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ARTICLE I.

An Account of some Experiments on different Combinations of Fluoric Acid. By John Davy, Esq. From the Philosophical Transactions, 1812.

Introduction.

Two years ago, I engaged, at the request of my brother, Sir H. Davy, in an inquiry respecting the nature of common fluoric acid gas. My principal object was to ascertain whether silex is essential to its constitution, and whether the proportion is constantly the same. This subject, and experiments on the fluoric and fluoboracic acids, occupied me for about six months. Since that time, the work of MM. Gay Lussac and Thenard has appeared, entitled "Recherches Physico-Chimiques," in the second volume of which is an elaborate dissertation on fluoric acid. These philosophers, I find, have anticipated many of my results, and consequently very much abridged my labour of detail in the following pages. To repeat what is already known would be useless; I shall therefore confine myself to describe what I have observed, which appears to me yet novel, or different from the observations of the French chemists. The order which I shall pursue, will be that which I observed in my experiments. I shall divide what I have to advance into four
four parts. The first part will relate to the silicated fluoric acid gas, and to the subsilicated fluoric acid; the second to the combinations of these acids, and of pure fluoric acid with ammonia; the third to fluoboracic acid; and the fourth to its ammoniacal salts.

**Sect. 1. On silicated fluoric acid Gas, and subsilicated fluoric Acid.**

The facts which have already been published by MM. Gay Lussac and Thenard and others, appear to me to be sufficient to prove that pure fluoric acid has not yet been obtained in the gaseous state, and that silex, or boracic acid, is requisite that it may assume this form. Were more evidences necessary, I could advance many in point. One circumstance only I shall mention, proving that common fluoric acid gas is perfectly saturated with silex. I have preserved this gas, made by heating, in a glass retort, a mixture of fluor spar and sulphuric acid, for several weeks over mercury in a glass receiver uncoated with wax, without observing the slightest erosion to be produced.*

This gas, with great propriety, has lately been called silicated fluoric. Before I proceed to its analysis, I shall notice what method I have found the best for obtaining it. I have, for a considerable time, long before MM. Gay Lussac and Thenard's work was published, added to the mixture of fluor spar and sulphuric acid, a quantity of finely pounded glass, and have thus procured the gas with the greatest facility. The advantages of this addition are considerable. The retort is saved, which otherwise, in less than one operation, would be destroyed; and a much larger quantity of gas is procured from the same materials, and with less trouble and less heat; the action indeed at first is so powerful, that gas begins to come over before the application of heat is made, and a very gentle one only is required to continue its production.

* The sides of the receiver indeed became obscure; but this was not from erosion, but from deposition, as appeared from the transparency and polish of the glass being readily restored by slight friction. What the deposition was, I am ignorant of. After several weeks it was so trifling, as to give only a slight degree of opacity to the receiver.
Previous to its analysis, it was necessary to ascertain the specific gravity of the gas. This I have endeavoured to do.

The gas, the subject of experiment, was quite pure, being totally condensed by water. A Florence flask was exhausted; in this state, weighed by a very delicate balance, it was

\[ \text{Filled with common air} = 1452.2 \text{ grains.} \]
\[ \text{Again exhausted} = 1452.2 + 10.2 \]
\[ \text{Filled with silicated fluoric gas} = 1452.2 + 36.45 \]

Hence as \( 10.2 : 31 :: 36.45 :: 110.78 \)

Thus it appears, that 100 cubic inches of silicated fluoric acid gas, at ordinary temperature and pressure, are equal to 110.78 grains.

When silicated fluoric acid gas is condensed by water, it is well known that part only of the silex is deposited. To obtain the whole, in order to ascertain the proportion in the gas, I have employed ammonia in excess. 40 cubic inches of the gas (barom. 30, therm. 60) were transferred in portions of 10 cubic inches at a time to a solution of ammonia. The silex precipitated was carefully collected on a filter, and washed till the water that passed through it, ceased to be affected by nitrate of lime. It was next dried and strongly heated in a platina crucible. It weighed 27.2 grains, and was pure silex.

Supposing fluoric acid to be the remaining 17.1 grains, which added to 27.2 grains are equivalent to the weight of 40 cubic inches of the gas, it appears that 100 parts by weight of this gas consist of

\[
\begin{align*}
61.4 & \text{ silex} \\
38.6 & \text{ fluoric acid}
\end{align*}
\]

\[ \frac{61.4 + 38.6}{100} = 100 \]

That this estimate may be correct, it is evident, that ammonia should have the property of precipitating the whole of the silex of silicated fluoric gas; which I shall not now endeavour to prove, but leave it to be considered in another part of the paper.

There is no improbability attached to the idea, that silicated fluoric acid gas may, from the manner in which it is prepared, contain a proportion of alkali. To discover whether this was...
the case, a solution of nitrate of lime was added to the ammoniacal solution neutralized by nitric acid, from which the silex in the preceding experiment had been removed. The precipitate of float of lime was separated by filtration. The filtered liquid was evaporated to dryness; and the ammoniacal salt heated in a platina crucible till it was entirely dissipated. The residue had the appearance and taste of quick lime. It was dissolved in acetic acid, and the solution yielded sulphat of lime on the addition of sulphat of ammonia. The liquid was evaporated to dryness, and when the residuum has been heated to dull redness, nothing remained but a little white powder, weighing about a grain, and having all the properties of gypsum. Thus it appears that silicated fluoric acid gas contains no alkali.

My next object was to ascertain the composition of common liquid fluoric acid—that acid obtained by the decomposition of silicated fluoric acid gas by water, and which, on account of the separation that occurs of part of the silex, may, with greater propriety, be called subsilicated fluoric acid. For this purpose, 43.21 cubic inches, barom. 30.4, therm. 50, or 44 cubic inches at common temperature and pressure, were successively added, two cubic inches at a time, to one cubic inch of distilled water in a small jar over mercury. The whole of this, the gas being pure, was readily condensed. The temperature was somewhat raised. The silex precipitated, formed a gelatinous mass of a blueish colour, which had absorbed all the water like a sponge, so that none appeared fluid. This gelatinous mass was carefully transferred to a filter, and washed with distilled water till it was rendered insipid and incapable of reddening litmus paper. It retained its blueish hue only whilst moist. When dried and ignited, it was in thin lamellæ, and of a snow-white colour, and surprisingly bulky. It weighed 7.33 grains, and was found to be pure silex. Thus it appears that the subsilicated fluoric acid formed by the decomposition of 44 cubic inches of silicated fluoric acid gas contains 7.33 grains of silex less than the gas itself. Consequently, independent of water, which no doubt is essential to this acid, 100 parts of it seem to consist of

54.56 silex
FLUORIC ACID

54.56 silex.
45.44 acid
100.00

I have endeavoured to ascertain what quantity of silicated fluoric acid gas a given quantity of water will condense. In one instance 1\(\frac{1}{6}\) of a cubic inch of distilled water absorbed 51 of sil. fl. a. gas; cubic inches, barom. 30.5, therm. 60. The gas was added to the water in a jar over mercury, as fast as it was absorbed. The experiment was stopped, when the gas, after having remained in contact with the water a whole night, ceased to be diminished. According to this result, the proper correction being made for the additional pressure, water decomposes about 263 times its bulk of silicated fluoric acid gas.

Dr. Priestley observed, that muriatic acid gas re-produced silicated fluoric gas from the crust of silex formed, when the latter is condensed by water*. This experiment I have repeated, and as it appears to show more correctly the quantity of gas water can condense, I shall describe the result. 2.4 cubic inches of muriatic gas were added to a drop of water, that had previously absorbed one cubic inch of silicated fluoric gas, in a jar over mercury. There was an immediate absorption equal to \(\frac{2}{5}\) of a cubic inch. The mixture of silex and subsilicated fluoric acid effervesced, and from an apparent solid became fluid, the whole of the silex gradually disappearing. After the first mentioned absorption, there was no farther. The gas produced was silicated, as appeared from the crust it deposited when removed to water, and the liquid formed was pure muriatic acid, for decomposed by concentrated sulphuric, it afforded merely muriatic acid gas, without any silicated fluoric. The evident conclusion from the preceding result is, that water condenses equal quantities of the muriatic and silicated fluoric acid gases, and consequently that the first estimate is too low, and instead of 263 times its bulk, it is probably more correct to say that water to be saturated requires at least 365 times its volume. Neither will this estimate appear inconsistent with the former results, when the deposi-

tion of silex is considered as an obstacle to the free exposure of the surface of the water to the gas.

Subsilicated fluoric acid is decomposed by ammonia and the fixed alkalies, and by all the earths that I have made trial of. It is also decomposed by the sulphuric acid and the boracic, as well as by the muriatic acid gas.

Of the particular changes which occur when it is acted upon by the alkalies, I defer giving any account at present, as it is my intention to do it in the next section.

To learn the effect of heat on it, a small quantity of strong acid, pure and transparent, was introduced into a retort connected with mercury. A spirit lamp being applied, about three cubic inches of silicated fluoric acid gas were produced. The neck of the retort was lined with silex in a gelatinous state, and much liquid subsilicated fluoric acid, that had distilled over, was condensed in the colder part of the neck, and was absorbed by bibulous paper previously introduced, to prevent the distilled fluid from entering the jar for the reception of the gas. When the whole of the acid in the bulb of the retort had been evaporated, little or no silex remained.

The general result of this experiment is very different from that which Dr. Priestley, who first made it, obtained. Instead of silicated fluoric acid gas, he procured "vitrilic acid air," sulphureous acid gas.

I have tried also the effect of heat on the silicious crust, formed by the decomposition of silicated fluoric acid gas, by water; but could obtain no sulphureous acid gas, as Dr. Priestley did only a small quantity of silicated fluoric.

The correctness of Dr. Priestley's observations cannot be doubted. I can only account for his results, by supposing that some sulphuric acid in consequence of the high temperature employed in making the gas was volatilized, and mixed with the subsilicated fluoric acid, and that mercury also was present, from the acid being prepared over this metal.

These experiments too oppose another statement relative to a method prescribed for making fluoric acid gas free from silex, by merely heating strong subsilicated fluoric acid in a retort, and collecting the gas over mercury. It is asserted, in chemical works of some reputation, that this process is successful. I have never found it so, having always obtained results
results similar to those above stated. This, I suppose, is one of the many errors that have secretly crept into repute, and has been believed, because never subjected to the test of experiment.

The action of concentrated sulphuric acid on subsilicated fluoric acid, is similar to that of muriatic acid gas, occasioning a disengagement of silicated fluoric acid gas. Facts which appear to prove, that water is absolutely essential to the existence of this acid.

Boracic acid decomposes it, in a very different way, not from Boracic acid any predominant affinity for the water, but in consequence of a stronger attraction for the fluoric acid itself. Silicated fluoric acid of course is not produced; but liquid fluoboracic acid and the silex is precipitated in a gelatinous state, as when ammonia is employed.

These are the principal facts I have to notice respecting this acid. Before I conclude, I shall briefly mention a few other circumstances. Applied to the tongue, in its concentrated state, it produces a very painful sensation, like that which strong muriatic acid does, and it has a very similar effect on the cuticle. It does not appear to erode glass, for I have kept it in bottles of this substance more than a month without any action being perceptible. Exposed to the air, it slowly and almost completely evaporates, there being only a very trifling silicious residue; and when gently heated in an open vessel, it is rapidly dissipated in white fumes.

Section II. On the Combinations of silicated fluoric acid Gas, and the subsilicated Fluoric, and the fluoric Acids with Ammonia.

M. Gay Lussac has shewn that silicated fluoric acid gas, like carbonic acid gas, condenses twice its volume of the volatile alkali.* The experiment I have several times repeated, and constantly with the same result, no difference appearing when the acid gas was added in great excess to the alkaline, or the alkaline to the acid. This being the case, and knowing the specific gravity of the two gases,† 100 parts by weight of silicated fluat of ammonia seem to consist of

* Vide Mém. d'Arcueil, Tom. II.
† According to Sir H. Davy, 100 cubic inches of ammonia, barom. 50, therm. 60, weigh 18 grains. It is this estimate which I have taken.
FLUORIC ACID.

24.5 ammonia
75.5 acid

100.0

Silicated fluid of ammonia volatilizes unaltered, if heated by a spirit-lamp in the vessel in which it is formed, and provided moisture be entirely excluded.

Like silicated fluoric acid gas itself, this salt is decomposed by water, and a similar precipitation of silex occurs, and in the same proportion. Thus the salt formed by the union of 30 cubic inches of silicated fluoric gas, and 60 of volatile alkali (barom. 30, therm. 60) in a small glass jar over mercury, being carefully collected and introduced into water, afforded five grains of pure silex, weighed after being well washed and heated to redness.

The saline solution, since part of the silex of the silicated fluoric acid gas is separated during its production, appears to be a subsilicated fluid, or a combination of subsilicated fluoric acid and ammonia. Another mode of making it, more directly proves that this is its composition. When ammonia is added to the subsilicated fluoric acid in excess, this salt is formed without any precipitation. From these facts, it may be concluded, that independent of water, which appears to be essential to its existence, 100 parts of it consist of

28.34 ammonia
71.66 acid

100.00

Subsilicated fluid of ammonia has a pungent saline taste. It just perceptibly reddens litmus paper. Slowly evaporated, it forms small transparent and brilliant crystals. The largest I could obtain, appeared to be tetrabedral prisms. The solid salt is very soluble in water; but is not deliquescent. When heated it appears to sublime unaltered. It is curious that the solution of this salt, when evaporated by a heat near its boiling point, powerfully erodes the glass or porcelain vessel, and a residuum of silex appears, on the addition of water, to re-dissolve the salt. This erosion and residue of silex I have seen produced three times following with the same quantity of salt. I mention the fact,
fact, which, I believe, was before observed by Scheele, without attempting an explanation of it. It may, perhaps be said, that as the water evaporates, the affinity of the subsilicated fluid for silex increases.

Subsilicated fluid of ammonia is decomposed by the sulphuric acid, and by muriatic acid gas, and also by the fixed alkalies and by ammonia.

Sulphuric acid expels from it, silicated fluoric gas and hydrated fluoric acid fumes.

Muriatic acid gas acts slowly on it, and effects its decomposition apparently through the medium of its water. A little of the crystalline salt was introduced into muriatic acid gas in a jar over mercury. In a short time, some silicated gas was produced, as the silicious deposition, on the addition of water, indicated. Strong muriatic acid was substituted for the acid gas. Now no apparent change took place, for on evaporating the acid, the residue, decomposed by sulphuric acid, afforded only silicated fluoric acid gas.

The alkalies form by the decomposition of this salt, the same compounds that they do by their action on subsilicated fluoric acid.

Potash expels the ammonia, and produces the silicated fluid and fluid of potash, as MM. Gay Lussac and Thenard have described.

The changes occasioned by soda appeared to me similar; but the gentlemen just mentioned, assert that this alkali precipitates the whole of the silex, and does not form a triple salt with it and part of the acid.

Ammonia seems to me to separate completely the silex, and by uniting with the pure acid to constitute a true fluid. MM. Gay Lussac and Thenard are of a different opinion. They say that the whole of the silex cannot by this method be removed, but only the principal part. Their reason for this belief, is, that on repeatedly evaporating the salt after the addition of ammonia and re-dissolving it, they have each time observed a residue of silex. If they employed metallic evaporating vessels, the results of my experiments do not agree with theirs; for making use of platina for this purpose, and adding an excess of ammonia, I never detected traces of silex on evaporating the filtered fluid. But our results agree, if they employed glass.
or porcelain vessels, which fluid of ammonia has the property of corroding.

(To be Continued)

II.


(Concluded from p. 334, Vol. XXXIII.)

But to return to our subject of the English measurement, if the uncertainty which yet subsists, with respect to the exact figure of the earth and its dimensions, occasions some small errors in the calculation of the series of triangles, the sum of these errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured. Now, in the English measurement, we find exactly the reverse of this. For the difference between the results of calculation and observation is only 1¹⁄₂₈ on the whole arc; but is even as high as 4⁰ 77' on one of the smaller arcs. So that, whatever error we may suppose to have been introduced into the calculation, by assuming a false estimate of the sphericity of the earth, or of other elements employed in the calculation, it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error, wholly independent of these elements.

It was not till the date of the measurement of the meridian in France, that M. Delambre published and explained, with admirable perspicuity and elegance, all the formulae and methods relative to the calculation of spheroids, and put it in the power of astronomers in general to make use of the elliptic elements in verifying the results of their observations. In the present state of science these elements are well known, and the errors that can arise from any uncertainty in them, are not so considerable as is generally supposed. The oblateness and the diameter at the equator are the only elements wanting in the calculation; for the purpose of seeing what effect
our present uncertainty respecting them can have on the subject in question, I have employed three different estimates of the oblateness $\frac{1}{3^o}$, $\frac{1}{3^o}$, and $\frac{1}{3^o}$. With respect to the radius of the equator, that is ascertained with sufficient precision by the mean of the arc extended from Greenwich to Formen- tera, corresponding to latitude 45° 4' 18''. The value of the degree in toises is 57010,5, and it is highly probable that in this estimate the error does not amount to so much as half a toise, as it is deduced from an entire arc of 12° 48' between the two extremities, the latitudes of which have been determined with extreme care, and by a great number of observations.

The following are the logarithms of radius at the equator, which I have employed as adapted to each degree of oblateness and opposite to them are placed the corresponding computed estimate of the entire arc between Clifton and Dunnoose.

$\frac{1}{3^o}$ $\ldots$ 6,5147,400 $\ldots$ $\ldots$ $2^o$ 50' 21,972
$\frac{1}{3^o}$ $\ldots$ 6,5147,485 $\ldots$ $\ldots$ $2^o$ 50' 21,974
$\frac{1}{3^o}$ $\ldots$ 6,5147,570 $\ldots$ $\ldots$ $2^o$ 50' 21,976

so that the greatest difference is but 0',38. Let us suppose it 0',4, or even 0',5, for the second calculation was made only by means of the western series of triangles, and the third only was the eastern; but even then the error arising from uncertainty in the elements is not half the difference we find between the results of computation and of observations of the fixed stars. It appears, therefore, that these elements are by no means to be neglected as a method of verification; and in fact the quantity of 1',38 is so small, that it is extremely difficult to ascertain this quantity with the very best instruments. Of this we shall find further proof hereafter; but as this discussion is not without its use, I shall enter into some details on this subject.

The measurement in Lapland was performed by means of the same double metre, and with a repeating circle of Borda, sent by the Swedish observations;
FIGURE OF THE EARTH.

bers. With these elements, and with the data to be found in
the work of M. Svanberg, we have by the western series of
triangles 5840°, 196 and 5840°, 138 by the eastern. So that
the mean calculated arc is 1° 37' 20", 167, while the arc ob-
served was 1° 37' 19", 566. The difference there is 0", 6 for
the total arc, and 0", 37 for the mean degree, or 5,86 toises
excess in the linear extent. One can never depend upon
quantities so small as this, so that the agreement between the
results of computation and actual observation, proves not only
the skill of the observers and the accuracy of which their in-
struments admit; but also that the elliptic elements employed
in the calculation are a sufficiently near approximation to the
truth to be deserving of confidence.

In the 8th volume of the Asiatic Researches, published by
the Society at Calcutta, are contained the details of another
measurement performed in 1802, by Major William Lam-
ton in Bengal, on the Coromandel coast. In this undertaking,
which was executed with great skill and attention, Major
Lambton employed Bengal lights as signals, chains for the
linear measures, and a theodolite, and a zenith-sector made by
Ramsden. The base measured was 6667,740 fathoms reduced
to the level of the sea, and to the temperature of 62° Fahren-
heit; and the stations were so chosen, that four of the sides of
the triangles were almost in the same line, and nearly parallel
to the meridian at the southern extremity of the arc, so that
their sum but little exceeds its whole extent. The lengths of
these arcs in fathoms reduced to the meridian are thus given in
the Memoir of Major Lambton.

AB 20758,13    north latitude of A 11° 44' 52", 59
BC 17481,245
CD 22237,04    north latitude of E 13° 19' 49", 018
DE 35246,43

From these data Major Lambton deduces the degree of the
meridian to be 60435 fathoms, or 56762,3 toises. By apply-
ing to this the same elements as we did to the measurement
by Svanberg, we have the entire arc measured equal to 1°
34' 55", 698; so that the difference between the results of cal-
culation and of the observations is only 0", 532 for the whole arc,
or 0", 337 for the mean degree. The elliptic hypothesis and
observation agree more correctly in this instance, for the diffe-
Figure of the Earth.

Reference is rather less than in that of Lapland, although the
two arcs are very nearly of the same extent. Thus the de-
gree on the meridian measured in Bengal, in the latitude of
12° 32′ 21″ north, cannot be supposed to exceed Major Lam-
bton's estimate by more than 5,22 toises; and it is extremely
difficult to speak with certainty to quantities so small as this.

The same observer also measured one degree perpendicular
to the meridian, by means of a large side of one of his triangles,
which cut the meridian nearly at right angles, and of which he
observed the azimuth at the two extremities. The data from
which his results may be verified are these:

Length of the chord of the long side in English feet AB = 291197,70.
Azimuth of the eastern extremity A equal to 67° 0′ 7″.54
NW.
Azimuth of the western extremity B equal to 267° 10′ 44″.07
NW.
North latitude of A 12° 32′ 12″.27
North latitude of B 12° 34′ 38″.86.

With these data in the triangle formed by the long side, the
meridian at A, and the perpendicular from B on the meridian
at A, we have the chord of this last arc equal to 290845,8
feet, and the arc itself 290848,03 feet. By applying the
method of M. Delambre, we find the azimuth of the extremity
B less by 2″ than it was observed to be; so that we have no
reason to suppose a greater error than one second in the obser-
vation of each azimuth, and it seems next to impossible to
arrive at a greater exactness.

The difference of longitude between the points A and B is
48° 57″.36. With this angle and the co-latitude at A, we have
in the spherical triangle right angled at the point A, the extent
of the normal arc equal to 2867,330 seconds, and dividing its
length in feet by this number, we have for the degree perpendi-
cular to the meridian, at the extremity A, 60861,20
fathoms, or 57106.5 toises. Now these values are precisely
what we find on the elliptic hypothesis, with an oblateness of
\( \frac{1}{4} \) or \( \frac{1}{4} \); and in short, the correspondence between the
hypothesis and the measures of Major Lambton, is as complete
as can be wished. Major Lambton, indeed, finds the degree on

the
the perpendicular too great by 200 fathoms, but this arises from a mistake in his calculation.

Lastly, I shall apply the same method, and see how nearly the elliptic hypothesis agrees with the last measures taken in France, which merit the highest degree of confidence, both with respect to the observers who have executed it, and the means which they had in their power to employ. I have taken only the arc between Dunkirk and the Pantheon at Paris, from the data published by the Chevalier Delambre in the 3d vol. of the Measurement of the Meridian. I employed the same elements and similar calculations to those made on the English arc. The oblateness of \( \frac{1}{36} \) gives the difference between the parallels equal to 7883.615 seconds by the eastern series of triangles, and 7883.617 by the western series. The mean of these 7883.616 may be taken as the true extent of the total arc.

The two other elements give for this quantity, 7883.621 and 7883.493, or 2\(^\circ\) 11\(^\prime\) 23\(^\prime\) 6 and 23\(^\prime\) 49, as the calculated extent of the arc. But the arc observed was 2\(^\circ\) 11\(^\prime\) 19\(^\prime\) 83, according to M. Delambre, and 2\(^\circ\) 11\(^\prime\) 20\(^\prime\) 85 according to M. Mechain; so that the least difference between the calculation and the observations will be 2\(^\prime\) 64. M. Delambre is of opinion, that the latitude of Dunkirk, which is supposed to be 51\(^\circ\) 2\(^\prime\) 9\(^\prime\) 20, should be diminished; and in fact the distance between the parallels of Dunkirk and Greenwich, which is 25241.9 toises, gives by the mean of the three assumed ellipticities 26 32\(^\prime\) 3 for the difference of latitude. After deducting this quantity from 51\(^\circ\) 28\(^\prime\) 40\(^\prime\) the supposed latitude of Greenwich, there remains 51\(^\circ\) 2\(^\prime\) 7\(^\prime\) 7 or 8\(^\prime\), for that of the tower at Dunkirk. If from this again we deduct the calculated arc 2\(^\circ\) 11\(^\prime\) 23\(^\prime\) 5, we have 48\(^\circ\) 50\(^\prime\) 44\(^\prime\) 5 for the latitude of the Pantheon, while, according to the observations of M. Delambre, it is 49\(^\prime\) 37, or 48\(^\prime\) 35 by those of M. Mechain. If various circumstances, with regard to unfavourable weather, and also others of a different kind connected with the revolution, and of which M. Delambre complains with much reason, have occasioned some uncertainty with respect to the observations at Dunkirk, still the numerous observations made at Paris, both by him and by M. Mechain at a more favourable season, and in times of perfect tranquillity, render the supposition of an error of 4 seconds in the latitude of the Pantheon
Figure of the Earth.

Pantheon wholly inadmissible. It is, however, too true, that such errors are possible, and it is only by careful perseverance, and by repeated verification, that they are to be discovered and removed, as we have seen to be highly probable with respect to the station at Arbury Hill.

But the same celebrated observer, M. Mechain, who handled instruments with great delicacy, and was possessed of peculiar talents for this species of observation, has given us an instance of singular irregularity in the observations made at Montjui and at Barcelona.

The latitude of Montjui, determined by a very long and regular series of zenith distances, is full 3°24 less than that deduced from a similar series of observations made at Barcelona, with the very same instruments, and with equal care. Moreover, there is reason to think, from other observations, that the difference between the observations at Montjui and at Barcelona, will probably be diminished still one second, so that the difference between Barcelona and Montjui:

between Barcelona and Montjui:

ascribed to local attractions;

but the deviations are of a contrary nature.

Moreover it does not follow that the latitudes of two places are correct, because the declinations of the stars deduced from them correspond; for the deviations caused by local attractions, or from any other source, are made to disappear in correcting
recting the Declination, but remain uncorrected in the latitude of each.

Lieut. Col. Mudge is also of opinion, that the irregularity in the value of his degree may be ascribed to deviation of the plumb-line, occasioned by local attractions. This is certainly very possible, and may be decided by an examination of all circumstances on the spot. But if there be really an error of 1" in the extent of the whole arc, this should rather be ascribed to some defect in the observations themselves, than to any extraneous source; for the observations of different stars give results that differ more than 4 seconds from each other.

I shall now conclude this Memoir, by expressing a wish, which men of science in England have it more in their power than any others to gratify; I mean by making new measurements in the southern hemisphere. Those which have been made hitherto in the northern hemisphere are 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; and hence we might expect the opposite hemisphere to be equally so, and to be a portion of the same curve. Nevertheless, the degree measured at the Cape of Good Hope by Lacaille, in latitude 33° 18' appears to indicate an ellipse of less eccentricity, or of greater axis; for the linear extent of 57037 toises, corresponds to the measure of a degree in latitude 47° 47' in the northern hemisphere. If now we calculate the arc as before, with an oblateness of \(\frac{1}{20}\), and with the sides of Lacaille's triangles reduced to the meridian, we find it greater by 10" than it was found to be by observations of the stars. An error of 10 seconds, by an astronomer so skilful and scrupulous as Lacaille, is too extraordinary to be admitted as probable. It is true, that there was a greater error well ascertained to have occurred in the measurement in Lapland, amounting to 13 seconds; but the academicians engaged in this undertaking were by no means equally conversant with observations as Lacaille.

There remains, therefore, but one method of removing all doubt on this subject, and this is to repeat and verify the measurement at the Cape, and, if possible, to extend it still farther to the north. 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, and would furnish us with a measurement in the other hemisphere, as much to be relied upon as the former. He would have the glory of deciding two important questions by his own observations; first, the similarity and magnitude of the two hemispheres: and, secondly, the degree of reliance to be placed on the elliptic hypothesis.

It might be still further desirable, if other measurements could also be undertaken, either in New Holland, or in Brazil; for though neither of these countries differs much in latitude from the Cape of Good Hope, they are so remote in longitude, that a correspondence of measures so taken would nearly establish the similarity of all meridians.

Note.

I shall now explain the formulae employed in deducing the results to which I have come in the foregoing Memoir. The demonstration of them is to be found in the work of M. Delambre, on the Meridian.

In the first place, let $a$ be the radius of the equator, $e$ the eccentricity, $\phi$ the latitude of one extremity of a side, or arc, in any series of triangles, and $\theta$ the azimuth of that side. The radius of curvature of this arc will be expressed by

$$\frac{1}{R_1} = \frac{1 + \frac{e^2}{1-e^2} \cos \theta \cos \phi}{R}$$

and

$$\frac{1}{R} = \frac{(1-e^2 \sin^2 \phi)^{\frac{3}{2}}}{a}$$

Hence we see that $R$ is the radius of the arc at right angles to the meridian. One may in general neglect the azimuth, and take the last radius for the radius $R_1$. Now, in computing the arc between Clifton and Dunnoose, I have supposed the oblateness to be $\frac{1}{330}$ or $e^2 = \frac{669}{330^2}$, and log. $a = 6,5147200$ expressed in toises.

The latitude of the southern extremity of the base is the same as that of Clifton, and its azimuth, if we choose to attend to it, is nearly 335° 23'. This base, considered as an arc of a circle, is reduced to its sine by the formula:

$$\log x = \log 1 = \frac{K \cdot e^2}{6R^3}$$

Vol. XXXIV.—No. 157. H
By means of the logarithmic sines of the base, and the angles of the triangles, considered as spherical, the logarithmic sines of the sides in the series were next computed, and then reduced to logarithms of the arcs themselves by the formula,

\[
\log s = \log \sin s + \frac{K \cdot \sin^2 s}{6R^2}.
\]

For the purpose of making this last reduction, it is sufficient to take a single value of \( R \), corresponding to the mean latitude of the entire arc 52° 2' 20''. It was thus that the table was formed of logarithmic sides considered as arcs.

Let \( m \) be one of these arcs, and let us represent by \( \psi \) and \( \psi' \) its value reduced to the meridian, the one in toises, the other in seconds of a degree; and we shall have the following formulæ:

\[
\begin{align*}
\psi &= m \cdot \cos \theta - \left( \frac{m^2 \cdot \sin^2 \delta}{2R} \right) \cdot \tang \psi - \left( \frac{m^2 \cdot \sin^2 \delta}{2R} \right) \cdot \left( \frac{m \cdot \cos \theta}{3R} \right) \cdot (1 + 3 \tan^2 \psi) \\
\psi' &= \left( \frac{\delta \psi}{R \cdot \sin 1'} \right) + \left( \frac{\delta \psi}{R \cdot \sin 1'} \right) \cdot \psi \cdot (1 + \psi) \cdot \cos \frac{\psi}{2} \cdot \left( \frac{3 \tan \psi}{R} \right) \cdot \left( \frac{\delta \psi}{R} \right) \text{; the superior sign being taken when the latitude } \psi' \text{ is greater than } \psi; \text{ and the inferior when it is less.}
\end{align*}
\]

The correction dependent on the convergence of the meridian for the azimuths is \( \delta \theta = \left( \frac{m \cdot \sin \theta}{R_1 \cdot \sin 1'} \right) \cdot \left( \frac{\sin \psi + \psi}{\cos \psi' \cdot \cos \frac{\psi}{2}} \right) \).

Hence the azimuth of the first station seen from the second and reckoned westward from the north, is \( \theta = 180' + \theta + \delta \theta \).

If \( P' \) be put for the difference of longitude between two points distant by an arc which measures \( m \), we have \( \sin P = \frac{\sin m \cdot \sin \theta}{\cos \theta} \cdot \log \sin m = \log \left( m \right) - \frac{K}{6} \cdot \left( m \right)^2 \), and

\[
\log P' = \log \left( \frac{\sin P'}{\sin 1'} \right) + \frac{6}{K} \left( \sin P' \right).
\]

The arc of the meridian, between Greenwich and Formentera, is so fortunately situated, that its middle point is in latitude 45°. Its whole extent measures 12° 48' 44'', and the distance between the parallels, in linear measure, was found to be 730430.7 toises. Hence the mean degree, corresponding to the latitude of 45° 4' 18'', is 57010.5 toises; and if we multiply
ply this number by 90°, we get one-fourth part of the meridian of the earth.

The correction to be deducted for oblateness is 58, 59, or 61 toises, according as it is assumed to be \( \frac{3}{2} \pi, \frac{5}{2} \pi, \) or \( \frac{7}{2} \pi \), and if we take the mean of these, we have the fourth part of the meridian \( Q = 5130886 \) toises; and hence the metre = 4430867 lines; so that the value of the metre turns out to be almost entirely independent of the elliptical form of the earth.

The radius of the equator is derived from the expression

\[ \log a = \log \left( \frac{2Q}{\pi} \right) + K \cdot \left( \frac{1}{2} \varepsilon + \frac{1}{2} \varepsilon^2 - \frac{1}{2} \varepsilon^3 \right), \]

the oblateness, and \( \pi \) the periphery of a circle = 3,1416.

In order to compare any degrees measured with those obtained on the elliptic hypothesis, we have a very simple formula. Let \( m \) and \( m' \) be the values of two degrees on the meridian, of which the mean latitudes are \( \frac{1}{2} \) and \( \frac{1}{2} \); in comparing the analytic expressions for these two degrees developing them, and then making \( \varphi = 45° \), we have \( m' = m \cdot (1 - \frac{1}{2} \cdot \rho \cdot \cos 2\frac{1}{2} + g \cdot \cos 2\varphi 2), \) \( m = 57010,5 \) toises, \( \rho = \frac{1}{2} e^9 \cdot \left(1 + \frac{1}{2} e^2 \right) \cdot \frac{\sin 1^\circ}{\sin 1^\circ}, \) \( \text{and} \) \( g = \frac{1}{2} \cdot e^4 \cdot \left(\frac{\sin 2^\circ}{\sin 1^\circ} \right) \).

And then we shall find that the oblateness \( \frac{1}{2} e^2 \) gives 57075,66 and 57192,38 toises for the degrees in England and Lapland.

I shall here subjoin one reflection more, which appears of importance. The oblateness of the earth is a quantity which varies considerably, by the least difference in the elements on which it depends. Accordingly, it is not surprising, that its value fluctuates between two proportions which differ sensibly from each other. To illustrate this, let \( \rho \) be the function which serves to determine the oblateness of the earth, so that \( \frac{1}{2} = \rho \). When this equation varies \( \Delta \rho = e^9 \cdot \delta \rho \).

Now the coefficient \( e^9 \) being very great, we see why the least variation in the elements of the function \( \rho \), occasions so considerable a variation in the denominator of the oblateness. This is precisely what happens in the lunar equations dependent on the figure of the earth, and which M. Laplace has deduced from his beautiful theory. Thus, for example, in the inequality that depends on the longitude of the moon's node, which he has determined analytically with so much precision.
the numerical coefficient found by Burg gives \( \frac{1}{3} \) for the
oblateness; but if this coefficient be diminished by \( 0'0'665 \),
then the oblateness becomes \( \frac{1}{3} \), so that a variation even to
this small amount in the coefficient augments the denominator
of the oblateness nearly \( \frac{1}{40} \) part.

The same happens with regard to the pendulum vibrating
seconds; for, supposing its length at \( 450 \) to have been cor-
rectly ascertained by MM. Biot and Mathieu, if we wish
to know the length of a second's pendulum at the equator,
corresponding to an oblateness of \( \frac{1}{3} \), we find it to be 439,1810
lines. Now this length differs from that determined by Bou-
guer only by \( 0,029 \) of a line, and M. Laplace even thinks
that the result of Bouguer should be diminished by about
double this quantity. We see from hence how much these
little differences, whether produced by errors of observation,
or irregularities in the earth itself, are liable to affect the deno-
minator of the fraction expressing the oblateness.

Fortunately, it seems probable, that the utmost latitude of
our present uncertainty is between the limits of 330 and 310,
and the mean of these may be considered as a very near ap-
proximation to the truth.

III.

*Critical Observations, on Dr. Wollaston's stated improvement of
the Camera Obscura and Microscope in the application of the
Meniscus, and Two Plano-Convex Lenses; proving their in-
ersority to the double Convex Lens generally used. By Mr.
William Jones, Optician.*

To Mrs. Nicholson.

SIR,

As my observations, published in your Journal, Volume 7,
proved, I trust, that the Periscopic Spectacle Glass, ad-
vertised by Dr. Wollaston, as possessing a new optical prin-
ple,
ple, and affording an improvement in the figure of a spectacle glass, was no other than the old rejected Meniscus Lens; contained no principle of refraction, different from the plano-convex, and double convex lenses; but, as it caused a greater aberration of the rays of light than those two lenses, was a worse form of lens for spectacles, or any other instrument, than the double convex lens generally used by practical opticians: It must, therefore, surprise others, besides myself, that Dr. W. should be induced again to propose the Meniscus, as an improvement in the Camera Obscuroa, by substituting it for the double convex lens, his account of which you copied last month into your Journal, from the Philosophical Transactions for 1812.

The desire I have to maintain an optical truth, and the duty I owe to our professional interest, obliges me again to point out to your readers, what I judge to be the error of his reasoning, and the fallacy of the inference.

In his description of the effect of the double convex lens in the common camera, page 27, he states the known effect of the images distant from the middle, or direct focus of the lens, being somewhat indistinct, on account of the plane of representation becoming, in distance, greater than the principal focus of the lens; and the oblique pencils of rays being refracted to a focus, rather shorter than the principal one. "On this account," he adds, "it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre." The aberrations of the lens add also to the indistinctness.

The collateral indistinctness in our portable chest Cameras, is but trivial and unimportant; and, in my opinion, the remedy, as above proposed, will be found by the artist to be worse than the defect, as the distinct and vivid central images will be vitiated, and the extreme images but little improved. The most perfect remedy is that which has been used by opticians in large cameras, for more than 50 years past, of placing a bottom board, or whitened table, with a concave surface, proportioned to the focal distance of the lens; which, corresponding very nearly
nearly to the focus of all oblique refracted rays, exhibits universally the images with the greatest brilliancy and distinctness.

The exact curve of the surface of this board or table should be that of a conic section, but the concave (spherical) one answers sufficiently well. It is necessary for the reader unskilled in optics to know, that what opticians name the axis of a lens, is that imaginary line that is supposed to pass through its centre, and is not subject to any refraction, and all other rays incident on the surface are refrangible, in proportion to the angle they make with this axis, those rays impinging nearest the centre of the lens, and with the least obliquity of position, are refracted with the most perfect images, or with the least aberration, in double convex, plano-convex, and meniscus lenses. The longitudinal aberration produces a focus short of the principal one, and the lateral aberration a confused lateral extension of the images, blended with prismatic colour. These aberrations increase directly with the diameter and thickness of the lens, and inversely with its focus. In lenses of large diameter, and short foci, these aberrations will, by experiment, be rendered very manifest, and which have been clearly demonstrated by that learned optician, Mr. Benjamin Martin, in his Elements of Optics, Dr. Smith, and others.

The subsequent paragraph, page 27, describes Dr. Wollaston's proposed improvement: the substance, in his own words, is as follows. "The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed, that its concavity is presented to the object, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted. The advantage of this construction over the common camera obscura is such, that no one who makes the comparison can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens any oblique oil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described,
the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For, since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E J, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius; and hence are made to converge to a point so distant, that the image (at J) falls very nearly in the same plane with that of an object centrally placed."

The radii of curvatures for a meniscus of 22 inches focus, Observations to show that the meniscus is being as two to one, is not essential. The theory of dioptrics shews, the greater the proportion, or the nearer that the radius inferior in its effect to the double convex lens will be. Dr. W. has not stated the diameter of the convex lens, but the reader must suppose it to be four inches, like that of the meniscus; nor has he told the reader what improvement would be produced, if he placed a similar circular opening, or limited aperture, also over the convex lens. I must, therefore, inform the reader, and he may himself prove it to be correct. The diameter or aperture of four inches is too great for a lens of 22 inches focus, either double convex, or meniscus lens, placed in a Camera Obscura, as it transmits too much light, and produces too much aberration for the most distinct representation of the images within the Camera. Dr. W., therefore, no doubt, was obliged to correct this palpable defect, by a curtailment of the area of his lens no less than three-fourths of the whole, and the lens would have been more like one applied by a skilful optician, if he had at first inserted a lens of about two inches diameter. The limited aperture, therefore, it is evident, advantageously excludes superfluous rays, but has nothing to do with the determination of their direction. Upon a fair comparison, the reader will not only doubt of the superiority of Dr. W.'s Camera, but be convinced of its absolute inferiority; for the double convex lens, under
Observations to shew that the meniscus is inferior in its effect to the double convex lens.

same diameter and focus as the meniscus, has less spherical surface, and consequently less longitudinal and lateral aberration of the two. Let us now advert to the transformation of the convex lens to become a meniscus, with the same focus; by considering their figures in the diagrams, the reader will perceive, that as much as the upper surface of the convex has been incurvated for a meniscus, so much the more has the convexity of the under side been augmented, to retain the original focus. The oblique pencils of rays first entering the meniscus, or any part of its surface, are from the immutable law of refraction refracted from the axis of the lens, contrarywise to the first direction on the convex, and afterwards in their passage into air, by the increased inferior convexity, refracted back towards the axis proportionally more than by the under side of the double convex to be converged to the same focal distance; and all pencils of rays that impinge on the surface in an oblique direction to its axis, must be united the same as by the convex lens, at a focus somewhat shorter than the principal focus from direct rays. The meniscus lens, in refractive property, differs not from the double convex one. The above explanation is agreeable to all writers on optics, and to correct experiment. In this meniscus, it is not "if the incidence," &c. but the incidence always is so oblique on the second surface, as to increase the convergence; and no kind of opening E whatever will change nature's laws of refraction, so as to elongate the focus, or to produce two different focuses in one lens; and his previous explanation of "occasioning all pencils to pass, as nearly as may be, at right angles to the surfaces of the lens," page 27, is an irrelevancy in optics, and is the error of reasoning that I imputed formerly to Dr. W. on his spectacle glass. It is the angle that the rays make with the axis of the lens, of whatever shape, that refraction is estimated from, as the science teaches us; not from the geometrical positions of pencils and surfaces. From the greater aberration that the meniscus possesses, the images formed by it will be less distinct, have less light, and be more distorted than by the double convex lens. It is from the extended lateral distortion, and bringing the meniscus nearer to the plane than its exact focus, that I can assign a cause how Dr. W. could have fallen into the error; had he placed the concave side, it would have been a better position, the images would
would have been more defined and enlightened; it was so applied in his spectacles, the convex side being next to the object: but in neither case will the images be so perfect and vivid, as by the double convex lens. The meniscus in a Camera is not a new application; several, some years back, were made for the purpose, but not preferred. I can refer to the machine now existing with one. I have caused two lenses to be ground, one a double convex, the other a meniscus, as Dr. W. directs, of the same diameter, nearly four inches, and focus twenty-two inches; which experimentally verify the correctness of my observations, and which any intelligent person may inspect, by application at our manufactory, 30, Holborn.

The following quotations may to some of your readers better corroborate the truth of my remarks.

"If the side were concave (of a plano) so that the lens became a meniscus, there is no proportion of the radii, or position of the lens, with regard to the radiant, but what will give the aberration greater than the plano convex in its best position; and, since this was first observed by opticians, the meniscus began to lose ground in the construction of optical instruments, and is now quite rejected." Martin’s Elements of Optics, 1759, page 29.

An oblique pencil of rays has its focus a little nearer the I (double convex) than a direct pencil. Cor. fig. 2.

This prop. holds good in a concave lens, and also in a meniscus, as well as in a convex one. Emerson’s Optics, page 124, prop. 24.

"When parallel rays fall upon the plane side of a plano-convex glass, the aberration of the extreme ray, which is of the thickness, is less than the like aberration caused by any meniscus glass whose concave side is exposed to the incident ray.

"When the said glasses have their convexities turned to the incident rays, the aberration of the extreme ray in the plano-convex, which is now but of its thickness, is less than the like aberration of any meniscus in this position."

The best of all double concave glasses has the semi-diameters of its first and second concavities as 1 to 6; and consequently, this is the best figure of a glass to help short-sighted persons, as the.
the double convex, one of the like figure is the best for spectacles." Smith's Optics, Art. 661, 662, 665.

"For since a meniscus, unless the surfaces of it are parallel to one another, has the same effect either that a convex lens, or a concave one would have, all the cases of diverging or converging rays that are refracted by it, will be the same with those already explained in the instances of convex or concave lenses." Rutherford's Philos. vol. 1, page 286.

"A plano-convex glass, with its convex side towards the incident parallel rays, has less aberration than any meniscus with its convex side exposed to parallel rays. Whence it necessarily follows, that that meniscus is best, which approaches nearest in shape to a plano-convex lens." Harris (of the Mint) Optics, 1776, p. 67.

So sensible have some optical glass grinders been of the impracticability and insufficiency of the meniscus glasses of short foci for spectacles, that I have in my possession some plano-convex and plano-concave glasses actually fitted in the frames, and sold for the new perisopic glasses.

Observations on the perisopic microscope.

The sort of French angle of reduction that Dr. W. has given, to obtain geometrically but nearly the radii of meniscus for a given focus, will be useless to the workman, as he already knows, by a very short arithmetical operation, how to obtain exactly such radii in half a minute's time, or a tenth part of the time necessary to construct that problem by Gunter's sliding rule, the time would be still shorter.

The combination of using two glasses in ordinary simple microscopes, or hand magnifiers, to diminish the errors arising from the spherical figure of one glass, was known to Sir Isaac Newton, and successive opticians. That late excellent practical optician, Mr. Ramsden, by the combination in the best position of two plano glasses, with their convex sides to each other, applied eye-pieces to his instruments with great advantage, to read off divisions of his circles, and magnify the wires of his telescopes, with clear definition at the circumference of the field of view, the diameters of the glasses being no smaller than the aperture of the tube. The same principle has since been advantageously applied to large object lenses for the lucernial microscope,
scope, by the late Mr. G. Adams, and ourselves, where the diminution of light was of less consequence than indistinctness of the image. In many cases the combination of two convex lenses answer very well; but the combining of two similar plano-convex lenses together, of superfluous diameter and thickness, and for the greatest defect or aberration in the worst position to each other; and afterwards to palliate it with a small aperture as shown in figure 4, is such an anomaly or absurdity in optics as not to require any serious comment on my part. I shall only appeal to the least experienced constructor of microscopes, whether he does not know, that the substitution of a double convex lens of the diameter only of Dr W.'s aperture, and of the same focus, would produce an image infinitely more perfect and vivid than the mutilated one proposed by Dr. W.

From these remarks I presume there will be nothing to apprehend from the attempt of Dr. W. to depreciate the excellence of the spectacles, Camera Obsuras, and Microscopes, as have been constructed by the most eminent Opticians of the day.

I am, Sir,

Your's, &c.

W. JONES.

Holborn, 16th Jan. 1813.

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IV.

Rules for discovering new Improvements, exemplified in the art of thrashing and cleaning grain; hulling rice; warming rooms; preventing ships from sinking, &c. By Oliver Evans, of Philadelphia.*

NECESSITY is called the mother of inventions; but upon Origin of In-quiry we shall find that reason and experiment bring them forth; for almost all inventions have been discovered by such steps as the following; which may be taken as a

* From the Appendix to his "Young Mill-wright and Miller's Guide" printed by subscription in Philadelphia, but very scarce in this country.
Step 1. Is to investigate the fundamental principles of the theory, and the process of the art or manufacture we wish to improve.

II. To consider what is the best plan in theory that can be deduced from, or founded on, these principles, to produce the effect we desire.

III. Consider whether the theory is already put in practice to the best advantage, and what are the imperfections or disadvantages of the common process of the art, and whether they can be evaded and the process improved; and what plans are most likely to succeed.

IV. Make experiments in practice to try any plans that the speculative reasonings may propose or lead to. Any ingenious artist, taking the foregoing steps, will probably be led to improvement in his own art; for we see by daily experience that every art may be improved. It will, however, be in vain to attempt improvements, unless the mind be freed from prejudice in favour of established plans.

Example 1. Suppose we take the art of thrashing grain.

Then by the rule.

Step 1. What are the principles on which this art is founded? The grain is contained in a head on the top of the straw enclosed in a husk, or chaff, that requires a force to break the hull, and disengage it; which may be done either on the principle of beating or of rubbing.

II. What is the best plan in theory for effecting this? As we find that it requires nearly equal force, and is all contained in the head, which is much less in quantity than the straw, theory directs the force to be regularly and uniformly applied to the head only, which will require but little power, seeing we can rub it out between our hands.

III. How is this theory put in practice; and what are the imperfections and disadvantages of the common process? The grain in the straw is laid on a plank floor, and beaten by men, with flails; or on the ground, and trod out by horses.

Disadvantages. The disadvantages are.

1st. The force is in both cases applied equally to the straw, as well as the head;
RULES FOR INVENTION.

II. Much force is lost, being unnecessarily expended in beating the straw, yet many heads escape undone, because the force is so irregularly applied.

III. In treading by horses the grain well as the straw gets dirty.

IV. Thrashing by men is both expensive and tedious. Now cannot improvements be made to overcome all these disadvantages? Such speculations have produced several.

First, a machine on the principles of a coffee mill, which requires very little force to rub the grain out of the heads, which are separated from the straw, by means of a machine on the principle of a comb, cutting them off. A machine to reap the heads without the straw is wanted to complete this theory, in countries where the straw itself is not an article of demand.

Secondly, a machine invented and put in practice by Colonel Alexander Anderson, of Philadelphia; the principles of which are to apply the strength of horses to strike the straw regularly with a uniform force, which finishes as it goes and clears the grain at the same time.

A Cylinder, 4 feet long, and 3 feet 6 inches diameter, with eight bats fastened to its circumference parallel to its axis, and of its whole length, is made to revolve with rapidity; the bats strike the straw at every fourth of an inch, it being drawn into the machine by and between two collars that move slowly. This machine makes great dispatch, but is expensive. (and destroys the straw.)

Others, attending to the principles of treading, have made a thing in the form of the frustum of a cane or sugar loaf, set full of cogs to act like the horses' feet. This is drawn by horses round a circular floor, adapted to it, on which the grain is laid, the centre of the circle being the vertex of the cone. This having considerable weight and many cogs, a horse will beat out much more with it than with his feet, because it will strike a great many more strokes with equal force: it has these advantages; it can be made by an ordinary carpenter—is cheap—and the dirt is not mixed with the grain, straw, &c.

Example II: The art of cleaning grain by wind.

By the rule.

Step 1. what are the principles on which the art is founded. Bodies falling through resisting mediums, their velocities are as
are acted upon by the air more than heavy ones.

Practical result. Blow the light bodies from the grain, by a current of air; deep; but not wide, across the stream. Let the grain &c. fall into compartments and use the same blast repeatedly.

Present practice. The grain does not fall through a suitable cavity; nor is it cleaned at one operation.

III. Is this theory in practice already, and what are the disadvantages of the common process? We find that the common farmers' fans drop the grain in a line 15 inches wide, 10 fall through a current of air about 6 inches deep, (instead of falling in a line ½ an inch wide through a current 3 feet deep) so that it requires a very strong blast even to blow out the chaff; but garlic, light grains, &c. cannot be got out, they meet with so much obstruction from the heavy grain. It has to undergo 2 or 3 operations; so that the practice is found to be no way equal to the theory; and appears absurd when tried by the scale of reason.

IV. The fourth step is to construct a fan to put the theory in practice, to try the experiment.*

Exp. III. Art of warming apartments.

Nature of fire not discussed.

Effects of fire not discussed.

II. Causes part to ascend;

III. particularly in the chimney, which pro-

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*This Machinery, with a large passage or channel, is useful to clean feathers from dirt and heavy bodies.—W. N.
II. Considering the principles, what is the best plan in theory for warming a room?

I. We must contrive the fire to spend all its heat in warming the air as it comes in the room.

II. To retain the warm air in the rooms, and let the coldest out first to obtain a ventilation.

III. Make the fire in a lower room, conducting the heat through the floor into the upper one, and leaving another hole for the cold air to descend to the lower room.

IV. Make the room perfectly tight so as to admit no cold air, but all warmed as it comes in.

V. By stopping up the chimney to let no warm air escape up it, but what is absolutely necessary to kindle the fire, a hole of two square inches will be sufficient for a very large room.

VI. The fire may be kindled by a current of air brought from without, not using any of the air already warmed. If this theory, which is founded on true principles and reason, be compared with common practice, the errors will appear the disadvantages of which may be evaded.

III. I had a stove constructed to put the theory as fully in practice as possible, and have found all to answer according to theory.

The operations and effects are as follows, viz.

1. It applies the fire to warm the air as it enters the room, and admits a full and fresh supply, rendering the room moderately warm throughout.

II. It effectually prevents the cold air from pressing in at the chimneys or crevices, but causes a small current to pass outwards.

III. It conveys the cold air out of the room first:—consequently,

IV. It is a complete ventilator rendering the room healthy.

V. The fire may be supplied in very cold weather by a current of air from without, that does not communicate with the warm air in the room.

VI. Warm air may be retained in the room any length of time at pleasure; circulating through the stove, the coldest entering first to be warmed over again.
VII. It will bake, roast, and boil, equally well with the common tin-plate stove, as it has a capacious oven.

VIII. In consequence of these philosophical improvements it requires not more than half the usual quantity of fuel.*

Example IV. The art of hulling and cleaning rice.

Step 1. The principles on which this art may be founded, will appear by taking a handful of rough rice, and rubbing it hard between the hands; the hulls will be brushed off, and by continuing the operation, the sharp texture of the outside of the hull (which through a magnifying glass appears like a sharp fine file, and no doubt is designed by nature for the purpose) will cut off the inside hull; the chaff being blown out, will leave the rice perfectly clean, without breaking any of the grains.

II. What is the best plan in theory for effecting this?*

III. The disadvantages of the old process are known to those who have it to do.

Example V. To save ships from sinking at sea.

Step 1. The principles on which ships float, are the difference of their specific gravities, from that of the water, bulk for bulk, sinking only to displace water equal in weight to the ship; therefore, they sink deeper in fresh than in salt water. If we can calculate the cubic feet a ship displaces when empty, it will show her weight, and subtracting that from what she displaces when loaded, will show the weight of her loaded. Each cubic foot of fresh water being 62.5 lbs. if an empty rum hogshead weigh 62.5 lbs. and measure 62 cubic feet, it will require 87.5 lb. to sink it. A vessel of iron, &c. filled with air, so large as to make its whole bulk lighter than so much water, will float; but if the air be let out and filled with water it will sink. Hence, we may conclude, that ships loaded with any thing that will float will not sink, if filled with water; but, if loaded with any thing specifically heavier than water, will sink as soon as filled.

II. This appears to be a true theory.—How is it to be put in practice, in case a ship springs a leak that gains on the pumps?

* The description will appear in a future number of our Journal.

† He describes a machine, which likewise deserves to be attended to, though less immediately connected with the industry of Great Britain.

I shall consider it. W. N.
III. The mariner who understands well the above principles and theory, will be led to the following steps:

1st. To cast overboard such things as will not float, and carefully to reserve every thing that will float, for by them the ship may last be buoyed up.

2nd. Empty every cask or thing that can be made water tight, and put them in the hold, and fasten them down under water, filling the vacancies between them with billets of wood, even the spars and mast may be cut up for this purpose in desperate cases, which will fill the hold with air and light matter, and as soon as the water inside is level with that outside, no more will enter: if every hogshead buoy up 975lbs. they will be a great help to sustain the ship, (but care must be taken not to put the empty casks too low, which would overset the ship) and she will float, although half her bottom be torn off. Mariners for want of this knowledge often leave their ships too soon, taking to their boat, although the ship is much the safest, and does not sink for a long time after being abandoned; not considering, although the water gain on their pumps at first, they may be able to hold away with it, when arisen to a certain height in the hold; because the velocity with which it will enter, will be in proportion to the square root of the difference between the level of water inside and out; added to this, the fuller the ship, the easier the pumps will work; therefore, they ought not to be so soon discouraged.

V.

Useful or Instructive Notions, respecting various objects. 1. Multiplying of Copies of Writing. 2. Scintillation of the Stars. 3. Large Achromatic Lenses.—W. N.

1. Art of Copying, or of multiplying Copies.

Every one is aware of the invaluable benefits which society has derived from the arts of printing, by moveable types, as well as by blocks and copper plates. But there are many cases, in which it would be of advantage to produce copies of writing, without requiring a stock of types or engraved plates; and the presses, or implements, by which the impression is made. A

saving, either in machinery, labour, or skill, is much to be desired. Under the present head, I have a few observations and facts to offer, relative to manuscript writing. The celebrated James Watt, about thirty years ago, obtained a patent for a copying machine, for making copies of the description, known by the name of counterproofs. His apparatus, consisting of a portable rolling press, a receptacle for keeping very thin unsized paper in a due state of wetness, and a peculiar ink more mucilaginous and less speedy in drying than common writing ink, is at present in general use, particularly in merchants' counting houses. In a former Journal it was remarked, that sugar or treacle, added to ink, gives it the disposition to come off upon wet paper, and that if the paper be well soaked, so as not to shine and yet to be considerably transparent, a very light pressure, such as that of a warmed flat iron, would produce the copy.

It is to be regretted, that this ingenious application should require as much apparatus and skill as it does; though its value is undoubtedly very great. The following process is less neat, but may be practised wherever a round ruler and gauze paper, or blotting paper, can be had. I have availed myself of it on a journey; in which it first occurred to me, as an expedient for copying letters.

The process.—Roll a piece of gauze paper upon a small sound ruler, and place the ruler, thus covered, upon the sheet of paper intended to be written upon, in such a manner as that the ruler shall be just above, and parallel to the intended first line; and the outer edge of the gauze paper on the same side as the upper edge of the paper. Then write the first line, and immediately upon concluding the same, roll the ruler just upon it; and the gauze paper will receive a print of that line. Return the ruler to its first position, write a second line, and take a print of that as before.—And in this manner the whole letter may be copied while writing. I found a little awkwardness at first, in bringing myself into the habit of this manipulation, which requires the writer to recollect, at the end of every line, that he is to apply the gauze paper; but this was soon overcome. And it may also be observed, that for a very light hand, which dries quickly, it would probably be needful to apply the ruler at shorter intervals. My hand writing, which is neither heavy
Useful Notices.

heavy or light, admitted of the operation being performed, as before described, but I could not defer it to any second line.

Another artist, of the name of Wedgewood, has, within a few years past, offered to the public, under sanction of Letters Patent, the engraver's method of tracing, by means of a piece of paper blacked with a pigment, (commonly lamp-black) applied by means of fat or a slowly drying oil. If such paper, which is sold at the shops, by the name of black tracing paper, be laid upon a leaf of common paper, and another leaf be laid upon that, the whole being disposed upon a firm flat table or plate of wood, or metal, or glass, and any writing be made with a small rounded steel or glass point, two copies will, by the same operation, be produced; viz. a reverse copy on the upper white paper, and a direct copy on the lower; the latter of which is sufficiently durable to be sent away to a correspondent, and the former will be very legible, as a direct copy, if the paper be thin.

Dr. Franklin mentioned to the Abbé Rochon* a method of rapidly engraving or marking plates, for multiplying copies. He wrote with gummed ink, upon a surface of hard stone or iron, and powdered his writing with sand, or emery, or cast iron dust; and when dry, he applied another plate of soft wood, or pewter, or copper, upon the surface, and forced the gritty matter into this last by the action of a press. This last served, in the usual method of copper plate printing, to give a very great number of copies, not neat or beautiful, but sufficiently legible.

The Abbé Rochon proposes, as a better method, to write with a steel point upon a copper plate ready varnished, and etch the face by aqua fortis. Reversed prints being taken from this etching, he piles these, while wet, along with other damped paper, and passes the whole through a press, which gives an equal number of counter proofs not reversed.

Both the last mentioned methods may be of use in armies and under other circumstances; but both suppose extensive means and apparatus, and only dispense with the engraver's skill. Perhaps it would be an addition to Rochon's method, that the

etching should be omitted, and the writing made upon soft metal with a sharp point leaving the bur on. Such a plate would afford many impressions.

It would be a great improvement upon Watt's method, if the counter-proofs could be taken upon dry paper. The tracing paper of Wedgwood and the engravers soon loses its colour, and it will not keep long. It soon becomes too dry to give off its colour.

2. Scintillation of the Stars.

Many speculations have been offered to account for and explain that apparently irregular and agitated emission of light, from the fixed stars, which has been called scintillation or twinkling. From its marked appearance at low attitudes, and almost total absence at higher, it has been commonly ascribed to the interposed atmosphere; which, by the changeable densities of its parts, and the interposition of opaque particles, is imagined to produce variations in the quantities, colours, and directions of the light before it arrives at the eye. In proof of this doctrine it has been farther noted, that the stars do not scintillate in a telescope. Undoubtedly the effect is still clouded with uncertainty. An observation I made upon the Dog Star (Sirius) in the autumn of 1807 may be considered as affording a few facts more in addition to those we already possess.

It is not true that the stars have no scintillation in a telescope. It may be strikingly observed by putting the instrument out of adjustment. In this case the circular disc of light, has a kind of oscillation, as if a number of discs were continually flashing before each other: the illumination seemed to come on at different sides, and these discs also differ in colour. Blue, steel blue, pea-green, bright copper, red and white, are among the most usual colours; but the rapidity of succession does not allow the sense to determine whether these colours may be more or less coetemporaneous, or completely distinctively succeeding each other. To determine this point, I took an achromatic glass of Ramsden's, magnifying 24 times, and directed it to the star—the object end being supported in a notch in a steady bar connected with the wall, and the eye end, upon an adjustable piece which was likewise capable of
being not very steadily. But upon this I rested my left hand, between the finger and thumb of which I held the eye end of the glass. In this situation, the glass being truly adjusted to distinct vision, I could observe the star, and by gently and rapidly striking the tube with the fingers of the other hand, I caused the image of the star to dance in the field of view, and describe the same kind of luminous line as is seen when a lighted coal is whirled about. The star was thus made to describe by each blow a curve returning into itself; but so contorted and irregular that no two successive curves were coincident with each other. The strokes were about ten in a second of time, and the curves were beautifully and distinctly tinged with different colours in their successive parts of different lengths: but it seemed at a medium that each of these vivid colours might occupy about one-third part or less of the whole curve, and upon my recollection those most predominant were greenish blue, steel blue, and maroon or an intense copper colour. The light from Sirius therefore as it arrived at the eye was by extremely sudden variations distinctly changed in its colour, at least thirty times in one second. No theory deducible from the known properties of the atmosphere, as an interposed medium, has yet presented itself to my mind, in a shape worthy of notice.

In the collection last quoted of Rochon, p. 380, he observes, that the scintillation of the fixed stars is an obstacle to measuring their diameters, and that when the light of Sirius was refracted into colours by a prism, it had no scintillation across the spectrum. As far as may relate to the apparent diameters of the fixed stars, the observations of Herschel do not seem to support the deduction of Rochon; but his fact appears to correspond with mine.

3. Advantage of upsetting or pressing in the borders of plates of flint glass to make the concave lens in achromatic combinations.

The same Abbé Rochon p. 372, remarks that the triple object lenses of Dollond of 3½ inches aperture, produce an effect equal to that which it seems ought to be obtained from the lenses of 30 or 40 feet, made by Campani. But that in making achromatic lenses of longer focus, the plates of glass being blown, are too thin to be worked without bending and spoiling the figures. All the cast glass he tried was found to be
Blown glass is superior to cast.

Blown plate glass may be worked up thicker.

Be more unequal in its quality, in different parts of the same plate, than the blown glass. It would not be difficult to explain this from the circumstances of the making; but the principal object of the present notice is, to mention that he succeeded in making a thick lens out of plate one quarter of an inch in thickness, by softening the glass by heat upon an earthen mould of the proper curvature, and upsetting or pressing the borders inwards, (taking care to avoid folds or wrinkles,) till the edge was an inch thick, and the diameter five inches. He then surrounded the glass by a metallic ring of six inches diameter, and three quarters of an inch deep. Within this ring he again heated the glass, upon which he previously placed an upper convex earthen mould. The glass thus obtained appeared very good, and when ground and polished, enabled him to make a triple object glass of seven feet focus, producing, as he says, a much greater effect than the glasses of Dollond, but without admitting of a proportionate aperture. For the lenses of that celebrated artist bore an aperture of 42 lines, and his lenses would not admit of more than 4 inches or 48 lines; which, however, adds more than one third to the whole quantity of light. From the great care in working, he did not think that the external parts of the lens were defective on account of the figure. The defect arose most probably from the flexure and contortion of the grain of the glass in pressing in. For an ingenious philosophical artist has assured me, that there is great difference in lenses and prisms made of the clearest plate glass; accordingly as the line of vision is directed at right angles to the natural plane, or more obliquely or coincident with it, the latter being in general good for nothing. Whence, and from other facts, he inferred that the layers of glass plates differ considerably in their densities.
An Account of some Experiments on the Congelation of Mercury, by means of Ether. By Alexander Marctt, M. D. F. R. S.

To Mr. Nicholson.

Mr. Leslie's new and ingenious mode of illustrating the well known fact of the production of cold by evaporation, by actually freezing water, in consequence of a rapid process of vaporization from the water itself, has already become a familiar experiment. Water is placed over an open vessel, containing sulphuric acid, and the whole being inclosed within the receiver of an air pump, the water cools as the exhaustion proceeds, and is ultimately converted into ice. I have learnt also, that Mr. Leslie has succeeded in freezing mercury by a similar process; that is, by investing the bulb of a mercurial thermometer with a thin coat of ice, and exposing this to the joint effect of exhaustion and of sulphuric acid.

After trying to repeat the last of these experiments, (an attempt in which I did not succeed) I effected the congelation of mercury with great facility and quickness, simply by substituting the evaporation of ether, instead of that of water, in the process in question. I am not aware of having been anticipated in this experiment; if I have, you will oblige me by taking no notice of this letter; but, in the contrary case, I shall thank you to give it a place in your Journal.

The mode in which the experiment is made is this: a conical receiver, open at the top, is placed on the plate of the air pump, and a mercurial thermometer is suspended within the receiver through the aperture. This is done, like some of the well known pneumatic experiments, by means of a brass plate perforated in its centre, and fitting the receiver air tight when laid upon its open neck. The thermometer passes through this plate to which it is carefully fitted by a leather adjustment, or simply by cork, secured with sealing wax; and it is so graduated, that when its bulb is sunk a few inches within the receiver, the stem rises externally through the plate, above which the scale begins...
CONGELATION OF MERCURY.

begins. The bulb is then wrapped up in a little cotton wool, or what is better, in a little bag of fine fleecy hosiery; and after being dipped into ether, the apparatus is quickly laid over the receiver, which is exhausted as rapidly as possible. In two or three minutes the temperature sinks to about 45 below 0, at which moment the quicksilver in the stem suddenly descends with great rapidity, (in consequence of the remarkable contraction which the mercury in the bulb undergoes in congealing) to a distance corresponding to between 300 and 400 degrees. This, however, seldom happens to that extent, because the descent of the mercury is often impeded by the freezing of the column itself at the entrance of the bulb, before the congelation within the bulb is completed.

If it be desired to exhibit the mercury in its solid state, common tubes may be used, which should be broken instantly after being removed from the pump. I have frozen in this way bulbs of an elongated shape, about an inch in length, and near an inch in diameter. The pump I have used for these experiments is one of a small size;* the gage of which stands at about a quarter of an inch, when the exhaustion is pushed to its utmost extent. I have occasionally succeeded in this experiment, when the temperature of the room, as well as that of the ether, was about 50°; but the certainty of success is much increased by operating in a room, the temperature of which does not exceed 40°, and by previously reducing the temperature of the ether. I have been in the habit, in making this experiment, of inclosing sulphuric acid within the receiver, as in Mr. Leslie's process, as it has appeared to me to promote the evaporation of the ether, and the production of cold; but the experiment has also succeeded without the assistance of sulphuric acid.

The same experiment may be varied by first dipping the bulb of the thermometer, surrounded with cotton wool or flannel, into water, and after freezing this by means of the pump, pouring a few drops of ether upon the frozen bulb, and exposing it again to the effect of exhaustion. This plan has sometimes

* Made by Mr. Bate, instrument maker in the Poultry.
succeeded when circumstances were not sufficiently favourable for the success of the other.

I have applied a method, similar to those just described to the freezing of water, by means of the ingenious instrument imagined by Dr. Wollaston*, to which he has given the name of chryophorus. This instrument consists in a tube, terminated at each extremity by a ball, like the common pulse glass, one of these being full of water, and both the balls and tubes being completely exhausted of air. By plunging the empty ball into a mixture of salt and snow, the water in the other ball, though at some inches, or even some feet distance from the cold mixture, is frozen in a few minutes. But by a process, similar to that I have just described, for the congelation of mercury, the same may be effected without any cooling mixture in less than one minute, and with a pump of very moderate power. I may take this opportunity of mentioning, that having constructed an apparatus of this kind, with a thermometer within it, I observed that the temperature of the water sunk to 20°; and, in one instance, even two or three degrees lower before it froze, which I at first ascribed to the water being deprived of its air by previous boiling; but the same circumstance not having uniformly taken place, when the shape and size of the apparatus, and the quickness of the process, were varied, I am now inclined to ascribe it to other causes.

I have the honour to be, &c. &c. &c.

ALEXANDER MARCET.

Russell Square, 22nd Jan. 1813.

VII.

Observations upon the best state in which it is advisable to bring the British Merino Wools to market. By EDWARD SHEPPARD, Esq. of Uley, in Gloucestershire.

MR. SHEPPARD has made his title good to that fame Introduction, which attends the patriotic and well-directed exertions

* This apparatus was described a few weeks ago, by Dr. Wollaston, in a paper which was read before the Royal Society, an abstract of which was published in the 1st number of Dr. Thomson's Annual Philosophy.
of so many of our country gentlemen, in improving our valuable stock of first materials. Wool has, for centuries, been considered as one of the first, and Mr. S. has claimed and received the Gold Medal of the Society of Arts, for having produced from his flocks of 1929 Merino and Merino Ryland, the whole bred and kept by him, 7749 lbs. of wool in the year 1812. He has communicated the following observations to the society.

Having had the experience of more than ten years, both in the growth and manufacture of British Merino wools, which, by the constant use of the Spanish rams that came into his Majesty's possession during that period, I have brought to very great perfection; I take this method of making public the result of my observations, as to the mode most profitable for the grower and manufacturer, to prepare the Merino wools for the market; as considerable difference of opinion and practice prevail on the subject.

I had the honour, in the year 1806, to present a memoir to the Board of Agriculture, in a successful claim I made, for the Gold Medal given for the greatest quantity of fine wool, grown within the year. I therein stated my opinion, that the principal cause of the superior and characteristic softness of the Saxon and Anglo-Merino wools, was, their remaining in their native grease, without its being expunged in the extreme degree practised in Spain. Excepting the moderate washing that Saxon and British wools receive on the sheep's back before shearing; they continue in their grease till they are worked up by the manufacturer; while the wools in Spain, as soon as shorn, are thoroughly scoured, by an injudicious process, and then exposed for days to a burning sun, in which brittle and hard state they are so closely packed up, that they come out of their bags here, almost as much pressed and hard as hops, wholly deprived of that unctuous preservative, which I conceive to be necessary to the soft feel of wool.

It has been thought by some, that Saxon and Anglo-Merino wools have a softness peculiar to themselves, and different from the Spanish, their parent stock, obtained from their cross...
cross with another and coarser woollen sheep. I am, however, very much disposed to attribute the quality here spoken of, to the better management of the wools in this country. Unfortunately, we have no opportunities of discovering what Spanish wool would be preserved in the grease; as the mode of laying on the duties at Burgos, by the pound, prevents the grower or merchant exporting it in that condition. Otherwise, I am much inclined to think the same softness would be found in the pure parent fleece, as in the spurious offspring. From the small experience afforded by the ill-conditioned fleeces lately imported with the sheep from Spain, I am very much confirmed in my opinion.

Lambs’ wool, not being so completely washed from its grease in Spain as sheep’s wool, comes very near to the softness of the Saxon and British lamb’s wool. As a proof of their possessing an extra quantity of grease, they are much sooner liable to breed the worm than Spanish sheep’s wool. I have often proved, in the manufacture of wool, that where it has been long saturated with oil, artificially, the fibre has been lubricated with it, and the cloth very superior in feel and softness.

It has long been known to manufacturers and wool-staplers, that the wool of dead sheep, or Vell-wool, as it is called, is very harsh, and quite unlike the same wool shorn from the sheep’s back, occasioned by its being disengaged from the skin, by the fell-monger, by the action of lime, which entirely dries up and destroys the oily particles. May it not, in some measure, arise from the cause, that wool from sheep used to calcareous or silicious soils, is of a harsher description; as those from the Sussex, or Wiltshire downs, when compared with the fleeces grown on the argillaceous lands of Hereford and Shropshire? The absorption of the native grease, by the frequent contact of the sheep’s coat with the soil, and the dust from it, may help to remove that great preservative of softness, and leave the fibre exposed, unprotected by moisture, to the action both of the sun and rain, which, in those exposed situations, would act with double power.

From the above theory I would wish to deduce a few inferences, which may be of service in the growth and management of British fine wools. In the first place, I am satisfied that nothing can so much tend to preserve this necessary state of
of native grease, as the protecting the fleece from the humidity and inclemencies of climate. In a country where such exist in any great degree, it would be requisite, in order to attain and preserve a superior degree of fineness, that the sheep be housed in the winter, as practised in Saxony, and the northern parts of Germany, where they not only cot them in the winter, but drive them under cover at every thunder-storm in summer. The frequent washing of a sheep's coat, will very much deprive it of its grease, as is evident from comparing the external part of the fleece with the internal. The same comparison will show how greatly such washing has impaired its fineness. The closeness of the coat of the Spanish sheep; compacted as it is, by its vast diffusion of grease, into almost a coat of mail, prevents the admission of the rain infinitely beyond that of any other sheep we know of; and accordingly protects the quality of the wool longer from deterioration. But even the Spanish fleece, by constant exposure to a humid climate, and to driving winds, and rains, will be penetrated, and every year become more open and hollow, and less tenacious of its native grease, and, in proportion, less fine.

My opinion, as to the best mode of preparing Merino wool for the market, is, that where a certain and ready sale offers, it should be left wholly in its native grease, without being washed on the sheep's back. This further advantage attends it, that the fleece is much more captivating to the eye, and the fibre appears much more silky and fine. I fear, however, that there is not, at present, that quickness and certainty of sale, which will permit the grower to produce his wool in this condition. For if they have a chance of lying a long time in the grease, they will heat and be injured. I cannot, therefore, recommend it as a general practice, but I think where wools are likely to be used within six months of shearing, there can be no objection to keeping them in the full grease. I have, however, the satisfaction to state, that by the moderate degree of ablation which takes place in washing the wool on the sheep's back, this grease is not expunged in a degree to injure the softness of the fibre. The same mode is practised in Saxony, and is altogether different from the complete washing in hot and cold water, which the wool receives after being shorn in Spain.
The waste on British Merino wool, which has never been Clean wool washed on the sheep’s back, is rather more than one half, or weighs only about 10 lbs. in 20, reckoning to its clean picked state. The same wool, when washed on the sheep’s back, loses with the wool in the manufacturer about one third, or from 6 to 7 lbs. in 20, which is about the average of the waste of Saxon wool. Whereas, the best imported Spanish wools will not waste more than half that amount: viz. from 3 to 4 lbs. in 20. It is obvious, that a proportionate difference must be made in price, for the different conditions in which British Merino wools are produced; the manufacturer will be better able to estimate the probable waste of the wool that has been washed on the sheep’s back, as there is so much dirt, sand, and filth, generally with the wool in its genuine, unwashed state, that the waste must be always uncertain. I think, therefore, that wool washed on the sheep’s back will be the most merchantable.

I would also remark on the most preferable mode of managing the lamb’s fleece, which I should recommend cutting in preference to remaining on the lamb, till he becomes a yearling, as practised by many. The external part of the hog’s fleece, which was the original lambs-wool, suffers most materially from the inclemency of the weather and the winter. In its state of lambs-wool it is beautifully soft, but being afterwards protruded from the new coat, it is in that condition exposed to the snows, winds, and rains of the winter, by which it becomes entirely deprived of its grease, and as coarse as the wool of our common country sheep. The deterioration of this exposed part of the fleece, in one season only, fully proves what effect climate and weather have on the fibre of wool; it is therefore certainly desirable to shear the lambs, as in Spain; and although the covering may be more complete for the young sheep against the winter with the lambs coat on, yet the being rid of the incumbrance of a wet draggled fleece, in deep soils and bad weather, is of great advantage to the young and tender sheep.

EDWARD SHEPPARD.

Uley, Gloucestershire, March 5, 1812.
General Results of Beccaria's Observations upon the Electricity of the Atmosphere during serene weather; together with those of Romayne and Henley. Abstracted by a Correspondent.

(R. B.)

To William Nicholson, Esq.

SIR,

AFTER the systematic arrangements of clouds by M. Luke Howard, and his speculations upon their formation and disappearance, which I consider as having greatly enlarged and regulated our knowledge and means of making atmospheric researches,—and particularly from the probability that the disposition, and even the notions of clouds, may be in a great measure referable to the ordinary phenomena of electrified bodies, I have thought it would be of service to the inquiries of other observers, to send you an abstract which I made for myself, of the facts and remarks of these very diligent and faithful observers; whose works, from their extent, their dispersion, and even their date, though well esteemed by philosophers, are at present less likely to be referred to. At all events, I submit to your judgment, and am, without farther preface,

Sir,

Your most obliged reader,

R. B.

The numerous and important observations of Father Giambatista Beccaria, on Atmospheric Electricity, render his conclusions on this subject highly estimable. His treatise annexed to the English translation of his Artificial Electricity deserves to be consulted. At present, I shall do little more than give his propositions or general results.

The apparatus by which those results were obtained, was settled on the pleasant hill of Garzega, in the neighbourhood of Mondovi; from which the whole compass of the Alps, as well
well as the whole plain of Piedmont is easily discovered. It consisted of an iron wire one hundred and thirty two French feet long, extending from a stack of chimneys, over which it was raised by a long pole to the top of a cherry tree. Its extremities were insulated and defended by a small umbrella of tin, covered beneath with sealing wax. From this wire, another was introduced into a room through a pane of glass. It was found

1. That the electricity, during serene weather, in its ordinary or mean state, causes two balls of pith of elder one line in diameter, to diverge six lines from a small plate of metal placed between them. The balls were suspended by very fine threads, sixteen lines long. 2. In the state of its greatest intensity the divergence of the balls is fifteen, twenty, or more degrees from the metal.

3. In its weakest state the balls move towards a conductor at a very small distance. 4. The electricity is sometimes so slow in its accumulation as to require one minute to become again sensible, after having been taken off by touching the wire; but at other times it became again sensible in the time of one second.

5. That it is always of the positive kind, excepting in some very rare instances, when the contrary happens, in consequence of the wind blowing from some other part of the sky which is not serene. The instances related by Beccaria are very curious.

Father Beccaria used an hygrometer consisting of a string of thirty-two flaxen threads twisted together to the thickness of two thirds of a line. It was twelve feet long, and the lower part passed round a pulley which carried an index. The stretching weight was two pounds. Such an hygrometer commonly served him a year, and he distinguished smaller mutations than it was capable of shewing by means of another hygrometer made of a twisted rye-stalk.

6. During clear weather the moisture in the air is the constant conductor of the atmospheric electricity; and this electricity is proportioned to the quantity of that moisture which surrounds the wire, except such moisture lessens the insulation both of the wire and of the atmosphere.

Beccaria observes, that he does not here pretend to point the cause or principle which produces the electricity, but only to ascertain the medium in which it is inherent, and to the quantity of which it is generally proportioned.

7. The
7. The electricity that takes place when the weather clears up is always positive. When the air takes up moisture very rapidly the intensity of the electric state of the wire, as well as its quickness in becoming again sensible when destroyed are great; but the latter diminishes as the weather becomes dryer. It sometimes happens that the electricity thus caused continues a long time in its state of intensity, and begins afresh after being interrupted. Beccaria thinks these effects are owing to electricity being brought from great distances by the wind.

8. If the sky becomes clouded over the place of observation, and only an high cloud is formed without any secondary clouds under it, and the cloud be not part of a cloud that drops rain elsewhere, then the electricity of the wire is either positive or null. But if the clouds resemble locks of wool moving to and from each other; or if the general cloud is forming very high and is stretched downwards like descending smoke, then a frequent positive electricity commonly takes place, which is more or less strong in proportion to the quickness with which the cloud is forming, and foretells the quantity and suddenness of the rain or snow which follows. 2. When a rare, even, and extensive cloud is forming, which darkens the colour of the sky, and renders it grey, positive electricity, very intense and speedily recovering its intensity when taken off, is produced; which state diminishes and even fails as the gathering of the cloud slackens; but on the contrary, if the cloud continues to increase gradually by the accession of smaller clouds, resembling locks of wool which are continually joining and separating, the positive electricity usually continues. 3. Low and thick fogs (especially when they rise into a superior air considerably free from moisture) carry up to the wire electricity which gives frequent small sparks, and the balls diverge between 20° and 30°. If the fog seems stationary and continues to envelop the wire, the electric signs soon disappear; if it continues to rise and another cloud of fog succeeds, the wire is again electrified, though less than before. Sky rockets sent through such thick low and continued fogs have often afforded our celebrated observer signs of electricity by means of a string affixed to them. He never, however, observed in any of the above circumstances, any signs of negative electricity except once by a sky rocket sent through a fog, in which he saw the
star of electric light denoting negative electricity, but thinks that he might have mistaken its figure.

As Father Beccaria in this place mentions his two fellow labourers, Romayne and Henley, I shall here take occasion to notice their observations, and then resume my subject.

Mr. Romayne* made his experiments between the year 1761 and 1772. He held an electrometer, consisting of two cork balls, suspended by threads six or seven inches long out of a garret window, by means of a pole five feet long; and to these, when electrified, applied excited glass, or sealing wax, by the help of another pole, and by that means determined the kind of electricity.

He found the air at a proper distance from buildings, ships' masts, &c. to be very sensibly electrified during winter, in foggy or in frosty weather; less so in mists, and still less in calm and cloudy weather. But in summer he never observed any electricity, except during a fog in the cool of the evening, or at night. He never found any electricity during the time of an aurora borealis, unless a fog happened at the same time; excepting once, and then it was weakly positive.

He always found the electricity of the air to be of the positive kind; excepting once only, during a fog, on an uncommonly warm day in winter.

When a fog became very thick, he observed that the cork balls came nearer to each other, but opened again on its recovering its former state; and he also found, that rain during a fog produced the same effect, which ceased as soon as the rain was over.

Mr. Romayne also observed that the smell of fogs, and frequently of the common air, resembles that of an excited tube. He observes, that when the density of fogs floating near the earth increases considerably, the balls always approach; but that the reverse generally happens when the fogs are high in the air. He once saw a struggle between breezes from N. W. and S. E. at the same time in which the one seemed sometimes to prevail and afterwards the other. The contention was preceded by a smoky haziness, like a fog, which occasioned the balls to diverge; as the haziness thickened they separated more, and the

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* Phil. Trans. Vol. LXII. p. 137.
repelling power was augmented in proportion as the drops increased.

On this occasion, M. Romayne was the first who made an elegant experiment, to shew, that the diminution of surface increases the intensity of electricity in bodies. He found, by repeated trials, that a piece of flannel, silk, &c. excited and suddenly twisted, not only struck at a greater distance than before, but sometimes emitted parcels of fire into the air. And from this he infers, that the electricity of vapour, when not in contact with the earth, ought to increase by condensation. This is still farther confirmed by the experiments of Volta and of Bennet, on the electricity of vapour.

At other times, M. Romayne made use of a tapering tube of tin, twenty feet long, and ending in a point, insulated, and projecting upwards out of a window. He took notice of that uncertainty and frequent change in the electricity of clouds, which was before remarked by Dr. Franklin and others; and, after several ingenious observations, he expresses his wish, that two or more persons, at a sufficient distance, would correspond by signals, indicating positive electricity by a red flag, and negative by a blue; as it is highly probable that much more satisfactory knowledge would be thus obtained, respecting the electricity of the clouds, thunder, &c. than any single observer could acquire.

The observations of Mr. Henley tend to corroborate those of Mr. Romayne, but do not lead to any further conclusions.

I now proceed in the enumeration of general facts, or the propositions of Beccaria.

9. In clear weather, when a low cloud, considerably distant from any other, happens to pass slowly over the wire, the positive electricity is usually much diminished, but is not rendered negative; and, when the cloud is gone, it returns to its former state. But, if numbers of whitish clouds, resembling locks of wool, continually uniting and separating, remain over the wire, so as to form a considerable extent, the positive electric...

* And more fully by the condenser and well-known experiments, made with Bennet's gold-leaf electrometer.

† Phil. Trans. vol. 62. p. 145, and vol. 64. p. 422.
ATMOSPHERIC ELECTRICITY.

The electricity never becomes negative in either of the above cases.

Father Beccaria, in his experiments on the electrified air of a room, found that the electricity is proportional to, and therefore most probably resides in the vapours floating therein. The same conclusion may, therefore, be observed, and naturally applied to the atmospheric electricity, which is not sufficient in general to produce electric figures, in electrometers which are not insulated. The two last propositions, 8 and 9, relate to such phenomena as take place when the weather either becomes overcast or clears up. The following relates to the effects of vapour or moisture, as shown by the hygrometer.

10. In the morning, if the hygrometer indicates a great degree of dryness, very little difference from that of the preceding day; then even before sun-rise an electricity takes place, causing junction, adhesion, or divergence, of the ball; and its intensity is greater the drier the air, and the less that dryness differs from that of the preceding day. But if no such great dryness obtains, no perceptible electricity takes place, till sun-rise, or a short time after.

11. The electricity of the air gradually increases as the sun rises higher. The gradual increase begins sooner, according as the hygrometer continues after sun-rise to indicate a higher degree of dryness, and as such dryness more speedily increases. This increase, both of intensity and speedy recovery, when taken off, last in serene days, when the wind is not violent, till the sun draws near its setting, provided the hygrometer keeps near the highest degree it has reached. But when the sun is near setting, and in proportion as the hygrometer retreats, the intensity of the daily electricity is diminished, at the same time that the quickness with which it is revived in the apparatus, when taken off, becomes greater.

12. Though the hygrometer may indicate equal degrees of difference in dryness in the middle of the day, on different days, yet the time in which the apparatus recovers its electricity on those days is less, the greater the increase of heat; and when the heat is greater, the electricity arises later in the morning, and fails sooner in the evening.

13. The friction of winds against the surface of the earth is not the cause of atmospheric electricity. Impetuous winds diminish.
nish the intensity of the electricity of clear weather. And if they be damp they diminish its intensity, by rendering the insulation both of the atmosphere and of the apparatus more imperfect.

Father Beccaria made many experiments to discover whether the friction of air against conducting bodies produced electricity. He used the bellows, and also turned fans of gilt pasteboard very swiftly round on an insulated axis, but obtained no electricity either in damp or dry weather. He had before observed, that air produces electric signs, when it strikes their glass*. He found also that the umbrella, with an insulating handle, which the French call paratonneres, never exhibited the least electricity when held obliquely to the wind. To these I may add, that the very sensible electrometer, of Bennet, does not become electrified by blowing pure air upon it†. The proposition of Beccaria does not, however, rest upon electrical experiment, but is likewise supported by a variety of actual observations on the state of the atmosphere. And though these cannot be transcribed, on account of their length, yet I am unwilling to pass over in silence his very cogent remark, that if the electricity in any degree arose from the friction of winds against the ground, it would be found the greatest near the surface of the earth, but the contrary is the fact.

XIV. In cold weather, if the sky be clear, the wind not violent, and the air considerably dry, an electricity of considerable intensity arises after sunset, as soon as the dew begins to fall. The quickness with which the apparatus recovers its electricity after being touched, is greater than during the diurnal electricity, and it disappears very slowly.

XV. In temperate or warm weather, and in the same circumstances of wind and moisture, an electricity perfectly similar to the above takes place as soon as the sun has set; but its intensity is not so constant, it begins with more quickness, rises to a state of more speedily recovering its intensity after being touched, and ends sooner.

XVI. When the air in the above circumstances is less dry, the electricity is less intense, by reason of the insulation being rendered more imperfect, but its quickness in recovering its

† Ph. Trans. vol. LXXVII. p. 30.
intensity after contact, is greater, as the quantity of dew is greater.

XVII. The electricity of dew seems to be, in proportion to its quantity, in the same manner as the electricity of rain depends on its quantity; and the peculiar manner or circumstances which attend the falling of the dew, influences the electricity in the same way, as does the peculiar manner in which rain takes place.

XVIII. As rain, showers, aurora borealis, zodiacal light, have a tendency to begin afresh for several successive days, with the same characteristic accidents, so the electricity of dew seems to have, as it were, an inclination to appear for several evenings successively, with like characters.

After these propositions relating to the dew, father Beccaria adds the following: let the air, in a closed room, be electrified, that is to say, the moisture and other vapours diffused in it; let a bottle filled with water, colder than the air of the room, and insulated on stove of glass, be raised pretty high in the room, and the insulation be carefully preserved. Then the electric signs that will arise in two threads suspended to the bottle, will exactly represent the electricity of dew, for they will exhibit the different manners after which this electricity takes place, according as the electrified vapors in the room are more or less rare; as the difference between the heat of the bottle, and of the air in the room, is more or less; and as the insulation of the bottle is more or less accurate.

This excellent and most industrious philosopher, after reciting various facts respecting the electricity of dew, concludes with the following summary observations:

The diurnal electricity resembles the electricity of a very rare fog, which rises, becomes dilated, and by that means, continually renders the insulation more perfect. The nocturnal electricity resembles that of a very rare and subtle rain, which descends, becomes condensed, and continually renders the insulation less perfect, whenever the diurnal electricity is more constant. But the nocturnal electricity frequently fails, and only attains its greatest intensity when the increase of that moisture, which is the conductor of it, happens to take place without injuring the insulation.
IX.

Notice of an Adventurer to the Interior of Africa.

It was some time since mentioned, that a German, of the name of Roentgen, had been making preparations to prosecute the same objects of discovery that excited the ardour of the celebrated, though unfortunate, Park; and, penetrating into the central regions of Africa, to reach, if possible, the city of Tombuctoo, which has never yet been explored by any European traveller. The following article on this subject has appeared in a German journal of the 8th of October, quoted in the General Chronicle.

"There has been lately published, at Nenwied, an interesting letter from the traveller Roentgen to his brother. It reached him through Professor Hagen, who received it from Mr. Nunnemann, of London. Roentgen, it appears, after visiting Paris, Vienna, and London, had repaired to Mogadore, where he resided a considerable time; and the letter in question, dated the 21st of July, 1811, was written on the bank of the river Teniliff, at the moment of his departure for the interior of Africa." The following is some of the most interesting information it contains:

"During my residence at Mogadore, I was engaged day and night in studying the Arabic; and I have succeeded in making myself to be understood by the natives of the country. I will avail myself of that knowledge of the country, and of the manners of the people, which I have acquired, in order to travel to Tombuctoo. I would not act with so much boldness, were I not convinced, that providence has destined me to make the discovery of the interior of Africa. My good stars have furnished me with a companion in my travels, than whom I could not have wished for a better. He is a German, who, when only twelve years old, quitted his paternal roof, having an irresistible inclination for roaming; he has never since lived six months on the same spot, and is now thirty-eight years of age. He knows all the European languages, the Slavonic excepted. Fourteen years ago, when destitute of money or protection, he was impressed by the English for a sailor, in an island of the Mediterranean,
Mediterranean, where he happened to be; he was inhumanly treated by them, and reduced almost to despair. His ship Africa anchored before Tetuany for the purpose of watering; and there, having struck an English officer who had used him ill, in order to avoid punishment, he escaped, and became a Mussulman at Tetuan. Since then, he has traversed the Barbary states in all directions, and has lately returned from a pilgrimage to Mecca. He has lived at Jamba, in Africa, as a coffee-house keeper, and at Janol, as a physician. At Constantinople, he has superintended the gardens of a Pacha. I got acquainted with him at a merchant's in Mogadore, who had hired him as a gardener. I have taken him into my service, and I treat him rather as a friend than as a domestic; the benefits which I shall derive from his experience are immense.

About a month ago, I travelled with a caravan of merchants to Morocco, where I procured valuable information respecting the communications with the interior of Africa.

It is impossible to convey an idea of the violent hatred which animates the Moors against Christians. Even at Mogadore, I could hardly go abroad without being overwhelmed with insults. I was obliged, in order to view the city of Morocco, to get an escort of four soldiers, who, by orders of the government, were to keep back the populace. Even then I was often assailed by stones, one of which hit me so severely a blow on the forehead, that for some time I thought myself dangerously wounded. This hatred of the Moors arises in a great degree from our dress.

I saw, at Morocco, preparations for the setting out of a caravan, which was to reach Tombuctoo by Tafilet and Tunt. I immediately formed a resolution to join this caravan, and I returned to Mogadore. My companion was delighted with the plan, which I did not communicate to any one else, but to one Christian. I caused it to be reported at Mogadore, that, disgusted with the bad treatment I had received at Morocco, I meant to repair to Tangier, and from thence embark for Gibraltar. This pretended project furnished us with a pretext for purchasing a mule, and every other necessary for my journey. I secretly procured some Moorish garments. Having finished my preparations, I invited some Christians at Mogadore to a party of pleasure on a mountain, about six English miles off, whither they
they were often in the habit of going. I have there spent one
day with them, and declared that I meant to proceed for Tan-
gier. They will accompany me to a certain distance, and give
out at Mogadore that I am on my way to Tangier. As soon as
I am left alone with my fellow-traveller, I mean to clothe
myself in my Moorish garb, and to enter the great road which
leads from Tafilet to Morocco. From thence I shall reach
Deminit, a town situated at the foot of Mount Atlas, where
I shall be safe from any searches which the governor of Moga-
dore might make; should he learn that I have not gone to
Tangier. At Deminit, I shall join a caravan, which will pass
there about that time, and with it I shall cross Mount Atlas,
covered with snow, and next enter the burning plains of Tafilet.
I shall remain at Tafilet with a German renegado. There are
in that city a number of Germans. There are some Germans
in Morocco, and to one of them I am indebted for some valu-
able information. I expect to find a German in Tombuctoo,
and there I mean to remain six months, making it the centre of
my observations on the interior of Africa. I shall pass for a
physician: I have laid in a supply of medicines, of which I
know the application. It is my wish to penetrate towards the
south, and to be able to reach Wesemb, or the Cape. Should
I find this too difficult, I mean to return to Europe to publish
the journal of my travels; and shall again return to Africa,
where I am destined to make some discoveries.

X.

Description of a remontoire Escapement for Pendulum Clocks,
invented by Mr. George Prior, Jun*.

The swing wheel, A, figs. 1 and 3, Plate III, has thirty
teeth cut in its periphery, and is constantly urged forwards
by the maintaining power, which, in the model represented in
the engraving, is supplied by a small weight, X, figs. 2 and 0;
CD are two spring detents, catching the teeth of the wheel

* Soc. Arts., XXIX. anno. 1811. The society bestowed a premium
of 20 guineas for this invention.
alternately; these are, at the proper intervals, unlocked by the New escapement, parts marked 2 and 3, fig. 1, upon the pendulum rod H, excepting small pins, a l, fig. 2, projecting from the detents, as it vibrates towards the one or the other; E is the renovating or remontoire spring, fixed to the same stud F as the detents. It is wound up by the highest tooth of the wheel, as seen in fig. 1, (its position when unwound being shown by the dotted line.) This being the case, suppose a tooth of the wheel is caught by the detent D, which prevents the wheel from moving any further, and keeps the renovating spring from escaping off the point of the tooth: in this position, the pendulum is quite detached from the wheel; now, if the pendulum be caused to vibrate towards G, the part of it marked 2, comes against the pin b, fig. 2, projecting from the renovating spring E, and pushes this spring from the point of the wheel's tooth; on vibrating a little farther it removes the detent D, which detained the wheel by the part 3 striking the pin (a, fig. 2) which projects from the detent; the maintaining power of the clock causes the wheel (thus unlocked) to advance, until detained by a tooth resting upon the end of the detent C, on the opposite side; by this means, the renovating spring will be clear of the tooth of the wheel as it returns with the pendulum, and gives it an impulse, by its pin b, pressing against the part 2 of the pendulum, until the spring comes to the position shown by the dotted line; in which position it is unwound, and rests against a pin fixed in the cross-bar of the plate; the pendulum continues vibrating towards I, nearly to the extent of its vibration, when the part 1 meets the pin in the detent C, and removes it from the wheel and unlocks it; the maintaining power now carries it forward, pushing the renovating spring E before it, until another tooth is caught by the detent D, which detains the wheel in the position first described, the renovating spring being wound up, ready to give another impulse to the pendulum.

N. B. The pin b, fig. 2, is not fixed to the renovating spring itself, but is part of a piece of brass, which is screwed fast to the renovating spring, and is made very slender near the screw which fastens it; this permits the end of the renovating spring to give way, if, by the weight being taken off the clock, or any
any other accident, the escape-wheel should be moved backwards, so as to catch on the detents improperly.

The following observations are necessary to be attended to in this escapement.

1st. That the renovating and detent springs must spring from one centre, and as similarly as possible.

2d. That the force applied to the train must be so much more than what will wind up the renovating spring, as will overcome the influence of oil and friction on the pivots of the machine.

3d. That the renovating spring, when unwound, must rest against the point of the tooth of the wheel; which will be an advantage, as it thereby takes as much force off the tooth of the wheel resting against the detent spring, as is equal to the pressure of the renovating spring C, against the face of the tooth of the wheel.

4th. The detent springs must be made as slender and light as possible; though whatever force they take from the pendulum, by their elasticity in removing them, to unlock the wheel, so much force they return to the pendulum in following it, to where it removed them from; therefore action and re-action will be equal in contrary directions.

5th. That it is unnecessary for the pendulum to remove the detent or renovating springs, much farther than is necessary to free the teeth of the wheel, as it will always vibrate up to the same arc; in table clocks it ought to remove them further, so that it can go when not placed exactly level, or what is generally termed, out of the beat.

XI.

Description of a simple, cheap, and easy Method of preventing the Annoyance of steam from Boilers in Manufactories and other Places. By Mr. George Webster, of Leeds.

The introduction of steam into workshops and manufactories, is injurious to the articles, to the buildings, and to the workmen; and, when the matter evaporated from boilers

* For which the Soc. of Arts gave their silver medal in 1811.
is of an offensive nature, it must be still more desirable to dissipate, or carry it off, in the most efficacious and simple manner. Mr. Webster, after various trials, has accomplished this by an ascending trunk or pipe, which communicates with the chimney, and is explained in the following description, by reference to fig. 4. Plate III.

A A, the brick work surrounding the pan.

B, the steam chimney, made of wood, about two feet broad and six inches deep. A small opening at the back part of the pan admits the steam into this chimney; it may from thence be carried up to the top of the building, or turned into any smoke chimney near at hand.

In order to keep the water in the pan as hot as possible during the night, there are two dampers in the steam chimney at D, and if both these dampers are shut, and the whole top of the pan covered closely over at C, the boiling water, even when the fire is withdrawn, will keep hot for the workmen till the next morning.

C C, are loose boards, fitting close to each other, and covering completely the better half of the circle of the top of the pan; and upon this circumstance depends the whole secret of getting quit of the steam. If you remove these boards or partial coverings, the steam chimney loses all its use. The letter b shews the part of the top of the pan which should be left open to admit to the workmen a ready communication with the hot water; and through this open part a current of cold air is constantly seen to press and force the steam rapidly up the steam chimney.

It is proper to add, that there must always be an empty space of two or three inches between the surface of the hot water and the under part of the cover cc, so as to permit the steam to pass to the bottom of the steam chimney. To effect this purpose, and at the same time to allow the copper to be full of hot water, a rim or curb of wood F, about three inches thick, should be fixed on the top of the copper, and upon this the covering boards cc placed. This allows sufficient room for the steam to press forward to the steam chimney at all times.

The cover and wood steam chimney are removeable, and may serve for another copper, if both be not wanted at the same time.
### XII.

**METEOROLOGICAL JOURNAL.**

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<td>29.790</td>
<td>38</td>
</tr>
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<td>Var.</td>
<td>30.02</td>
<td>29.82</td>
<td>29.920</td>
<td>42</td>
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<td></td>
<td>N</td>
<td>30.30</td>
<td>30.02</td>
<td>30.160</td>
<td>36</td>
</tr>
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<td></td>
<td>N</td>
<td>30.40</td>
<td>30.30</td>
<td>30.380</td>
<td>35</td>
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<tr>
<td></td>
<td></td>
<td>30.51</td>
<td>28.98</td>
<td>29.882</td>
<td>52</td>
</tr>
</tbody>
</table>

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A.M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.
REMARKS.

Eleventh Month, 28. The sky, about sunset, was overspread with Cirrus and Cirrostratus clouds, beautifully tinged with flame colour, red and violet. 30. a.m. The sky again much coloured.

Twelfth Month. 5. The weather, which has been hitherto mostly cloudy, with redness at sunrise and sunset, begins now to be more serene. 6. Hoar frost. 7. A little appearance of hail balls on the ground. 8, 9. Clear, hoar frost. 11. Snow this morning, and again after sunset. 13. An orange-coloured band on the horizon this evening; this phenomenon arises from reflection by the descending dew. 15. A gale from N. E. unaccompanied by snow, came in early this morning. 16. a.m. The wind has subsided to a breeze, and there now falls (at the temp. of 27°5) snow, very regularly crystallized in stars. 17. a.m. It snowed more freely in the night, and there is now a cold thaw, with light misty showers. 18. A little sleet, followed by snow. Ice has been formed in the night, by virtue of the low temperature which the ground still possesses. A wet evening. 21. A little rain, a.m. 22. A dripping mist. 24. Cloudy; a little rain; some hail balls in the night.

RESULTS.

Prevailing winds easterly.

Barometer: greatest observed elevation, 30°51 in.; least 28°98 in.;
Mean of the period 29°882 inches.

Thermometer: greatest elevation 52°; least 13°.
Mean of the period, 36°66°.

Rain and snow 0°95 inches. The Evaporation during this period has not been ascertained.

Plainsw, 1st Month, 7, 1813.

L. Howard.
We may consider the affinity of chemistry as a tendency by which bodies are incessantly urged. By this affinity they are disposed to combine, in such proportions and in such numbers of each body, that they afterwards cease to manifest any farther affinity of combination, which property may from that period be considered as in a state of repose or inactivity. Such a combination or compound, which no longer shews any affinity towards the greater part of other bodies, may be itself called an indifferent body. If, for example sulphur, carbon, and oxygen, come into contact, they tend to combine in such a manner, as to produce sulphate of barytes; and in this compound the affinities of the ingredients appears to be in a state of repose; that is to say, they constitute an indifferent compound.

The entire tendency or activity of the affinity is, therefore, excited to arrive, by an effect, which occupies a longer or shorter time, according to circumstances, at this state of repose or indifference. If the elementary bodies were collected at the same place, and all possessed an equally strong chemical affinity, an active chemical phenomenon would ensue, which would terminate in eternal-repose. No force would tend to change this state of repose, and such different combinations as might be thus formed, being attracted by each other, by the means of gravitation and cohesion, would constitute a mass or aggregate of indifferent bodies.

But this is not the construction which takes place in nature, among the surrounding bodies of that small part of the universe which is submitted to our observation. A series of mutations...
take place in unorganized matters, by which organized nature is supported, and we have plausible reasons to conjecture, that a similar disposition prevails in the other parts of the immensity of the universe.

The circumstances which incessantly tend to destroy or to prevent the repose of combined elements, are light, caloric, and electricity, assisted by the circumstance that the chemical electricity, affinity of the different elementary bodies is not equally strong.

Caloric, light, and electricity, have a mutual relation with each other; easy to be perceived, but very difficult to be comprehended. Very often the presence of one of these produces the other, without one being capable of determining whence it comes. When a large and powerful electrical pile is discharged by means of two points of platina, a sun is produced at the point of discharge; indeed upon a scale of infinite minuteness as to magnitude, but which, by the intensity of its light and heat, surpasses every other phenomenon of fire produced upon one globe; which fuses the metal, and loses nothing by the comparison, even when produced in the midst of a flame, supported by oxygen gas. The production of light and heat at the point of the electric discharge; that is to say, at the point where the two separated electricitys it cease to manifest themselves as electricity, cannot be mistaken; and proves, that there is a relation between these substances, which we may, perhaps, hereafter be better able to comprehend than at present.

Caloric and the electricitys exhibit, in our experiments, a kind of tendency to acquire an equilibrium; that is to say, to arrive at the same state of repose, which appears to be the ultimate end of the chemical affinity of ponderable matter. But this equilibrium of caloric and the electricitys is incessantly broken by the rays of the sun, by which the surfaces of the planetary bodies is alternately enlightened at determinate intervals.

There is therefore a process carried on in the sun, by which the repose of the united elements is incessantly intercepted or prevented, and which preserves them in a certain state of activity. It is impossible for us to determine the nature of this process; because the truth of our conjectures will never, in all probability, be proved in a satisfactory manner; but, notwithstanding the difficulty, it will always be a subject of interest to
to ascertain which of our conjectures may be the least improbable.

We know that the phenomenon of fire is produced on one globe on two principal or leading occasions. 1. When two bodies combine; for example, in oxidation, sulphuration, the combination of acids with bases, &c.; and 2. When the separated electricities mutually penetrate each other, and cease to appear as electricities.

(There are, nevertheless, two other manners by which fire may be produced; namely, friction and compression. As to friction, there is reason to believe that it will be found to class itself along with the electric discharge; and compression, on the other hand, does nothing more than drive the caloric out of a body which it contains already produced. But in the present part of our discussion, we attend only to the cases in which caloric appears to be produced, that is to say, in which we cannot conceive whence it comes).

It is incompatible with every scientific notion we possess, that the phenomenon of an interior fire should be produced in the sun, by a chemical combination, or by a condensation of ponderable substances. Such an opinion has been rejected by our ancestors, though their notions of combustion were less precise than ours; and it appears to be contradicted by the circumstance, that the magnitude of the mass of the sun remains constantly without alteration, at least, as far as our observations can determine. It remains therefore as the least improbable of our conjecture, that a process is affected in the sun, analogous to that which obtains between the points by which an electric pile is discharged; and we must imagine that this process, when once commenced, must, from the nature of the actual arrangement of things, continue for ever; and that, consequently, the activity of created matters is maintained, as it were, by a gyration in a circle, or by always returning again to their first situation or state, as in astronomy we know to be the case with their motions in space. It is beyond the limits of human reason to determine how these processes at first began, and it would no doubt be unworthy of an enlightened and discerning mind to presume seriously to form any conjecture upon the subject.

The electric... Our experiments with the electric pile, have proved how much
much the Electricities are concerned in the operations of chemical affinity; and that sometimes they are suppressed, and at other times made to act in an opposite sense. It was even observed, before the discovery of the Electric pile, that the Equilibrium of Electricity is sometimes disturbed by chemical operations, and the knowledge acquired from the labours of the last ten or twelve years, has shown us, that there is not a single action of affinity, in which the electricities do not cooperate.

We do not know how this co-operation is made, and, for the moment, we must be satisfied with conjectures upon it. What we with certainty know is, that two bodies which have affinity for each other, and which have been brought into mutual contact, are found upon separation to be in opposite states of electricity. That which has the greatest affinity for oxygen usually becomes positively electrified, and the other negatively. Bodies which have little affinity between them, or, which have nearly an equal affinity for oxygen, do not sensibly derange the electric equilibrium by their mutual contact. This is not only the case with combustible bodies, but it also takes place with the oxides; as for example, the oxalic acid, dry and deprived of its water of crystallization, brought into contact with quick lime, becomes, according to the experiments of Davy, negatively electric, while the lime becomes positive. And since the electric state of these bodies is more marked, the higher the temperature, that is to say, as the chemical affinity becomes more active; and lastly, as at the moment of their union there is a production of heat, which may vary from a very slight elevation of temperature to that of the most intense fire, we think we may conclude, that at the moment of the chemical combination, there is a discharge of the opposite electric state of the bodies, which here, as in the pile, produces the phenomenon of fire, at the instant when the electricities disappear.

A derangement of the equilibrium of electricity appears therefore to precede, and as it were predispose, the action of the chemical affinity; though this phenomenon from physical reasons cannot be always discovered by our instruments; as may happen, for instance, when one of the bodies is in the liquid state. Davy found, in conformity with this, that sulphur, heated upon

upon copper, gave signs of very strong electricity, constantly increasing with the temperature till the sulphur melted, at which instant the signs disappeared.

After the union of ponderable bodies, in which the electricity is seen to fly off in the form of light and heat, the ponderable bodies are reduced to a state of chemical repose. The elements of the combination can no more be separated, nor be restored, to their original form and characters, without the influence of a mass of electricity, in a state of charge or of separation, as in the operation of the pile. But in this case the electricity, tending to regain their equilibrium, decompose the combination, by operating each upon its relative constituent part to which it restores its original form and characters.

(To be Continued.)

XIV.

Facts and Remarks, upon the Interruption which the situation of the maintaining weight produces in the rate of a clock, when near the pendulum. By H. K.

To Mr. Nicholson.

SIR,

In your Journal for October last, I observed a paper by Mr. Thomas Reid, on the effect produced on the going of clocks, by the attraction between the weights and the pendulum.

The effect alluded to, viz. that of the arc of vibration becoming less when the weight is near the ball of the pendulum was remarked some years since, by the late Dr. Hornsby. This gentleman having done me the honour to accompany me in a visit to the observatory at Oxford, pointed out an astronomical clock there, the weight of which he had contrived to pass behind the clock case. He informed me, that he had remarked an irregularity in the going of the clock, when the weight approached the ball of the pendulum, and attributed it to the increased
increased resistance of the air, from its free motion being
impeded by the weight of the clock.

Indeed, it does not seem that attraction could produce the
reason why effect alluded to; for, though the ball of the pendulum might
be retarded in its ascent, its motion would be proportionably
accelerated on its return.

I have been induced to trouble you with this, merely from
respect to Dr. Hornsby's memory, and not with the slightest
intention of depreciating the talents of Mr. Reid.

I am Sir,

Your obedient humble Servant,

H. K.

Ipswich, Dec. 6, 1812.

REMARK.

From the nature and tenor of Mr. Reid's communication I
concluded, that his single weight descended either in front or
behind the ball, and not on one side of it; and in this arrange-
ment its attraction would add to that of gravity, whether per-
ceptibly or not. I likewise requested his brother, who brought
the paper, to suggest that it might be desirable to make trial of
a temporary piece or mass, to be put on or taken off at pleasure,
in the place where the weight had been inferred to produce the
greatest acceleration; and to keep the weight out of the limit
of disturbance; this would remove all suspicion of irregularity
in the train: And I would, from the ingenious observations of
my Correspondent, suggest farther, that the temporary piece
should be a thin shell of brass, with a solid core of lead; which,
when taken out, would greatly diminish the attraction, but not
the impediment from increased resistance of the surrounding
air.
AN Account of a Journey, undertaken in 1807, by M. Valen-
berg, has been published lately, at Stockholm, under the
auspices of the Academy of Sciences of Sweden, for the pur-
pose of determining the height of the mountains of Lapland, and
observing their temperature. The mountains visited by M.
Valenberg, make part of the great chain which runs through
Sweden and Norway, and stretches in some of its branches
even to Finland and Russia. They are situated between 67
and 68 degrees north latitude, and belong to the polar regions.
On several points their bases are washed by the sea, and from
their summits the immense plain of the Northern Ocean is
discoverable. These mountains had been only hitherto viewed
in all their majestic grandeur by the Lapland nomade, following
his flocks of deer and his game. A few travellers had contem-
plated them at a distance; and M. de Bruck, a learned
German, during his travels in Norway, approached within a
short space of them; but no person had ever yet penetrated
into this asylum of nature, and attempted to struggle with the
difficulties of ascending these summits, eternally covered with
snow and ice.

The undertaking was difficult in many respects. The ascents
were mostly excessively steep, and in climbing them the trav-
ereller was by turns sus-pended over deep fissures, lakes, tor-
rents, bottomless marshes, and gulfs. He had no intelligent
guide, there was no habitation on his route, and no assistance
to be expected. He frequently was obliged to make circuits of
many leagues to reach a summit; and he crossed not only
snow and ice full of crevices, but also marshes, where he run
a continual risk of being buried in the mud and stagnant water.
He passed the nights on naked rocks, without a tent or the
smallest shelter; and he was frequently reduced to quench his
devouring thirst by swallowing snow, which occasioned him
inflammations and painful suppurations in the mouth.

M. de Valenberg's measurements give the Lapland moun-
tains an elevation of from 5 to 6,000 feet above the level of
the sea. Although this elevation is less than that of the mountains of Switzerland and the Pyrenees, all the phenomena of the Alpine regions, and particularly glaciers, are observable. At such a proximity to the polar circle, the region of eternal snow commences at nearly 4,000 feet above the ocean, while in the Alps it begins at from 7 to 8,000, and in the Pyrenees at 8,000 feet.

On the 14th July, M. de Valenberg ascended the most considerable glacier, called Sulitelma, a Lapland word, which signifies Solemn Mountain, because formerly the Laplanders adored on one of its summits their principal idol. This mountain, which is the Mount Blanc of the north, is composed of a succession of summits, of which the base has an extent of several leagues. Its greatest elevation is 5,700 feet above the sea. To reach this elevation, our traveller was obliged to make his way over enormous crevices, where recently before some hunters had been engulfed with their deer and their dogs. Seas of ice have descended into the vallies 700 feet below the line of snow. There is a border of earth surrounds the ice, consisting of slime and stones. The ice of Sulitelma is very clear, and almost transparent; it is as hard as stone, but not so heavy as the ice of the sea. The traveller gives several details respecting its internal composition, the figures by which it is characterized, and the crevices formed on it. The snow is sometimes 100 feet in depth, and so hard that the footsteps leave no mark on it. That which is detached from the summits, or crevices, roll to immense distances. Fortunately, these avalanches in their descent act only on inanimate nature: whatever direction they take they seldom encounter living beings, or the abodes of men. All is desert in these regions for vast extents, where industry has gained no conquest over the solitary domain of the primitive creation.

The traveller terminates his account by general considerations on the temperature, and by tables of meteorological observations. He determines with precision the different regions of the mountains, and characterizes them by the productions which he found there. In proportion as the line of snow is approached, the productive force of nature diminishes, and men, brute animals, and plants, yield to the rigour of the cold. At 2,600 feet below the line, the pines disappear, as well
well as the cattle and habitations. At 2,000 feet the only tree is the birch; and its degraded form and indigent verdure attest the inclemency of the climate; at the same time the greatest number of wild animals disappear, and the lakes contain no fish. At 800 feet below the same line of snow, the Laplander's progress is stopped for want of moss for his rein-deer. Above the line everything presents the picture of agony and death. The most robust lichens are only to be found at 1,000 and 2,000 feet, in the crevices of perpendicular rocks; and the bird named Emberiza nivalis, or snow-bird, is the only living creature to be seen. The heat does not rise to one degree of Réaumur, in the region, which is 5,000 feet above the sea.

Mr. Fiddler, a captain in the Hudson's-bay service, has communicated to Mr. Arrowsmith, the draught of the district of country which lies between the rocky mountains and the great ocean, and between the latitude 52 and 46. It contains all the head waters of the Colúmbian River; of a lake, called, by Mr. Fiddler, Lean's Lake; a river running into it, called Arrowsmith's River; and a river of magnitude, called Wedderburn's River. The whole tract is inhabited by tribes of flat-head Indians, otherwise called Têtes de Boules, and one large extent is filled with wild horses. Mr. Arrowsmith purposes to introduce these discoveries into his General Map of North-American Discoveries.

Mr. Arrowsmith has completed a new Map of Germany, in six sheets of double elephant, being the largest map of that empire ever drawn and published in England. Like all the maps of this eminent geographer, this new one is derived either from original or unquestionable and superior sources.

The same geographer has for some years been engaged on a Map of England and Wales, in 18 sheets, which, when put together, will be 10 feet by 12. Of this extraordinary map it deserves to be noticed, that it will contain at least 1,000,000 names, which is the more remarkable because the places enumerated in the Population Return are only 15,741; and Capper's Topographical Dictionary does not contain above 20,000 places for the three kingdoms, although double the number contained in Luckombe's Gazetteer.
It is with regret that I find myself under the necessity of taking notice of some passages in the preface to Dr. Thompson's Annals of Philosophy, in which he animadverts upon the English Philosophical Journals.

1. Of my Journal he says, "that for several years it was "excellent," and adds, "that for some years past, if report "says true, it has not been the property of the original editor, "but of a bookseller; and, in reality, edited, not by Mr. "Nicholson, but by some unknown person employed by the "bookseller.

2. Of the Philosophical Magazine, which he calls a rival publication, he says "it is edited by Mr. Tullock, a printer "from Glasgow, and publisher of the evening newspaper, "called the Star," and that "it, perhaps, never contained so "much original matter as my Journal.

3. Of the Repertory of Arts, he says it consists chiefly of specifications of patent inventions, with a few additional papers copied from the Transactions, or other Journals; but he overlooks the remarks and discussions from the inventors and others, which are inserted in that work.

4. And of the Retrospect of Discoveries; or Abridgment of periodical and other Publications, he says, that it is, as the title implies, merely an abridgment of the other three Journals, of the British Transactions, and of one or two French periodical works. But, in so doing, he denies the existence of those numerous, clear, and able criticisms upon the subjects so abridged, which constitute part of the plan of the Retrospect, and are every where to be met with.

5. His deduction then follows: "Such," says he, "being "the state of the English Philosophical Journals, our readers "will not be surprized that we (Dr. T.) venture to offer our "claims to the attention of the public."

Whether it became Doctor Thompson to have assumed the office of Censor, with regard to the productions which he appears to consider as rivals; or whether it would have been more decorous for him, as a man offering himself in the venerable presence of the public, to have felt the consciousness of human infirmity, and expecting to have his own faults viewed with candour, to have avoided the volunteer task of exposing those of others. Into these points I do not enquire; and, if these had been the only objects of question, I should have been silent.
But I can enquire and can decide, that it did not become Dr. Thompson to endeavour to depress his rivals by stating or giving currency to untruths. This is a point of moral character which I will treat in no other way than by shewing he has done so. My statements are numbered correspondently with the paragraphs at the beginning of this Notice.

1. The Philosophical Journal was published for the first year, 1787, for the joint account of myself and Messrs. Robinsons. In 1788 the entire Copy-right became mine and has continued so without interruption ever since. No bookseller ever had power to employ, or did employ any person in editing or interfering with the copy of the Journal. That copy has been provided by myself and Correspondents; and I have always had one assistant, fully acquainted with the Sciences, and Languages, of my own knowledge and appointment, and not employed by a Bookseller. My name as Author and Proprietor has every month been before the Public; was it not the duty of a good man, instead of sheltering himself under the words, "if Report says true," to have enquired whether the Report was or was not true, before he ventured to join in propagating it?

2. I am informed that Mr. Tilloch (not Tulloch) was not a printer at Glasgow, and is proprietor (not publisher) of the Star. These are unimportant sounds in themselves; but they shew the disposition of Thompson to lower his supposed opponents, and his want of accuracy and correctness. All the world knows how many eminent men have been printers, and how little in a nation like ours, the science and acquisitions of men, depend upon their pursuits in business.

3. His prejudiced notice of the Repertory speaks for itself.

4. And so does his positive assertion that the Repertory is merely an abridgment. He could not but have seen, though he has thought fit to deny, the excellent original discussions it contains.

5. His summary deduction points out the spirit and motives of his statements; namely, to shew that the English Journals are bad, and, by inference, that his own will be much superior.

My principal object has been to expose the Doctor's conduct with regard to myself. The world must determine for him, whether that conduct can promote any interest for which a well-disposed mind ought to be solicitous.