100 antimony form this oxide.

4. White oxide of antimony*. (a) Antim. oxidized by nitric acid, and ignited.

(b.) Antim. dissolved in nitromuriatic acid, and precipitated by water and ignited.

Deduction, 100 antimon. and about 26 oxygen, which is nearly 1½ times the oxygen in the second oxide.

In my essays on determinate proportions, I have often ascertained the composition of an oxide, which was difficult to analyse with exactness, by analysing the sulphuret of the same metal, and calculating the composition of the oxide from this analysis. I endeavoured to do this in the present case.

3. Sulphuretum stibii. I mixed 100 parts of pulverized antimony with 500 parts of very pure cinnabar, and I exposed the mixture to heat in a retort. When the cinnabar appeared to be entirely decomposed, and the excess driven out of the bulb of the retort, I left the sulphuret of antimony in fusion for several minutes at a cherry red heat, and then took the retort from the fire. The sulphuret of antimony weighed 137·3 grs. In the upper part of the retort I found a small quantity of a reddish substance sublimed. I supposed it to be cinnabar not completely expelled, and heated the sulphuret again in the retort till it boiled. The red substance was increased, and I at last discovered that it was crocus of antimony produced by the access of air. As the sulphuret of antimony is slightly volatile in a very elevated temperature, the result of this experiment likewise is not very exact; but it may, however, be inferred, that the quantity of oxygen in the oxidium stibiosum cannot be less than 18·6 for one hundred parts of metal.

4. White oxide of antimony*. (a) Two parts of pulverized antimony oxidized (in a phial carefully weighed) by pure nitric acid, and the oxidized mass strongly ignited in the phial, produced in different experiments 125·8, 126·13, and 127·8 parts of white oxide.

b. 100 parts of pulverized antimony first dissolved in nitromuriatic acid, and then precipitated and well washed with water, produced a quantity of oxide of antimony, which, after strong ignition, weighed 126·56 parts. The acid liquor, after dilution, contained no more oxide, and did not become turbid by saturation with an alkali. The experiments appear, therefore, to prove, that this oxide does not contain less than 25·8 nor more than 27·8 of oxygen to 100 parts of metal. We see, therefore, that this oxide must contain 1½ times as much oxygen.

* I shall hereafter explain why I do not here use the words oxidum stibium.—B.
gen as the preceding degree, though the exact number was not ascertained.

In order to obtain a more determinate result by another process, I endeavoured to reduce the white oxide at the first degree of oxidation by means of metallic antimony. I, therefore, mixed the metal in extremely fine powder, with less of the oxide than would have been required to oxide the metal. I introduced the mixture into a small phial, of which I drew out the neck into the form of a capillary tube. The body was bedded in sand in a small crucible, and exposed to a sufficiently strong fire to make it red-hot; and, at the moment when the matter entered into fusion, I hermetically closed the end of the capillary neck, by melting the extremity, and I left the mixture in that heat for half an hour. Three grammes of the white oxide of antimony had oxidized 0.323 grs. of metallic antimony, and afforded a fusible oxide. I reduced this again to powder, and mixed it with powdered antimony, after which I fused it in a similar phial; but in its present state it was capable of dissolving only a small quantity of antimony, which would have been correspondent with 0.03 grs. of antimony upon the whole quantity of oxide. The fused oxide which I had obtained by this operation, was of a pearl colour, its fracture crystalline, granulated, and very compact; it was extremely coherent, and difficult to break, and all its external properties proved that it was not a pure stibious oxide. I repeated this experiment several times, and always found that the white oxide, fused with metallic antimony, dissolved one-third more than it before contained; that is to say, that 100 parts of the oxide can oxide an addition of 26 parts of the metal; and if we attend to this result, which is determined with the utmost possible accuracy, we shall find that the fusible oxide produced, of which the external characters are so different from those of the oxidum stibiosum, cannot be the same as this last; but that it must be a combination of the oxidum stibiosum with the white oxide in such proportion that the oxygen of the former is double that of the latter. Such combinations between oxides of different degrees of the same metal, are not very rare, though they have not hitherto been much attended to, or else have been considered as different degrees of oxidation of the metal. I reduced the oxide into powder, and
and digested it with the surturate of potash: the stibious oxide was dissolved, while the white oxide was separated in the form of an extremely white and voluminous powder.

In order to compare the quantity of oxygen in the white oxide with the quantity of sulphur which can combine with the metal it contains, I mixed 100 parts of white oxide with 100 parts of sulphur, and heated them together in a small phial exactly weighed, which had a narrow neck, and of which the aperture was closed with a stopper of charcoal. The heat employed was at first very gentle, until the disengagement of sulphurous acid had ceased, after which I heated the phial amidst the burning charcoal until the uncombined sulphur was entirely dissipated, and the buton of sulphuret remained in fusion at the bottom of the phial. It weighed 107.25 p. Although the volatility of the sulphuret of antimony could not be perfectly obviated in this experiment, I have reason to think, that the length and smallness of the neck of the phial prevented any loss, and thence especially as the mass was never so much heated as to put it into a state of ebullition.

(To be continued.)

III.


DEAR SIR,

When I say that I have been greatly surprised to see in the second part of the Philosophical Transactions for 1812, Don Rodriguez's animadversions upon part of the English

* I never operated upon a substance which in general afforded me results so various as antimony and its oxides. In order to ascertain whether the oxides, which are often obtained with less oxygen than they ought to contain, have been volatilized with the acid employed for their oxidation, I heated such an oxide with sulphur, and converted it into sulphuret. 100 p. of antimony produced 128.5 of yellow oxide, which by ignition left 125.8 of white oxide, and these produced with sulphur 137.5 of sulphuret. It appears, therefore, that these oxides contain combinations of different degrees of oxidation, which prevent the satisfaction with oxygen.
lish Trigonometrical Survey, I conjecture that I am merely
describing a feeling which has been more or less experienced
by every man of science in the kingdom. The publication of
an attempt by a foreigner to cast discredit upon a great national
undertaking, in the transactions of the most eminent philo-
sophical institution of that nation, the Royal Society,—that is, in
a work which learned men on the continent contemplate as a
fair picture of the science and genius of England, is, I believe,
a thing unprecedented in the history of literature. If the
great work which Don Rodriguez has taken upon himself to
examine, had been really reprehensible, it would still have been
extraordinary that he should have been permitted to give his
censures currency in such a vehicle; but how much more ex-
traordinary must it be thought, if, on inquiry, it shall appear,
that his strictures are causeless, and therefore unjust. This is
an inquiry which every man of competent information, who has
at heart the honour of his country, has a right to institute;
and, however unpleasant the undertaking may, in some re-
spects be, I enter upon it without delay, because Colonel
Mudge, whose reputation is so deeply implicated in this busi-
ness, is at present prevented from giving Don Rodriguez's
paper that decided and complete refutation which it will here-
after receive at his hands, and because his silence, though un-
avoidable, may be construed into defeat.

Impressed by these considerations, I propose, in this com-
munication to show, that the observations of this ingenious
foreigner are, on all his main positions, unfounded: and, al-
though the matter under investigation is, in general, so nearly
elementary that any man of moderate scientific attainments
might safely rest the truth of his assertions upon his own char-
acter and their intrinsic evidence; yet, lest it should be ap-
prehended that, on this occasion, my judgment may be warped
either by strong national feeling, or by private attachment, I
shall fortify my positions, as I go along, by such authorities as
neither Don Rodriguez, nor any other person, will be inclined
to question.

Before I proceed to the points which Don Rodriguez selects
as the basis of his animadversions, it may not be thought im-
proper if I briefly advert to what appears his main, if not his
sole, object, in making those animadversions at all. I shall
not, I hope, be deemed uncandid, if I say, that to me this object appears to be no other than the depression of English (and perhaps other) ingenuity and exertion, in order to the undue exaltation of the French scientific character. To this end, as it would seem, (for to what other purpose can it be?) we are told, that in consequence of "the general impulse which the human mind received" from the French revolution, the members of their Academy of Sciences "invented new instruments, new methods, new formulæ," for the purpose of ascertaining the figure of the earth, &c. and commenced "an important undertaking, almost the whole of which consisted of something new in science." I have no wish to depreciate the value of the discoveries and improvements of the French mathematicians; yet surely I may affirm, that much had been done with respect to the grand topic in question, long before the French revolution. Did not Euler invent "new methods and new formulæ" for this express purpose, and publish them so long back as the year 1753, in the Berlin Memoirs? Did not Dionis du Sejour much improve this branch of analytical theory? Did not Professor Playfair solve the general problem in all its useful varieties in the Edinburgh Transactions, before the publication of Delambre's investigations? Did not General Roy, and the subsequent English measurers, publish ingenious formulæ in the Philosophical Transactions, although Don Rodriguez insinuates, that their methods are kept back? And, with respect to actual admeasurements, might not the Don have learnt from the Philosophical Transactions (see vols. 75, 77, 80, &c.) that government surveys were commenced in Scotland so long back as 1745, by Lieutenant-General Watson; that in 1775 the work was continued; that in 1783 an authorized committee or deputation of the mathematical philosophers of England and France met at Dover to concert the best means of carrying a series of triangles from Greenwich to Paris; that the work was soon after pursued by the appointed persons in both countries; and that, from that period it has almost regularly proceeded in England, whatever interruptions it may have experienced in France? How, then, can a writer insert in the Philosophical Transactions, where evidence to the contrary abounds, a paper from which all who are unacquainted with the history of this important class of operations, would conclude
conclude, that they originated in the determination of the French to "establish a new system of weights and measures."

To the same end, apparently, tends the Don's assertion, that "the Swedish Academy of Sciences, encouraged by the success of the operations conducted in France, sent also three of its members into Lapland to verify their former measurement."

For the natural tendency of this statement is to produce the belief, that the recent operations of the Swedish philosophers were in humble imitation of the French, and that they were undertaken for the purpose of verifying, or of correcting, their own former admeasurement; in both which respects the colouring given is widely different from the truth. The Lapland measure in 1736 was not conducted by Swedish, but by French academicians; and the correction of it was proposed long before the French revolution. The following are the true circumstances of the case, as I received them from a learned Swede. Melanderholm, the venerable president of the Stockholm academy, had almost from his youth doubted the accuracy of the operations of 1736, and sought anxiously for an opportunity of repeating them; but waited many years before he could avail himself of a favourable conjuncture of circumstance, although latterly he had found in M. Svanberg, a young man of great talent and activity, to conduct the operative part. After hearing of the new measure of a degree by MM. Delambre and Mechain, he wrote to some of the French mathematicians on the subject, but with no intention of soliciting them to visit Lapland. Soon after this, Buonaparte, at the suggestion of the then national institute, wrote a letter personally to the late king of Sweden, requesting permission for some members of that body to proceed to Lapland, in order to determine an arc of the meridian. That high-spirited young monarch replied, that he would consult his own Academy of Sciences at Stockholm, whether such an operation was desirable for the interests of science; and if they were of that opinion, he had no doubt he could find Swedish mathematicians competent to the undertaking. Hence MM. Svanberg, Qveribom, Holmquist, and Palander, were appointed to examine and repeat the measure of the French academicians; and this is what Don Rodriguez terms the expedition of three of the Swedish academicians "to Lapland to verify their former measurement."

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Col. M. commended by Don. R. merely as a skilful observer.

With the same spirit, it is natural to suspect, Don Rodriguez speaks of Colonel Mudge as "a skilful observer," and merely such; adding, that "one cannot but admire the beauty and perfection of the instruments employed" by him: while, when he characterises the labours of the French measurers, he assures us they "merit the highest degree of confidence," and, "by the sanction of such an union of talents, give such a degree of credit and authenticity to their conclusions, as could scarcely be acquired by other means." I shall not animadvert upon this invidious contrast; but simply remark here, that the Don adopts a strange method of verifying his positions. He admits, that Colonel Mudge is a skilful observer, who knows very well how to employ his instruments; and, that there may remain no doubt on that head, publishes a long paper to prove, or at least to show it probable, that he has made a mistake of 4 1/2 seconds in the determination of a zenith-distance. This animadverter has, as he assures us, gone through all the Colonel's computations by different processes, and found them correct, or only evincing very trifling discrepancies, such as may naturally arise from the diversity of methods; yet he cannot find in his heart to drop a single word of commendation on him as a computer or as an investigator.

The preceding remarks will suffice, I apprehend, to render manifest the probable object of Don Rodriguez's paper. I shall now proceed to enquire how far the reasons assigned by this gentlemen bear him out in his attempt to throw suspicion upon the operations of Colonel Mudge, in measuring an arc of the meridian. The Don's paper, it is true, is rather desultory and unconnected; but, I trust I shall neither misrepresent him, nor do injustice to his arguments, by endeavouring to reduce them to the following order:

1. Colonel Mudge's observations must be wrong somewhere, because his results do not correspond with those of the French measurers. This is not positively affirmed, but everywhere strongly implied: for Don R. assumes his value of the radius of the earth's equator from the French measurements and computations; and he takes it for granted, that the fraction exhibiting the ratio of the difference of the earth's axes to the major axis, technically termed the compression, lies somewhere between those limits, ( and ) which a superficial ob-
server would adopt as most suitable to the French operations. Such assumptions, by the way, are neither consistent with fair criticism nor with sound logic: for the grand object in measuring arcs of meridians is to determine the ratio of the earth’s axes; and when, in the course of any such admeasurements, avowedly remarkable anomalies arise, it is a mere petito principio to conclude that there must be some error in the astronomical observations, because irregularities as great or greater than those which the operations indicated, result from computations resting upon a gratuitously assumed ratio.

But some of the French operations at home, compared with those at Peru, give about \( \frac{1}{4} \) for the compression. Be it so. That is no reason why any such ratio should be adopted, as the test by which to try the accuracy of English observations. Don Rodriguez himself, when applying the same test to the French meridian, thereby detects irregularities, and great ones too; yet does not whisper the gentlest hint that they were occasioned by inaccurate observations. Why not? Because M. Mechain “handled instruments with great delicacy, and was possessed of peculiar talents for this species of observation” So that a gratuitous assumption should suffice to render English observations doubtful, while it leaves the accuracy of French ones unimpeached. To me it appears that a candid critic would, in analogous circumstances, make analogous inferences; and not sift one class of results to the bottom, while he satisfies himself with merely glancing at the surface of the other class. Had he examined the French measures a little more minutely, he would, instead of adopting them as his standard, have found that they exhibit far too great irregularities to be entitled to that honour. Taking the results of the operations of Delambre and Mechain, as subdivided naturally by the assumed stations at Dunkirk, the Pantheon at Paris, Evaux, Carcassone, and Montjouy, and applying to them the principle developed by Legendre, in which, “the sum of the squares of the errors is made a minimum,” the requisite compression is \( \frac{1}{4} \); and even then the deviations from what the theory would require are, at Dunkirk—2°.23, that is, nearly 2\( \frac{1}{4} \) decimal seconds; at the Pantheon, +5°.63; at Evaux, —4°.79; and at Carcassone,

\[
\text{S2} + 1^{\circ}.3.
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* Biot. Astronomic Physique, tom. i. p. 159.
+ 1° 34. Here the compression which agrees best with the observations is more than double what it ought to be. If a medium compression had been chosen, the errors at the several stations would have deviated still farther from the probable errors of observation. Don Rodriguez will find this confirmed by Puissant, Géodésie, pa. 137. 141, and by Laplace, Exposition du Système du Monde, Liv. i. ch. 12. After he has duly reflected upon the deductions of those philosophers, he will, perhaps, be convinced, that he has been rather precipitate in taking the French operations as a standard.

But, secondly, this writer infers that there must be some error in Colonel Mudge's observations, because they tend to shew that the terrestrial spheroid is very irregular. All the measurements which have been hitherto made in the northern hemisphere, are (he tells us) extremely satisfactory by their agreement, and give us great reason to presume, that the general level of the earth's surface is elliptical and very regularly so. There would not have remained the smallest doubt respecting the earth being flattened at the poles, but for the measurement performed in England. But this measure alone would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part (the author means those parts) than at the equator, or at least, that its surface is not that of a regular solid. The degrees, in fact, increase as the latitudes diminish, which, says Don Rodriguez, excites a suspicion of some incorrectness in the observations themselves; whereas, the only fair inference is, that an insular situation is very ill fitted to promote the determination of the figure of the earth. Let us see, however, how satisfactory former measures have been by their agreement, and how completely they prove that the earth's surface is very regularly elliptical. Lacaille's degree in lat. 45° N. compared with Bouguer's at the equator, gives for the compression $\frac{1}{1\%}$. The degree in Maryland, with Bouguer's equatorial, gives $\frac{1}{5\%}$. The Spanish degree at the equator, with the French degree lat. 45°, gives $\frac{1}{6\%}$. Boscovich's Italian degree, lat. 43°, compared with Bouguer's at the equator, gives $\frac{1}{7\%}$. Bishop Horsley, by a geometrical mean of twelve different ellipticities, obtains $\frac{1}{8\%}$. Boscovich, taking a mean from all the measures of degrees, as
as to make the positive and negative errors equal, obtains \( \frac{1}{2} \). Lalande, by comparing Father Leisganig's degrees in Germany with eight others in different latitudes, gets \( \frac{1}{3} \). And the recent measures in France give, as we have seen, \( \frac{1}{4} \). Such is a summary of the evidence from which it is to be concluded that the earth is "elliptical," even "very regularly so." General Roy, who had got a habit, not very uncommon among scientific Englishmen, of deducing reasonable conclusions from anomalous appearances, and not twisting them to suit a fanciful hypothesis; assumed seven different spheroids of varying ratios between \( \frac{1}{2} \) and \( \frac{3}{4} \), and, on finding that none of them corresponded so uniformly as might be wished, with the operations in different latitudes, made these inferences: "Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not any way correspond with the measured portions of the surface of the earth." "Hence it is that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has no regular figure, that is, not such as would be generated by the revolution of a curve around its axis." And again, after specifying some other facts, "from all which we may conclude, that the earth is not an ellipsoid."

Nor is this opinion peculiar to General Roy, it is common, I believe, to all who have contemplated the subject, except Don Rodriguez. Thus, Puissant, at p. 187, of his Geodesie, says "La comparaison des divers degrés mesurés à l'équateur, en déductions France, en Pensylvanie, etc. donne lieu à décider que les méridiens sont différents entre eux et n'ont pas la forme elliptique." And at p. 222, "D'on l'on doit conclure que la terre n'a point la forme régulière que l'on serait tenté de lui attribuer." To the same purpose writes Laplace, at p. 56 of his "Exposition": "Les Laplace's degrés du nord et de France donnent \( \frac{1}{4} \) pour l'ellipticité de la terre, que les degrés de France et de l'équateur, donnent égale à \( \frac{1}{3} \), il paraît donc que la terre est sensiblement différente d'un ellipsoïde. Il y a même lieu de croire qu'elle n'est pas un solide de révolution, et que ses deux hémisphères ne sont pas semblables de chaque côté de l'équateur.

It is curious, however, to observe that, notwithstanding this extreme want of uniformity, in the results furnished by terres-
trial admeasurements, those which are deduced from astronomical theory, and the oscillations of pendulums, correspond very nearly. Thus, Laplace's deduction of the compression from the lengths of pendulums in different latitudes, is \( \tau \gamma \), (See Puissant, Topographie, &c. p. 60.) Clairault's well known modification of Newton's theorem, derived from the diminution of gravity, gives \( \tau_{1/3} \). The phenomena of the precession of the equinoxes and the nutation of the earth's axes, give \( \gamma^{1/4} \) for the maximum limit. A lunar inequality in longitude depending upon the earth's ellipticity, and expressed by \( -20'' \cdot 987 \sin \Omega \) of the moon in longitude, requires the compression to be between \( \frac{1}{3} \gamma \) and \( \frac{1}{6} \gamma \), but nearest the latter limit. And a lunar inequality in latitude, depending also on the compression, and expressed by \( -24'' \cdot 6914 \sin \phi \), requires the compression to be between \( \frac{1}{3} \gamma \) and \( \frac{1}{6} \gamma \), still leaning to the latter limit. So that the ratio of the earth's axes, as deducible from these independent theoretical considerations, lies within much narrower limits than we can get in any other way. But this does not affect the truth of the preceding remarks. It serves principally to shew, that whatever may have been the derangements of the terrestrial spheroid since its original formation, they are not such as have differently affected the several phenomena occasioned by its aggregate attraction: while a very slight consideration of the effects of the deluge, of earthquakes, of volcanic operations, of extensive dislocations of strata, &c. may serve to convince us, that, however regular the earth might once have been in its general shape, there is now no reason to expect that "very regular" surface from which Don Rodriguez persuades himself, there ought to be no essential deviation.

3. Don Rodriguez is farther confirmed in his opinion, that there must be an error in the observations, especially at Arbury Hill, of "nearly 5 seconds," because he thinks no such anomaly as that can fairly be ascribed to the effect of local attractions. He does not deny "that irregularities of the earth and local attractions may occasion considerable discrepancies," yet he does not believe they can ever produce a deviation of the magnitude just specified. Here again he is at war with the decisions, I believe, of all preceding philosophers who have
directed their attention to this subject. There are, obviously, three causes which may jointly or separately occasion a deflection of the plumb-line from the true perpendicular to the earth's surface; namely, an insular situation, the attraction of mountains, and strata of unequal density beneath the surface; and either of these may be productive of considerable effects.

To arrive in the easiest manner at an estimate of the effect upon a plumb-line arising from observations made in an insular situation, let Don Rodriguez imagine the simple case of a triangular island so posited on the surface of an aqueous spheroid, that a meridian shall run along from its vertex, directed northward to the middle of its base: he will perceive that, in such a case, as an observer proceeded from the south towards the north, there would be a constant variation in the deflection of the plumb-line; in such manner, that there would be only one point on the meridian, where the attractions occasioned by the island itself should be so counterpoised and adjusted, that the true and observed vertical lines should correspond. Pursuing this hypothesis, with the requisite modifications for a neighbouring continent on the south, and an immense ocean north, he will find that the singular order exhibited by the English estimates of degrees, though an unexpected, is by no means an unnatural, consequence of our insular situation. Dr. Hutton has treated this very point with his usual perspicuity, in a valuable note at page 198, vol. ii. New Abridgment of the Philosophical Transactions, published in 1803. That note is too long to be copied into this place; I shall, therefore, merely transcribe the Doctor's concluding inference: "Hence also it follows, that insular situations must be worst of any; having the plumb-line deviating to the north at the south end of the line, to the south at the north end, to the east at the west side, and to the west at the east side; thus producing errors in all observed latitudes and longitudes."

Laplace, most probably alludes to this kind of effect, at p. 59, ditto. "Exposition," where he speaks of the much more extensive attractions than those of mountains, of which the effect is sensible in Italy, England, &c.

That the deflections of the plumb-line, and the consequent estimate of the lengths of degrees, must be greatly affected by hills and valleys, is also very manifest. Professor Playfair, after
Playfair, on the after describing the irregularities thus occasioned in the degree of attraction of hills, &c. — at Turnn, adds, "there are, no doubt, situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian, infinite, or even negative." See Edinburgh Transactions, v. p. 5. And Dr. Maskelyne, after treating of Mason and Dixon's degree in North America, says, "Mr. Henry Cavendish having investigated several rules for finding the attraction of the inequalities of the earth, has, upon probable suppositions of the distance and height of the Allegany mountains, from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be so produced in the length of the degree; and finds that it may have been diminished by 60 or 100 toises by these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be very sensibly affected by the attraction of hills, and defect of the attraction in the Mediterranean Sea and Indian Ocean." Phil. Trans. vol. lvi. ii. New Abridgment, vol. xii. p. 578.

With respect to the third cause of irregularity, Puissant, Géodésie, p. 137, remarks, that "anomalies in the latitudes, are, doubtless, produced by local attractions which change the direction of the apparent vertical." And Professor Playfair, in the excellent memoir I have just quoted, (a memoir, it should be recollected, which was written five years before the remarkable anomalies in the English measures were known) affirms, that "from suppositions no way improbable, concerning the density and extent of masses of varying strata beneath the surface, he has found, that the errors thus produced, may easily amount to ten or twelve seconds." "This cause of error, (as he justly remarks) is formidable, not only because it may go to a great extent, but because there is not any visible mark by which its existence may always be distinguished."

Here, then, are three sources of deflection from the true plumb-line, neither of which is correctly appreciable in all circumstances, yet of which each may be not only perceptible, but important; and the concurrent effect of all may, doubtless, be very considerable. Yet, Don Rodriguez is unwilling to attribute a deviation of 4 or 5 seconds, to any, or all, of these causes.
4. This writer infers, that mistakes must have occurred in the observations, because the sum of other "errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured; but in the English measurement we find exactly the reverse of this." Here he assumes the principle proposed by Boscovich, but condemned by Laplace, for a reason thus briefly assigned by Puissant:—"La solution donnée d'abord par Boscovich est vicieuse en ce qu'elle est fondée sur une hypothèse inadmissible, savoir, que les erreurs dans le mesure des arcs du méridien sont proportionnelles à leurs longueurs."

5. He concludes that there must be "an error of some seconds in the observations of the fixed stars," because "the results of the observations made on different stars, differ no less than 4 seconds from each other." Now, what are the facts on which this inference rests? Simply these: that the only two stars which indicate any such difference in the whole series of observations, are μ Draconis and ζ Ursae; that they give a difference of 4" 19, not in the amplitude of the arc between Dunrose and Arbury Hill, but of that between Dunrose and Clifton; and that, whether these two stars be rejected, or retained with the other fifteen employed in finding that amplitude, they will not occasion a difference of a quarter of a second in the result. How, then, can a fair investigation bring this as a reason for an alleged inaccuracy, when it obviously cannot apply to the case? And what must be thought of his impartiality, if it shall appear, that even in this respect, the observations of the French and of Major Lambton, which he so manifestly prefers to the English observations, are far more open to censure? Allow me, therefore, just to make the comparison.

Of the English observations, none are suppressed, (the observers going upon the principle explained by Simpson, in his "Tracts," which clearly establishes the propriety, if not the necessity, of taking the mean of a number of observations) and yet, no irregularity of consequence, except the one above specified, appears. But, it may be seen from p. 72, Discours Préliminaire, tome i. Base du Système Métrique Décimal, that no less than sixty-eight of the French observations upon β Ursæ majoris were rejected, and termed bad; for no other reason
reason that I can perceive, than, that if they had been employed, they would have given the latitude of Dunkirk about a second less than the observations of the pole star gave it. Let Don Rodriguez reflect upon this, and then repeat that the French operations "merit the highest degree of confidence." But this is not all. From p. 39 of the same Discours Précis, it appears that three stars only were selected by Mechain at Montjoie, in consequence of the coincidence of the results arising from them. Among the stars rejected, was γ Ursae, because different observations gave a difference of 4". So that the French also detected an irregularity respecting this star.

Herschel's observations on double stars, show the reason of the apparent anomaly, in Col. M.'s case, and make it rather a proof of accuracy.

They assign, however, a wrong reason for the fact; for they attribute it to errors in Bradley's table of refractions, while the truth is, that γ Ursae is a double star, by no means easy to observe properly. Indeed, it appears, not only from the observations of Col. Mudge, &c. but from those of Dr. Herschel (Phil. Trans. vol lxxxii. New Abridgment, vol. xv.), that both μ Draconis and γ Ursae are double stars; that of the former, the two constituent stars appear equal, both white, and not easily distinguishable, and at the distance of 4". 35 from each other, mean measure; and that of the latter, the two are considerably unequal, and the largest difficult to bisect. Hence Herschel's observations completely confirm those of our trigonometrical surveyors. See also the catalogues of Wollaston and Bode.

Let us next enquire how far Major Lambton's observations, which Don Rodriguez also seems to delight in eulogizing, deserve to be preferred to Colonel Mudge's. From p. 350, vol. x. Asiatic Researches, we learn that the Major's observations upon a Serpentis were 14, of which two were 5°57' 3".38 and 5°56' 53".98 furnishing a difference of 9".3; more than double the difference that has been found in the English observations, of which the Don complains! At p. 357, again we have a register of 16 observations upon a Aquilæ, of which two differ by 6".77. At p. 358, we have 18 observations upon a Taurus, of which two differ by 5".38. There are also some other palpable differences in Major Lambton's results, as deduced from different stars. The greatest is between a Taurus and Markab, being 5".48. a Taurus, from the number and agreement of its observations among themselves, should be correct in zenith.
zenith distance, yet it gives the latitude of the station, Doda-
goontah, less by 3' 4" than the mean of the nine stars, employed by Major Lambton, exhibits it, and the latitude found from a mean of the four northern stars, is 2' 04" greater than the latitude found from a mean of the five southern stars. Discrepancies of more than 4" may likewise be frequently found in the observations recorded in vol. viii. of the "Researches." Most of them are, probably, in great measure, attributable to the imperfections in Major Lambton's sector, which is only of 5 feet radius (while the English is of 8 feet); and is provided with but few comparatively of the requisite means of adjustment; but whether they are to be ascribed to the observer or his instruments, they prove that Don Rodriguez has been rather precipitate in saying, "the same Major Lambton, who has succeeded so well in Asia, and is in possession of such perfect instruments for the purpose, would be singularly qualified for a similar undertaking in Africa." In matters which admit of examination and proof, it is not the custom with Englishmen to bow at once to the authority of a mere ipse dixit. Was Don Rodriguez really ignorant that, with respect to accuracy of observation, the English proceedings are thus greatly superior to those of the French and of Major Lambton? If so, how greatly is he to be pitied for writing so much on a subject he had previously so little considered. If he was aware of this superiority, how much more is he to be pitied, for giving so unfair and unnatural a representation of the business before him.

From one or other of the reasons I have thus examined, Don Rodriguez says, "it is almost beyond a doubt that it is to errors in the observations of latitude," the singularity in Col. Mudge's results must be ascribed. There must be an error of some seconds in the observations, "especially at Arbury Hill." And he asks, "How is this to be discovered?" How? Why, by simply repeating the observations at Arbury Hill. The position of the station is so clearly described in the Philosophical Transactions, that any person may find it within 20 feet; and the farmer who owns the field, can show the identical spot. Don Rodriguez, or some one of his friends, has, doubtless, handy circular instruments of the French construction, by which the zenith distances could readily have been taken, and then the correctness
correctness or incorrectness of the English observers might have been proved in a way from which there could be no appeal. Though, to be sure, if that plan had been adopted, and the English results had, in consequence, been verified, Don Rodriguez's paper could never have appeared.

There is, however, a method of determining the point, even without taking this trouble. Having then shown, I trust satisfactorily, that Don Rodriguez's reasons for imputing an error of 4 or 5 seconds to the English observations, are nugatory; I shall now proceed, with all possible conciseness, to show that there cannot be an error of one second either in the observations at Arbury Hill, or at Dunnesse; and those at Clifton are, by the Don's own concessions, out of the question.

First, the manner of fixing the zenith sector could not lead to error; for, "to procure for the external stand (says Col. Mudge, Phil. Trans. 1803) and thence for the whole apparatus, a firm foundation, I caused four long stakes to be driven into the ground, one for each foot of the stand, to which its feet were firmly screwed down. The surfaces of the stakes were cut off smooth, and brought into the same horizontal plane, by which means the interior frame and sector were placed much within the limits of their several adjustments." The whole was enclosed in a suitable observatory.

Don Rodriguez may perhaps think the French method of fixing their instruments, on some occasions, preferable to this. The reader shall judge. Their instruments, both for taking horizontal and vertical angles, were sometimes placed on a stage so tottering, that even a little breeze much disturbed the observ.

french observations at Chatillon made during a high wind, on a stage so tottering, that even a little breeze much disturbed the observ.
notwithstanding, doubt the observations made with a stable instrument by the English? And let him not forget, that whatever error was thus occasioned in the distances between Boiscommun and Chatillon, is more than doubled in all the remaining triangles of the series, by reason of the bad shape of the triangle, Chatillon, Boiscommun, Chateauneuf.

If no error in the English observations can be fairly imputed to the manner of fixing the zenith sector, neither can any be ascribed to the "construction" of the instrument itself. This was most positively declared by two very excellent judges, the late astronomer royal, and the Hon. Henry Cavendish, on their close examination of the instrument. It will also be inferred, without hesitation, by all competent judges, on reading the description of it in the Phil. Trans. for 1803. To those who have seen neither the instrument nor the description, it may suffice, if I remark, that the equality of the divisions on the arch, is evinced from this consideration, that on running the micrometer screw from division to division, over the whole arch, there was no where an indication of an error amounting to half a second; and that the instrument still continues free from important "derangement," is tolerably well proved by this, that the line of collimation has been constant during all the observations and all the journeyings of the sector, and that it still continues the same.

In the next place, it may be remarked, that no error in observation can be imputed to a deviation from "vertical position" in the sector. Important inaccuracy, in this respect, is precluded by the great length of the axis, by which the instrument is rectified; and by the ready and certain means of placing the plumb-line directly over the illuminated dot which marks the middle of the axis, or true centre of the divided arch. For want of these admirable modes of correction, all previous instruments are necessarily imperfect. It appears from Phil. Trans. for 1803, pp. 405, 406, that when the instrument is adjusted in one position by means of the plumb-line and dot, it is turned to a position at right angles to the former, and the adjustment confirmed; and this being the case in these two situations, the instrument must necessarily be vertical in all others.

Various reasons may be assigned to show that the sector could not,
or to a deviation from the plane of the meridian. I shall select only two or three. As 1st, if the sector were inclined to that plane, just so much would the path of any star, in its apparent motion, be inclined to the horizontal wire of the telescope; instead of which, both Colonel Mudge and Captain Colby assure me, that when a star came into contact with the wire, the light of the star would appear on both sides of the wire for about three-fourths of a minute of time, the light on each side being equal at the central wire: which of itself is a positive proof. But, 2dly. had the sector been out of the plane of the meridian, the times of the transits of the extreme stars employed, as compared with two excellent time-keepers, must have shewn it. Farther, the errors arising from a wrong plane of the meridian, being comparatively very great in the extreme stars, and small in those near the zenith, it would follow that the error in Capella, which is almost at the extremity of the arch, would be great, compared with those in β Draconis, κ Cygni, &c. which were within a small distance of the zenith. But the amplitude of the arch, between Dunrose and Arbury Hill, as derived from Capella, is \(1^\circ 36'20''42\), while those derived from the other two stars, are \(1^\circ 36'19''42\), and \(1^\circ 36'19''44\): a coincidence which proves that the instrument could not possibly have any perceptible deviation from the plane of the meridian at either station. Other reasons for coming to the same conclusion will appear, on attending to the precautions in adjusting by double azimuths, &c. as described in the Phil. Transactions.

The correct position of the sector in all respects is further proved from this: that the observations, however distant in point of time, when the proper corrections for aberration, nutation, &c. are applied to them, reduce always very nearly to the same mean place.

Hence, it must be obvious, that no error could arise, as Don Rodriguez suspects, from the instrument, whether in "vertical position, construction, or some accidental derangement." I shall now advance still farther, and prove that there is no error in fact. For if there were any error in the zenith distances at Arbury Hill, it would at once be detected on comparison with the observations at Blenheim. Now, the distance between the parallels of latitude of Blenheim and Arbury,
139,822 feet, furnished by the survey, gives for the corresponding celestial arch, 22° 59'33", while the observations of γ Draconis at Blenheim, compared with the observations upon the same star at Arbury Hill, give 22° 59'0". So that there cannot possibly be an error of half a second at Arbury Hill, unless the observations, for five successive years at Blenheim, were all wrong; and Blenheim observatory, be it recollected, has been long celebrated for the excellence of its instruments; and is selected even by Svanberg for the accuracy of the observations there made.—So, again, with regard to the Dunnose station, the latitude of Portsmouth observatory, as inferred from the said station, and the data in the Trigonometrical Survey, is 50° 48'2"05"; while the Requisite Tables, the edition of 1781, give it 50° 48'3". So that the observations at Dunnose cannot possibly err half a second, unless there was an error made by Witchell and Bayley, in determining the latitude of Portsmouth observatory, with an admirable mural quadrant, by Birt. These two deductions, then, completely exclude sensible error at Dunnose and Arbury Hill: and these inferences, it is evident, might as easily have been made by Don Rodriguez as by me.

This gentleman may find still farther confirmation of the truth of the whole survey, if he will examine the operations by which the meridian of Dunnose is extended to Burleigh Moor, and those for carrying on a new meridian from Black Down to Delamere Forest. These, it is true, are not to be found (for what reason I cannot say) in the Philosophical Transactions. But they may be seen in the third volume of the Trigonometrical Survey, published in 1811, by order of the Board of Ordnance; a volume with which some of Don R.'s friends in England are doubtless acquainted.

As a last corroboration of the whole portion from Dunnose to Clifton, amounting to 2° 50' 28"38"; let me add, that when compared with the meridional arch of 3° 7' 1" at Peru, by means of the valuable theorem, investigated by Professor Playfair, (Edinburgh Transac. vol. v. pp. 8, 9,) for the comparison of large arcs; it produces \[ \frac{3}{2} \frac{1}{4} \] for the resulting compression. While Svanberg (pa. 192, "Exposition") gives \[ \frac{3}{3} \frac{1}{4} \] for the compression, as deducible from a comparison of his measure with that at Peru.

Thus, we have confirmation upon confirmation, of the correctness.
rectness of Colonel Mudge's operations, both general and particular; and of the extreme rashness with which Don Rodriguez has affirmed, that "it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error." The matter in question might, as you will perceive, have been settled in narrower compass; but the celebrity of the institution under whose auspices the Don's animadversions are circulated, seemed, in some measure, to call for a tolerably full reply to his paper.

For the reply here presented, the public must consider me alone as responsible: and I trust that when the two papers have been compared, I shall not be thought to speak imcompatibly with the courtesy due to a foreigner, or the respect due to a brother mathematician, when I say that Don Rodriguez has completely failed to establish the point, respecting which he ought to have felt certain before he commenced his strictures.

OLINTHUS GREGORY.

Royal Military Academy,
Woolwich, March 5th, 1813.

IV.


To Mr. Nicholson.

Edinburgh, March 3, 1813.

SIR,

In your Journal for January, an account is given of an experiment performed by Sir Humphry Davy in the College Laboratory of Edinburgh, in relation to the question on the existence of combined water in muriatic acid gas. I had found that the salt formed by the combination of this gas with ammonia, affords water when it is exposed to heat; and this water, I inferred, is derived from the acid. Sir H. Davy supposed it to be water which the salt had absorbed from the air; and he and his brother affirmed, that when the air is excluded, none is obtained. I resumed the investigation, and found that the salt
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Salt absorbs no water from the air, and that it affords water when heated, though the air has been excluded. The same results were obtained by Drs. Bostock and Traill. It remained, therefore, for Sir Humphry to shew that they were not correct, or to establish, by farther evidence, his former statement. With this view the experiment, above alluded to, has been performed. About 90 cubic inches of muriatic acid gas were combined with the requisite quantity of ammonical gas, in an exhausted retort of the capacity of 26 cubic inches, and the salt formed having been heated in the same retort, closed at its extremity by a stop-cock, water was obtained from it in small quantity, "a dew just perceptible lining the cold neck." On this experiment I have now to offer a few observations, and I have to state the result of another since performed.

When the experiment was made, I was informed by Dr. Hope of the result, and of the manner in which it had been executed. I stated to him in what respect it appeared to me objectionable, independent of the unfavourable circumstances inseparable from the mode of heating the salt in a close vessel; the large size of the retort rendering it difficult to apply the heat equally, so as to expel the water from one part without its condensing in another, allowing, too, a larger portion of any vapour disengaged to remain in the elastic form while the heat was kept high, and equally permitting its condensation when the heat diminished over an extensive surface, encrusted with a substance by which it would be absorbed, the unequal application of the heat producing a similar volatilization from one part, and condensation in another, the confinement of the heated elastic fluid operating by its pressure in resisting the separation of the water from the salt, and by its temperature counteracting the local condensation of the portion evaporated, and lastly, the encrustation of salt which had been allowed to remain at the curvature and upper part of the neck of the retort, where, in such an experiment, the condensation of moisture chiefly takes place, were all unfavourable to the result. If the experiment had been one in which a considerable quantity of water was to be looked for, these circumstances might have been of less importance. But this not being the case, it was more necessary to attend to their influence, and every arrange-
ment with regard to the experiment, ought to have been rendered favourable to the result, instead of being truly the reverse.

The principal circumstances which I conceived required to be attended to, were, to employ a much smaller vessel, to raise it through its whole capacity to an equal heat, to have the part of the apparatus in which the water is to be condensed free from salt, and to avoid, as far as practicable, the operation, either of pressure, or of a partial vacuum. It was nearly in this manner, that the experiment was performed by Dr. Bostock and Dr. Traill, and hence their successful result, while Sir Humphry, from not attending to these circumstances, was less successful, though performing it on a much larger scale. Dr. Hope, anxious to ascertain the matter of fact, readily agreed to repeat the experiment with these variations; Lord Webb Seymour and Mr. Ellis were present, and I have his permission to communicate the result.

Ammoniacal gas, previously exposed for two days to dry potash, and muriatic acid gas which had been exposed to dry muriate of lime for 24 hours, were combined in a dry exhausted flask, of the capacity of 3.8 cubic inches. About 90 cubic inches of the acid gas were employed, and the flask remained at the end filled with ammoniacal gas. The stop-cock being removed without exposing the salt to the air, a glass tube of four-tenths of an inch in diameter, previously fitted by grinding to the neck of the flask, was inserted, its open extremity dipping in quicksilver, and the flask being surrounded with sand in an iron box, was placed horizontally on a chafing dish, and fuel gradually introduced, so that the heat applied was slowly raised. In a short time moisture appeared in the tube, at a little distance from its insertion into the flask; this increased, proceeding to a greater extent along the tube, and condensing in globules perfectly distinct, which, at different periods of the experiment, covered the inner surface for a length of three, four, or six inches; and a small quantity collected at the under part, which, with a very slight inclination of the tube, moved slowly onward. At length the salt sublimed, and condensed in the tube close to the flask. The quantity of water, Dr. Hope was satisfied, appeared considerably larger than in Sir Humphry's experiment. The same quantities
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Quantities of gases had been employed as in that experiment, and I need scarcely say, that every precaution had been taken to exclude every source of fallacy. Some of the salt having reached near to that place of the tube where the dew was condensed, part of the moisture seemed to have been resumed by it during the cooling of the apparatus, and prevented Dr. Hope from ascertaining with precision the quantity of the fluid. To obtain an estimate of it, he next day put a little water into another flask having a similar tube, previously weighed, fitted to it by grinding, and applied heat to the flask till the inside of a portion of the tube was covered with dew, and a drop of water collected in the bottom, as in the preceding experiment. The quantity of humidity, thus condensed, weighed one grain, and in appearance so far exceeded that observed in the tube in the experiment of the preceding day, as to lead to the conclusion, that the latter could not be estimated at more than two-thirds of a grain.

Such is the result of these experiments intended to be decisive of the question with regard to the state of the fact, whether, when this salt is heated in close vessels, any water is obtained from it or not. Messrs. Davy affirmed, in the most explicit terms, that there is none; Sir H. Davy "did not observe the slightest traces of moisture in making the experiment on a larger scale in exhausted vessels." And Mr. J. Davy found, that "no water was produced—not even the slightest trace appeared." I affirmed, that though this mode of conducting the experiment is unfavourable to the result, and is not at all calculated to afford information with regard to the real quantity which the salt yields, still a sensible portion of water is obtained. It is now established, that my statement is correct, that of my opponents the reverse. In the experiment, as performed by Sir Humphry himself, a sensible portion of water appeared, and when the obvious sources of fallacy attending that experiment have been avoided, a larger quantity has been obtained.

To obviate the conclusion which might be drawn from this result, Mr. J. Davy endeavours to show, that the quantity obtained in his brother's experiment might be derived from extraneous sources, from vapour in the gases, or moisture from the mercury. This it is scarcely necessary to discuss. Dr. Henry, he remarks, found that ammonia obstinately retains aqueous vapour, yet Dr. Henry states, that ammonia may be
so far desiccated by exposure to potash, "as to show no traces of condensed moisture when exposed to a cold of 0° of Fahrenheit," and this precaution of exposing the ammonia to heat had been observed both in Sir Humphry's and in Dr. Hope's experiment. His brother, he adds, has proved, that a minute portion of solution of muriatic acid in water may be obtained by intensely cooling the gas. Dr. Henry, however, found, that muriatic acid gas, when freed from visible moisture, which it is completely by exposure to muriate of lime, (a precaution observed in the above experiments) deposits no water even when cooled to 26 below 0° of Fahrenheit, and Gay Lussac not only obtained the same result, but further found no indication of moisture from the action of fluo-boric gas, which is its most delicate test. And, even according to Sir Humphry's statement, the quantity of liquid deposited from 200 cubical inches at 75°, cooled to 10 below 0, is not equal to \( \frac{1}{15} \) of a grain, and only about half the weight of this is water. If any such water, therefore, is taken up by the gas at 50°, and retained by it after exposure to muriate of lime, of which there is no proof, but the reverse, it may amount, in 90 cubic inches, to \( \frac{1}{17} \) or \( \frac{1}{20} \) of a grain. Lastly, the mercury had been strained through warm linen, and was perfectly dry. The gases, therefore, having been submitted carefully to processes which are known to render them free from all moisture, being transmitted through dry mercury, and combined in an exhausted vessel, so that the mercury never came into contact with the salt, there is not the slightest reason to suppose a communication of water from any extraneous source. It is an obvious reflection, too, that if this salt is otherwise entirely free from water, as the new hypothesis assumes, were a minute portion communicated to it, it must be retained, in conformity to the law which universally regulates the combination of water with saline substances, by a very powerful attraction, so that it could not be expelled, and rendered sensible in such an experiment. And lastly, such causes are assigned by Mr. J. Davy only as "tending to account for the very minute quantity of water obtained" in his brother's experiment. They are, of course, still less adequate to account for the larger quantity in Dr. Hope's experiment; and are utterly incapable of accounting for the much larger quantity admitted by them to be obtained when the salt is heated in.
communication with the atmosphere, and which, it will be shewn, is derived from the salt, and not from the air.

Mr. J. Davy farther contrasts the small quantity of water obtained from the muriate of ammonia in his brother's experiment with the quantity which, according to the common doctrine, it contains; this latter quantity, he seems to imagine, ought to be procured; and, since it is not, he concludes that that doctrine cannot be maintained.

Any discussion with regard to the quantity of water obtained by heating the salt in a close vessel, is probably superfluous. That kind of experiment I never considered as one calculated to afford a proper indication of the real quantity which the salt yields. I repeated it merely because Messrs. Davys affirmed, that there is no appearance of water whatever. That assertion is now proved to be incorrect, which is all that the repetition of the experiment was designed to establish, and the original mode of conducting it I consider as the one which gives the true result.

It may be remarked, however, to obviate any difficulty from this point, even with regard to the quantity obtained in the more favourable mode of conducting the experiment, that the combination of muriatic acid gas with ammonia, was not regarded as adapted to determine the proportion of combined water in the acid gas; for, of all the combinations of this acid, it is the one in which there is the greatest difficulty in separating the water. Acids, in combining with salifiable bases, retain the whole, or the greater part of their combined water, especially when these bases have also an attraction to water. To expel this from the compound salt to any extent, a heat, equal or superior to ignition, is in general required; and, by the most intense heat, it does not appear, that the whole quantity is expelled. Berthollet has shown, that after exposure to the violent heat of a forge, salts retain water, so that when again exposed to heat in mixture with iron filings, they afford hydrogen gas; and this is the case even with those which appear to have little attraction to water, as sulphate of barytes. Where the salt, therefore, is volatile, such as muriate of ammonia, the expulsion of its water must be imperfectly attained. The degree to which the heat may be raised is not great, and, in raising it, it must operate nearly with as much force on the real salt,
sodium as on the water combined with it, and their mutual affinity must retain them in union till both are sublimed together. If other salts which are fixed, and which have a less strong attraction to water, yield it only at a high temperature, and then imperfectly, it is absurd to imagine, that muriate of ammonia should yield it at a much lower temperature, and yield it entirely. The experiment, therefore, was designed rather to prove the existence of combined water in muriatic acid gas, and though the quantity obtained may not be the whole quantity which, from other facts, there is reason to conclude, exists in the acid gas, it establishes this as much as if a larger quantity were obtained. The production of any water is incompatible with Sir Humphry's hypothesis, and, therefore, refutes it; it is conformable to the opposite doctrine, and becomes, therefore, a proof of its truth; and for the quantity being less than that from other saline combinations of the acid, an adequate cause can be assigned. The actual result, indeed, is precisely that which is to be expected, a sensible portion of water more considerable as the experiment is performed in a manner more favourable to its disengagement, but inferior to what is obtained from other combinations of the acid, from which it is obvious, a priori, that the water must be more easily expelled.

So far I have restricted my observations chiefly to the result of the experiment of heating the salt in close vessels. A point not less important, which remained for determination, is that relating to the result when it is heated in open vessels, and to the supposed fallacy connected with this in the absorption of water from the air.

I had found that, in this mode of conducting the experiment, a very sensible quantity of water was obtained; and this was not denied, but explicitly admitted, by my opponents. Mr. J. Davy, who had heated the salt in close vessels, without obtaining water, found, that when he "followed Mr. Murray's example, and collected the salt in the atmosphere, and introduced it into another retort, on heat being applied, water, in no inconsiderable quantity, was evolved, as he described." But to account for this, without admitting the conclusion subversive of his hypothesis, Sir Humphry Davy advanced the supposition, that the salt absorbs water from the air during its trans-
transference from the one vessel to the other, and that this is the source of the water which it yields.

A supposition so directly at variance with the known properties of this salt, required very ample proof, yet none was given of it, farther than the assertion of the salt not yielding water when heated in a close vessel, while it affords it when heated in an open vessel, this result being stated as affording "a demonstration, that the water liberated in Mr. Murray's experiment, was not derived from the muriatic gas, but from the atmosphere." It affords, I remarked, (Journal, vol. 32, p. 187,) no proof, since, admitting even the statement with regard to it to be correct, it might equally arise, since it is proved, that the salt yields water when it is heated without having been exposed to the air.

I had proposed the obvious experiment by which the fact, with regard to this supposed absorption of water, may be unequivocally ascertained—that of forming the salt without exposure to the air, and then ascertaining if, under such exposure, it gains weight, which it must do if it absorbs water. The mode of conducting the experiment, and the results, have been already minutely detailed (Journal, vol. XXXII, p. 191.) These results, proving that no water is absorbed, Messrs. have not attempted to controvert, but have rather thought proper to avoid repeating the experiment, though it had been urged against them, and is obviously decisive of the question—for what reason I shall not conjecture.

The importance of the fact with regard to this supposed absorption is such, both from the supposition having been introduced to account for the production of water from the salt, and from its having led, in consequence of that, to a form of experiment which has rendered the investigation more difficult and more liable to error, that I was desirous the experiment should again be performed with every precaution. Lord Webb Seymour and Mr. Ellis were present, and the principal steps of the experiment were executed by Dr. Hope. A vessel was selected, the interior of which might admit of a free exposure to the air—it was pear-shaped, having a wide orifice at each extremity, the one, one inch and a half in diameter, the other, one inch, its whole internal surface being equal to about 40 square inches. The orifices were closed with corks rendered air-tight.
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Air-tight by cement, a stop-cock being inserted in one of them for the introduction of the gases.

The vessel having been exhausted, about 27 cubic inches of muriatic acid gas, which had been exposed for two days to dry muriate of lime, were combined in it with the requisite quantity of ammoniacal gas, which had been exposed for the same time to dry potash; and an excess of ammonia was allowed to remain at the end of the combination. The corks, with their cement, were removed, and clean corks, previously fitted, were instantly inserted. The vessel was filled with atmospheric air, by opening one of the orifices, and introducing a tube attached to a caoutchouc bottle, the sides of which being pressed together, and then allowed to dilate, drew out the ammoniacal gas: and to secure the change being complete, both corks were removed for a second or two. The apparatus was then placed in a balance, which, loaded with it, turned very sensibly with much less than \( \frac{1}{20} \) of a grain. The balance being accurately adjusted, the corks were removed from the orifices, and placed beneath the vessel, and the progress of the experiment was observed. At the end of five minutes there was no perceptible change, of ten minutes no change, at fifteen minutes there was, if any thing, a loss of weight on the side of the salt; at twenty minutes this loss was apparent, and amounted to about \( \frac{1}{20} \) of a grain, at twenty-five and at thirty minutes it remained the same. Though from the form of the vessel, and the size of the apertures, the air had the freest access to the salt which encrusted the interior; yet, to leave no doubt, the internal air was changed repeatedly by means of the caoutchouc bottle. At forty minutes there was again the appearance of loss of weight in the salt, at fifty minutes this amounted to something less than \( \frac{1}{20} \) of a grain, in addition to the former loss. The air within the vessel was again repeatedly changed, both by means of the caoutchouc bottle, and by propelling the external air through it by the motion of the hand, and by the bottle, held at a distance and slowly compressed; but for half an hour longer there was no perceptible variation of weight*.

This

* In a preliminary experiment which I had performed, and in which the salt was freely exposed to the air for three days, the loss of weight was
This experiment was performed in the same apartment in which my former experiments had been executed, and the air was at the same temperature of 60. It is perfectly decisive in proving, that the salt absorbs no water from air in a common state of dryness and temperature.

As much of the salt was collected as could be removed from the vessel; it weighed 23·5 grains. It was introduced into a small retort connected with a small globular receiver, and the body of the retort being in part surrounded with sand, heat was applied by a lamp. A little of the salt suddenly rose in vapour into the neck of the retort. Afterwards moisture condensed beyond the salt where the neck was kept cool; the heat was slowly raised until the salt was sublimed into the top and beginning of the neck of the retort. The sand bath was then removed, a chafing dish was applied, and the heat continued for half an hour. In the course of the experiment, the moisture increased, and extended over about one inch and a half of the upper side of the neck of the retort, where the cold was applied. The half of this space next to the bulb appeared quite wet, being covered with compressed globules of water of a considerable size, on the remaining part the globules were very minute.

I formerly related an experiment in which mutiate of ammonia, after it had afforded a portion of water at a low heat, was sublimed through ignited charcoal, to ascertain if, by the higher temperature, and by the chemical affinities exerted by charcoal, an additional quantity might be abstracted. Portions of carbonic acid, and carburetted hydrogen gases, were accordingly obtained; and a quantity of water was condensed. This latter result led to the conclusion, that the high degree of heat had produced a more perfect separation of the water, and that, therefore, if such a temperature were applied to the salt alone, more water might be obtained from it than by an inferior heat, while any supposed source of fallacy from the presence of the charcoal, might be avoided.

More water inferred to have been separated in an exp. with higher heat.

was apparent to a still greater extent than in the above experiment. Such a result, with regard to any other salt, would be ascribed to the abstraction of water by the agency of the air; and I see no reason why the same conclusion should not be drawn with regard to it. At the end of a week the salt remained perfectly dry.
Common sal-ammoniac has its first portion of water expelled in the manufactory, and does not afterwards attract more; but it gives out another portion in an ignited tube.

A fact I had ascertained promised to afford a satisfactory mode of verifying this. The common sublimed muriate of ammonia, or sal-ammoniac, I had found, yields no water when exposed to a heat sufficient to sublime it. This is owing to its mode of preparation— it is first dried, then sublimed, and, during the sublimation, the upper part of the vessel is kept hot, to render the sublimed mass sufficiently dense, its orifice being also kept open, and hence all the water which can be driven off by this heat is expelled, and none is regained by exposure to the air (a decisive proof, if such were wanting, that this salt attracts no water from the atmosphere, since it is kept in the shops without any particular precaution. I exposed 100 grains of this salt in a retort to a heat sufficient to produce sublimation, but no moisture appeared during any part of the experiment. I then sublimed 100 grains of the same salt from the close end of a porcelain tube, placed across a furnace so as to be at a red heat. A very sensible quantity of moisture condensed in a glass tube, which was adapted to the porcelain one, appearing not only in globules, but at length running down the tube. This proved, that water may be separated from muriate of ammonia by a red heat, which is not expelled from it at a lower temperature. I then submitted to a similar experiment, the salt formed by the direct combination of its elements. Very little moisture appeared previous to its actual volatilization, but when this commenced, the condensation of water in sensible globules took place; they continued to accumulate, and the quantity appeared obviously greater than what, judging from former experiments, would have been obtained by a lower heat from the salt formed from the same quantity of muriatic acid gas.

In another experiment, the salt formed in an exhausted retort was first heated until it ceased to afford water, and was afterwards sublimed through an ignited porcelain tube. Moisture was again obtained, though not in so large a quantity as when the charcoal had been placed in the tube. There is no just objection to the introduction of the agency of the charcoal, if care be taken to have it thoroughly calcined; and, as the supposed source of fulness from the air affording water to the salt, is now proved to have no existence, there is no valid objection.
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Inference.

My preceding conclusions, I trust, are now sufficiently established, and it is unnecessary to enter on any recapitulation of the argument. Water has been obtained from this salt both when it is heated in close and in open vessels; and no source of fallacy exists, as was affirmed by Messrs. Davys, in any absorption of water from the atmosphere. They accounted for the production of water on that supposition, and it is now amply refuted.

I have only a single observation to make on Mr. J. Davy's concluding remarks in his last communication, that he has "no intention of answering personal aspersions, which are only injurious to the author when unjustly made." The necessity was imposed upon me by assertions which he had advanced of stating some circumstances connected with the manner in which he and his brother had conducted the controversy. I did so with reluctance, and only in so far as was necessary to my own vindication from a very intemperate attack. My observations conveyed censure, no doubt, but not aspersion; for they were founded on facts, and these were very explicitly stated, that Mr. J. Davy might, if he pleased, enter into any explanation with regard to them. This he has not done, and the facts, I believe, he is unable to controvert.

In concluding this investigation, I cannot but contrast the assertions that were made, and the tone that was assumed, with the result that has been established. "At first view," said Mr. J. Davy, speaking of my experiment of obtaining water from muriate of ammonia, "the result appears improbable, and opposed by several facts; and, in a very short time, I was convinced by experiments that it was incorrect." Again, propriety, have been more modest and temperate.

"Mr. Davy, my brother, informed me, that he had not observed the slightest traces of moisture in making the experiment on a large scale in exhausted vessels; and assured me, that I should not, was not the salt exposed to the atmosphere." In repeating the experiment accordingly, no water was produced "agreeably to my brother's result, not even the slightest traces appeared." Mr. Murray's error," he adds, "appears to have arisen partly from too great confidence placed in the accuracy of his experiment, and partly from overlooking that..."