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ARTICLE I.

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

(Continued from p. 146.)

In the explanation of these phenomena, there is a circumstance which confounds us more than all the others, viz., that there is only one body, that is to say oxigen, which possesses absolute and invariable electro-chemical characters. All the others, while they manifest a fixed and determinate relation with regard to oxigen, vary with regard to each other. Sulphur, for example, is positive with regard to oxigen, but is negative with regard to the metals; arsenic is positive with regard to oxigen and to sulphur, but it is negative with regard to the other metals; silver is positive with regard to oxigen, sulphur, and arsenic, but is negative with regard to most of the metals.

[The author here subjoins the following annotation, which, on account of its extent, I have printed in the body of the page.—W. N.]

Though it may, perhaps, be too early for us to adopt any Theory of notions respecting this difficult subject, I shall here offer a conjecture.
jecture upon the manner according to which the whole of the
effects may take place, without contradicting any of the results
we possess concerning electricity.

Admitting, that bodies consist of particles or atoms placed
near each other, in such manner as may appear from their pro-
erty of combining in proportions of their multiples, we may
consider these atoms as possessing an electrical polarity upon
the intensity of which the force of their affinity depends. In
this case the chemical affinity becomes identified with elec-
tricity, or rather the electric polarity. In order to explain the
different electro-chemical characters, we must add to the gen-
eral polarity a kind of specific unipolarity, by means of which
one of the poles contains more of the $+E$, or of the $-E$,
than the opposite electricity in the other pole is capable of sa-
turning. A body of which the positive pole predominates,
that is, which contains an excess of positive electricity, con-
stitutes an electro-positive body, and vice versa. Many bodies
require an elevation of temperature to enable them to act upon
each other. It appears, therefore, that heat possesses the pro-
erty of augmenting the polarity of these bodies; and that
the difference in activity of the affinity at different temperatures,
appears to depend on the same cause, in like manner as the
force with which a combination preserves its existence, appears
to depend on the intensity of the electric polarity when this is
at its maximum, or rather the intensity of that polarity at the
moment the combination is made. This circumstance explains
why the phosphoric acid is decomposable by charcoal at an
elevated temperature, although phosphorus decomposes the
air of the atmosphere at a temperature at which charcoal has
no influence upon that fluid.

In the theory of atoms, there is some difficulty in conceiving
the difference between the juxta-portion of homogeneous par-
ticles, separable by mechanical means, and that of the heteroge-
neous particles, which produce a new particle, very seldom de-
composable by means purely mechanical. The hypothesis of
polarized atoms assists us upon this occasion. The cohesion of
homogeneous particles may be compared to the juxta-position
which we observe in the electrophore between the opposite
electricities of the metallic plate and the resinous surface
Contact keeps them in a state of charge or neutralization;
which,
whicn, in fact, is simply juxta-position, and is destroyed when the surfaces are separated, and each appears again in possession of its original electric state. When heterogeneous atoms combine (whether the combination do consist simply in juxta-position, or, which is more difficult to comprehend, in a partial or total penetration) they appear to adjust or dispose themselves so as to touch with the opposite poles; of which the electricities produce a discharge which causes the phenomenon of elevation of temperature, almost constantly apparent at the time of any chemical combination, and the particles remain combined until their discharged poles are, by some means or other, restored to their former electric state.

As we know, from fact and experience, that bodies of the same electro-chemical class (that is to say, bodies in which we conceive that the same pole predominates) can combine, it appears, that the force of affinity depends rather on the intensity of the general polarity, than of the specific unipolarity; and from this reason it may be, that sulphur has more affinity with oxygen, than gold or platinum has, although sulphur has the same unipolarity as oxygen, and those metals have an opposite unipolarity to that of oxygen.

It is clear, that when two bodies, in which the same pole predominates, combine together, the new particle must possess their unipolar force concentrated in one of its poles, and must, consequently, have electro-chemical properties more intense; and this is a good reason why sulphur and oxygen produce the strongest acid. On the contrary, when particles possessing an opposite polarity unite, the polarity of one of the particles most frequently predominates; for example, in potash, and in most of the metallic oxides, the predominating pole of the metal also predominates in the compound. In some instances, the product is a neutral compound, in which neither of the poles predominate, such as the superoxides in other instances, the pole of the metal predominates in one degree of oxidation, and that of the oxygen in another.

The combination of polarized atoms requires a motion to turn the opposite poles to each other; and to this circumstance is owing the facility with which combination takes place when one of the two bodies is in the liquid state, or where both are in that state; and the extreme difficulty, or nearly impossibility, of

\[ M^2 \]
classification.

In my essay on chemical nomenclature, I have divided bodies into electro-positive and electro-negative, the first of which maintain the combination, and their transmission the electricity, the latter transmit the electricity from particle to particle, and the union of the opposite states of bodies is therefore more obscure. The poles of the voltaic pile appear to present the principles of a compound, points of attraction more powerful than the union, but in the present state of their unipolarity, it is not by their union, but in combinations of opposite unipolarity, that we can restore the specific unipolarity of the elements. We may conclude, therefore, that the general re-polarization takes place in the same manner as the lodestone gives magnetin to a small particle of steel, and that in the second, the pile contributes, by its own specific energies, to restore the predominating poles.

Here the annotation concludes. — W. N. J.

Pile merely restores, by the decomposition, the general polarity, but contains the specific unipolarity, it is not by their union, but in combinations of opposite unipolarity, that we can restore the specific unipolarity. The chemical action, being the combination of the pile.

The union ceases to be possible under a certain degree of dilution of the gases, as we know by the experiments of Broth, when mixed to a certain degree, cannot be set on fire at any temperature whatever. The chemical action affected by the discharge of the pile, and the union ceases to be possible under a certain degree of dilution of the gases, as we know by the experiments of Broth, when mixed to a certain degree, cannot be set on fire at any temperature whatever.
reflections upon this object, obliges me, at present, to change these denominations for each other. I shall, therefore, hereafter call those bodies electro-positive which are collected round the negative pole, and those electro-negative which are collected round the positive pole. With regard to the electro-chemical relations of bodies mutually, I shall divide them into five different classes.

1. Absolutely electro-negative; oxygen alone.

2. Electro-negative in general; all combustible bodies which produce acids with oxygen, are constantly collected at the positive pole of the pile. To this class the metalloids belong, and among the metals arsenic, molybdenum, and wolfram.

3. Bodies of a variable electro-chemical nature. This class includes (a) such bodies as, when combined with oxygen, are electro-positive with regard to the preceding class, but electro-negative with regard to the bodies which constitute the last of the subsequent classes; (b) such bodies as, in one degree of oxidation, constitute a saline base, and in another degree an acid. Tellurium is an example of the first, and antimony of the latter.

4. Indifferent. Oxidized bodies, which possess no decided character, being neither acid nor saline bases. Such are the oxides of tantalum and of silicon. This class likewise includes the combinations of acids with saline bases, that is to say, the salts.

5. Electro-positive. Combustible bodies and their oxides, which, during the action of the pile, are never collected round the positive pole, and of which a great part, when combined with oxygen in excess, instead of forming acids, produce superoxides. Such are potassium, barium, lead, silver, &c.

It is proved by experiment, that the more opposite the electro-chemical nature of two bodies is, the stronger in general is their mutual affinity. A combustible body consequently tends with greater force, to combine with oxygen, than with any other combustible body with which it may have affinity. Hence we may conclude, that, if it were possible to obtain pure oxygen in the solid form, and if, in that state, it were put into contact with a combustible body, it would become much more strongly electric than, for instance, sulphur with copper, and would, in fact, produce, in combining with the combustible body,
body, an elevation of temperature much higher than could be produced by the combination of any other body with the same combustible. These reflections appear to indicate that, in the phenomenon of combustion, as in general in every chemical combination, the phenomenon of fire is produced by a cause analogous to that which is manifested on the occasion of the discharge of the electric pile; that is to say, by a discharge between the opposite electricities of the oxygen and of the combustible body, which is made at the moment of combination.

The same considerations also explain why the phenomenon of fire is more intense accordingly as the affinity of the bodies which combine is more powerful (varying from the slightest elevation of temperature to the most intense fire) without any remarkable relation between the expansion or condensation the bodies may have undergone from their union.

Hence the effects of sulphuration are similar to those of combustion:

This electro-chemical view explains what was so difficult to be comprehended in the time of our predecessors, namely, how sulphuration could produce a phenomenon of fire exactly similar to that produced by combustion; and it classes together all the disengagements of caloric or fire, occasioned by chemical combinations. As it explains, in a consistent manner, that which the old theory could not account for, it appears to deserve our confidence, or at least our attention. I shall explain my notions by an example.

And charcoal between the poles of the pile in hydrogen or azote gas, we see the charcoal become ignited, and produce the same phenomenon, as if it were actually burning. A spectator, who, on this occasion, had no knowledge of the influence of the pile, would say that the charcoal was burning. But, nevertheless, there is, in this case, neither oxidation nor chemical combination of any ponderable matter with the charcoal, and, notwithstanding this, the phenomenon of fire is the same as if it had been produced by combustion. Now, it appears to be a well-founded conclusion, that the same effects are produced by the same causes; that is to say, that the fire in each of these cases is produced by an electric discharge.

The oxygen is not condensed by burning charcoal; and, contrary, is dissolved in the gas of which the volume under the oxidation.

Charcoal does not condense oxygen by burning, but, on the contrary, is dissolved in the gas of which the volume under
of combustion of the charcoal is the effect of a condensation, consequently the heat is not caused by condensation, nor by change of capacity.

namely, that the oxygen gas has parted with the caloric which was employed in maintaining its gaseous form; and it is clear, that the fire owes its origin to some other circumstance. Those who may not be disposed to approve the electro-chemical explanation, may, observe, that the fire in this combustion is produced by the difference between the specific heats of oxygen gas, and carbonic acid gas. But, although it cannot be denied, that such a cause (or incident) may contribute to (or accompany) the production of heat, it can be easily shewn, that it is not the principal or general cause; because the nitric acid in which the oxygen still preserves its property of producing fire with a number of combustible bodies, possesses as little specific heat as the carbonic and the sulphuric acids. In like manner, the difference of specific heat between the metallic sulphurites and that of a metallic body, is too inconsiderable to afford a plausible reason for the fire produced by sulphuration.

When a combination already formed, as, for instance, between A and B, is decomposed by the more powerful affinity of a third body C, so that this last separates A from the combination AB, and forms CB—such a decomposition is usually accompanied with an elevation of temperature, or even with fire; and this elevation is greater the more considerable the difference may be between the affinities of A and of C to B. We may form a notion, that this effect is owing to a more perfect neutralization of the electro-chemical properties of the constituent parts in the new, than in the old combination. If, on this occasion, B were oxygen, and A and C two combustible bodies, the electro-chemical nature of B must be admitted as more perfectly neutralized by C than by A; and at the instant when A is reduced to its original combustible state, it receives from C, which loses its like state, a quantity of positive electricity, equal to what it had lost when it entered into combination with B.

When bodies combine with others, in some instances more positive, and in others more negative, than themselves, are found after these two circumstances in very different states; as sulphur, for instance, is in a quite different state in the sulphuric acid, from that which it possesses in the sulphuret of lead. From the former it can be disengaged by a number of electropositive bodies, the properties of compounds are remarkably affected by the electric nature of their components, e.g. sulphur.
bodies, but from the last it cannot be disengaged by the affinity of any electropositive body to the lead; but, for that purpose the affinity of another body, more electronegative than itself, namely oxygen will be required. Sulphur has, therefore, occasion for opposite electricities, in order to effect its separation from these two different combinations. It is well deserving of attention, that when such an electronegative combustible is combined with an electropositive oxide, the combustibility of the former (or its electropositive relation to oxygen) is considerably increased; probably because its electronegative dispositions have been destroyed by the positive electricity of the oxide.

We observe this in the great oxidability of sulphur and of phosphorus, combined with the alkalis, or alkaline earths. In a combination of two combustible bodies of opposite electrochemical natures, this augmentation of combustibility does not take place, and the combination of the two is less combustible than that one of the constituents, which was the most so, because one of them has lost exactly as much of its electropositive characters, as the other (the electronegative) has lost of its characters; and as in this case the effect must result from the sum of the affinities, it follows that the affinity of the most considerable is diminished in proportion as the quantity of the other is greater, and its affinity for oxygen less. It is from this explanation that we may conceive a phenomenon of which I shall give an account; namely, that the oxide of tin mixed with the oxide of gold, becomes reduced to the metallic state without the addition of a more combustible body; simply by the action of heat, by forming a metallic alloy of gold and tin, which is not decomposable by fire, even when fused with salt-petre.

Heat often produces, without the co-operation of other circumstances, a decomposition of combinations; and as in the electrochemical theory we form the conclusion, that no body can be restored to its original properties without the influence of the same electricity which it parted with when it entered into combination, we must likewise imagine as a consistent consequence of this fact, that in the same manner as the separate electricities, by their combination disappear and produce fire, so calorific in its turn, when accumulated and tending to regain its equilibrium, is capable, in certain circumstances, of disappearing
disappearing as caloric, and re-appearing in the electric state restored to the separate elements.

If the chemical affinity be nothing more than the result of the polarity of the particles, it will follow decidedly, that it cannot be affinity which is the first mover, and causes the electric phenomena; but that on the contrary, the play of chemical affinities in the pile must be a consequence of these last: And this opinion is accordingly confirmed by experiment.*

[Annotation. See remark at the note on page 154.]

* I was long of opinion that the oxidation of the zinc in the electric pile, was the cause of the change of electricity, and I endeavoured to prove that this hypothesis was sufficient to explain the phenomena of the pile (See my Theory of the Electric Pile in the "Neues Allgemeines Journal der Chemie," by Gehlen, in the year 1807.) But the experiments of Davy and Pfaff, having rendered my opinions less probable, I endeavoured to convince myself of the truth, by an experiment which I think decisive. I took 12 tubes of glass, half an inch in diameter and three inches in height, and closed at one end, I half filled them with a strong solution of the submuriate of lime (such as is obtained by the residue after the preparation of caustic ammonia) and above this fluid I poured diluted nitric acid, with the precaution not to mix the liquids. I ranged these tubes in succession, and then took copper wires, round one of the extremities of each of which I had melted zinc, in order to attach a knob of that metal to that end. I immersed the zinc-coated ends of each into one of the tubes to the bottom of the submuriat, and then bended the upper ends of the respective wires, so as to immerse them into the middle of the acid of each nearest tube. This arrangement, consequently, formed a pile in the order following: copper, zinc, submuriate of lime, nitric acid; copper, zinc, &c. It is evident that the chemical affinity which produces oxidation at the common temperature was here at the surface of that part of the copper, which was in contact with the nitric acid; and that if this oxidation had been the primary cause of the electricity of the pile, the pole of copper in this construction ought to have possessed the same electricity (namely the positive) as the zinc pole in the common pile. Before the poles, or extremes, of this small pile were connected, the copper continued to be constantly dissolved in the acid,

By a row of tubes a pile was made in which the order was copper, zinc, submuriate of lime, nitric acid; copper, &c. While the extreme poles were unconnected, the nitric acid dissolved the copper, and the submuriate had no effect on the zinc. If oxidation were the cause of positive electricity on the copper, we would have been pos,
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acid, which it turned blue, and the surface of the zinc remained metallic and without any perceptible change. And lastly, I combined the poles by means of silver wires, passed into a tube filled with a solution of muriate of soda. But I was greatly surprised to find the effect directly contrary to what the theory which considers oxidation as the cause of the electricity of the pile had led me to expect. The solution of the copper instantly ceased and the zinc became covered with a mass of white oxide, vegetating on all sides in the form of wool. The pole of the copper produced hydrogen gas as usual, and the zinc pole caused an abundant precipitate of muriate of silver. The electric state, therefore, produced in this case an affinity, which, at the ordinary temperature of the atmosphere is inactive, and caused another very active affinity to cease, which was already in operation; and this could be effected by no other cause than that of the electricity produced by contact, which occasions the electric charge of the pile, and disposes the affinities which shall be put into activity. This little electric pile, was very powerful, and disengaged so large a quantity of gas, as would not have been exceeded by 100 pair of plates. But what could be the cause of this?—I exchanged the submuriate for neutral muriate; it then produced a very moderate effect, corresponding with the number of pairs; and lastly, I substituted neutral muriate of zinc instead of the muriate of lime, and then the effect was scarcely perceptible, though it continued sufficient to prevent the oxidation of the copper in the nitric acid, and to shew that the conduct or of the zinc pole continued always to be oxidized. It appears, therefore, that the activity of the pile depends on the liquid substance, which during the process must change place; and that the most advantageous construction of a pile is copper, zinc, alkaline substance, acid, copper, zinc, &c. The pile will continue active until the order becomes inverted, that is to say, copper, zinc, acid, alkali, &c. This experiment also proves how necessary it is in every theory of the pile to attend to the chemical effect which must take place in the liquid.

[Here the Annotation ends.]
ELECTRO-CHEMICAL PRINCIPLES.

be determined; but on the contrary, whenever that effect can manifest itself, it acts to very considerable distances, as for instance, in the precipitation of metals upon each other; and it is very probable, that actions at a distance produced by the electric powers do take place in the bowels of the earth, and contribute not only to the great revolutions of which we find the astonishing vestiges, but likewise to the tranquil formation and decomposition of minerals.

From the electrochemical view of nature we also derive a correction in our notions concerning the principles of acidity. The celebrated Lavoisier having found that sulphur, phosphorus, charcoal, arsenic, &c. produce acids when combined with oxygen, considered oxygen as the acidifying body. But notwithstanding this conclusion is supported by the circumstance that oxygen is the most electronegative of bodies, and the acids are also electronegative bodies, bodies were afterwards discovered, possessing the principal characters of acids without containing oxygen; and after the discovery that the saline bases are also oxidized bodies as well as the acids, it would be equally incorrect to attribute the acid characters to oxygen, or some of its combinations, as to suppose it to be the principle of basidity, or to call it the alkaligenous principle.

The electro-chemical explanation in which the combustible radical of an acid is already in its non-oxidized state, electronegative towards the radicals of the salifiable bases, being considered along with the experimental determination, that the radicals of the bases, and of the acids, very often combine in the same proportions in the combustible state, as in the oxidized state, proves that it is to the nature of the radicals, and not to oxygen, that we ought to attribute the nature of the product of oxidation. In this manner it is that sulphur and potassium combine for the most part in the same proportions, in the sulphuret of potassium, in the hydrosulphuret of potassium, in the sulphuret of potash, in the hydrosulphuret of potash, in the sulphite, and in the sulphate of potash; and it is by no means difficult to observe, that sulphur not oxidized, performs the part of an acid, that is to say, of an electronegative constituent in the sulphuret and the hydrosulphuret of potash. These observations upon sulphur and potassium may be applied to all the other combustible
hostile radicals which possess an opposite electric nature. And from all this it follows that it is the radical itself and not the oxygen which determines whether the oxide shall be an acid or a base.

A great question still remains to be discussed; Whether the electricities and caloric be matter or merely phenomena? This question has long been disputed, and will long continue in dispute before it shall be decided; which, perhaps, will never be done. At present we must content ourselves with reasoning, though our arguments can at best be considered as the sport of imagination upon interesting objects.

If, by the word matter, we understand a body which manifests its presence by gravitation, which possesses a certain kind of aggregation, and fills the place of its existence, in such a manner as to exclude all other bodies—it will certainly follow, that these problematic beings are not matter. But is it not possible that they should be matter, without possessing these characters; or are the reasons greater for considering them as phenomena?—A number of philosophers have considered light as the oscillations in a problematical matter produced by luminous bodies; and this hypothesis owes its origin to the analogy which exists between sound and light. But this oscillating matter has not yet been discovered by chemistry; and consequently the hypothesis itself cannot be satisfactory, because it presupposes a thing of which we cannot find the existence. But if we even admit that light, and the mechanical phenomena which are presented in its motion, can be attributed to a vibration analogous to that which constitutes sound, this mechanical motion cannot produce the chemical effects of light; such as the alterations in the form, the aggregation, or the composition of bodies; more especially as we have never discovered that sound could produce any such effects. There is, consequently, some probability that caloric may be matter, and that light and all radiations may consist in modes of propagating that matter.

It may be demanded whether we can imagine the existence of a matter possessing chemical affinities without obeying the laws of gravitation. There is certainly no contradiction in this position. We admit the difference between cohesion and gravitation,
and we are also led to distinguish the latter from chemical affinity. Caloric does not gravitate.

Some philosophers have sought to prove that caloric possesses weight, though too small to be perceived; but if caloric be even supposed to be matter, it is not probable that it should possess weight, because the property of radiating excludes all the effect of gravitation, and because this matter, if heavy, ought to accumulate without limit in the planetary bodies and at length destroy them.

From the relation which exists between caloric and the electricities, it is clear that what may be true with regard to the materiality of one of them, must also be true with regard to that of the other. These are, however, a quantity of phenomena produced by electricity, which do not admit of explanation, without admitting at the same time that electricity is matter. Electricity, for instance, very often detaches every thing which covers the surface of those bodies which conduct it. It, indeed, passes through conductors without leaving any trace of its passage; but it penetrates non-conductors which oppose its course, and makes a perforation precisely of the same description as would have been made by some thing which had need of place for its passage. We often observe this when electric jars are broken by an over-charge, or when the electric shock is passed through a number of cards, &c.

We may, therefore, at least with some probability, imagine caloric and the electricities to be matter destitute of gravitation, but possessing affinity to gravitating bodies. When they are not confined by these affinities, they tend to place themselves in equilibrium in the universe. The suns destroy at every moment this equilibrium, and they send the re-united electricities in the form of luminous rays towards the planetary bodies, upon the surface of which, the rays being arrested, manifest themselves as caloric; and this last in its turn, during the time required to replace it in equilibrium in the universe, supports the chemical activity of organic and inorganic nature. If we can imagine all this to be possible, we possess a notion how the sun can cause a body to emanate from itself without loss of its own volume, and without this emanated body producing on the bodies which arrest it the effects of a gravitating and falling matter.

But it is proper to put an end to these conjectures. I hope Apology for that
that the necessity of referring to some electro-chemical theory will excuse the attempt of having imagined one; and though this necessity cannot be pleaded in justification of extravagant conjectures, they will, perhaps, be thought pardonable in a department of science where experiment is yet wanting to regulate the efforts of imagination, although such efforts may be useful to arrange the existing facts, and indicate the course which may lead to the discovery of new ones.

(To be continued.)

II.

Notice respecting Experiments on the Freezing of Alcohol, by Mr. Hutton*.

I HAVE been prevailed upon to communicate a notice of some experiments and observations I have made on the production of a great degree of cold. It is scarcely necessary to observe, that my doing so at this time is not a matter of choice: these experiments and observations were mentioned to my friends, as they were made without any injunction as to secrecy, as I did not anticipate that such communications would either be received with so much avidity, or repeated with so much eagerness. The consequence has been, that accounts of these experiments have now got into very general circulation, and many very contrary and erroneous ideas have been entertained, not only as to their extent, but even as to their nature; and it has been imagined, that a communication like the present is the only way to obviate these misconceptions — misconceptions which I owe as much to you as to myself to remove.

The importance of a method of producing a great degree of cold becomes apparent, when it is considered, that it is at present a very common opinion among chemists,—an opinion founded on a very general analogy, that all gases may be reduced to the state of liquids by the abstraction of caloric, and

* Read to the Edinburgh Institute, on the 3d of Feb. last.

that
that by a farther abstraction of caloric all liquids in their turn, may be reduced to the solid state. If this be true, and we were in possession of a method of sufficiently abstracting caloric, all bodies whatever might be reduced to the solid state. We should thus become acquainted with a great number of substances that we have hitherto had no opportunity of examining; many powerful agents would likely be obtained; many new and interesting compounds formed, and much light could not fail to be thrown on the constitution of known substances.

Directing my attention to this subject, in the summer of 1810, a method occurred to me, by which I imagined a greater degree of cold might be produced than had hitherto been obtained. Although the power of this method appeared in theory almost indefinite, yet it was easy to foresee that in practice many circumstances might at first concur to set limits to its application; from the nature of these circumstances, however, it was to be expected, that some of them might be considerably modified, and many of them might in time be altogether removed; and thus the practice made, in some degree, to approximate to the theory.

At the time this method occurred to me, the pressure of my professional avocations did not allow me to prosecute it; but, as I anticipated some leisure in the following autumn, I immediately began to provide, at any leisure moments I had, such apparatus as I considered absolutely necessary, or was most likely to be useful. The little dependence, however, which is to be placed on general reasoning on such subjects, and the apprehension that the method might have been previously tried, and found insufficient by others, prevented me from providing any very extensive apparatus.

My first experiment was tried in the following autumn. The thermometer was filled and scaled by myself. The tube was previously tried by the common method, and found, as nearly as such tubes are commonly to be met with, of equal calibre throughout. The spirit with which it was filled was prepared by Richter's process, and afterwards re-distilled by itself. Its specific gravity at 62° was 798. The points 60° and 100° were determined by a mercurial thermometer, which had been made with the usual precautions; the interval was divided into four spaces, each of which, of course, correspond
to $10^\circ$; the part of the stem below $60^\circ$ measured nearly 18 of these spaces. A mark was made at every space, till, on arriving at the end of the 17th, the graduation could not be carried farther. This point, of course, corresponded to $+ 60^\circ - 170^\circ = -110$ deg. of Fahrenheit's scale.

This thermometer was exposed to the cold produced by the method alluded to, and after some time was examined, when the alcohol was found to have passed all the marks, and was obviously sunk within the ball of the thermometer. A slight degree of discoloration was observable. The thermometer was replaced, and examined about five minutes afterwards, when the ball of the thermometer was found broken, and crystals adhered to the fragments.

I next took a glass tube, about 3-10ths of an inch in diameter, and sealed at one end; into this I poured alcohol till it stood in the tube 4-10ths of an inch deep, and then exposed it to the cold, produced as before; after some time it was so completely solid, that on inverting the tube it did not drop, and only a very minute stream was perceived to glide slowly down the inside of the tube; when this stream had reached nearly the middle of the tube, the whole suddenly fell out, and, pitching in a glass, was broken into several pieces, which quickly melted.

This experiment was several times repeated, but by allowing the alcohol to remain a little longer exposed to the cold, it became so completely solid, that on inverting the tube, not the least portion of fluid could be perceived to separate from the mass.

In order to be as certain as possible of the strength of the alcohol I employed, I again took its specific gravity, and the result corresponded with what I before obtained.

These experiments, therefore, left me no room to doubt that I had frozen alcohol, which, at the temperature of $62^\circ$, is of the specific gravity 798.

Being appointed to deliver the course of lectures on chemistry for the session 1810-11, I had no leisure, at that time, to pursue these experiments. They were resumed, however, in the autumn of 1811. The second experiment was repeated and varied, and solid masses of alcohol of some magnitude obtaine.
tained. Some of these I soldered together, using as a hot bolt, a rod of frozen mercury, and sometimes a straw cooled down to a very low temperature.

It now appeared to me to be an object of some importance to ascertain the form of the crystals which this substance assumes. This I found attended with some difficulties, which I did not anticipate, and attempts to overcome them have led to the discovery of some facts which I did not at all expect.

The common masses exhibited crystals of different forms; two kinds appeared to predominate, and each was tolerably distinct in its kind; but it was not very easy to perceive by what increments or decrements the one could be supposed to pass into the other; a rather casual circumstance, however, explained the source of this variety. Attempting to freeze alcohol by a modification of the general process, which I conjectured would yield more regular crystals than the common method, I observed, that before crystallizing, the alcohol separated into three distinct strata; the uppermost was of a pale, yellowish green, while the second was of a very pale yellow colour; both these strata were very thin; the last mentioned was rather the thickest; the lowermost stratum was nearly transparent and colourless, and very greatly exceeded the other two in quantity. After allowing a part of the lower stratum, which I conceived to be the pure alcohol, to freeze, I attempted to pour out the remainder; but was prevented by the upper strata, which proved to be solidified. The lowermost of these two strata bore some marks of crystallization; the upper had none, and proved so firm, as to resist a straw with which I attempted to perforate it, to open a passage for the sublataent liquid. On removing part of these superior strata, and decanting the remaining fluid, the crystals of the lower stratum appeared very distinctly to be rectangular prisms of equal planes, a few of them on one side of the glass surmounted by quadrangle pyramids, but most of them by dihedral summits. This experiment I repeated several times, and the results coincided.

In order to ascertain whether these phenomena arose from a decomposition of the alcohol, or from the separation of foreign substances previously held by it in solution, the products of several of these experiments were mingled together in a stoppered matras; the whole was then raised to the temperatur...
perature of about 120 deg. by a water bath of that temperature. The substances forming the different strata united together, and formed a colourless liquor, which had the specific gravity, and all the other properties of the alcohol from which it was obtained. This experiment was repeated several times, and the results were uniform, affording sufficient evidence, that the alcohol had not been decomposed by this process, but that the superior strata consisted of foreign substances, which it had held in solution. The variety in the form of the crystals obtained by former experiments, was, therefore, most likely occasioned by the presence of these foreign substances, a phenomenon not uncommon in chemistry.

The result of these experiments led me now to perceive, that the assumption that alcohol, prepared by Richter's process, is perfectly pure, or at most contains only a very minute portion of water, is entirely gratuitous. The diluted alcohol of commerce, from which the more concentrated is obtained, is well known to contain different volatile impurities; and since Richter's process makes no provision for the separation of these, we ought rather to expect still to meet with some portion of them in alcohol prepared in this manner.

Next proceeded to examine the properties of the different substances into which I had separated Richter's alcohol; but the time I had now left for this purpose was too short for making much progress in this inquiry; a few only of their habituses with water, and one with another, were all that I had time to examine; even these I could examine only imperfectly.

The lowermost stratum, or nearly colourless fluid, which I have called alcohol, had no flavour, and produced on the organ of smell only a sharp pungent sensation. It has the remarkable property of smoking when exposed to the air, and when diluted with water it differs considerably in taste from common diluted spirit of wine.

The pale yellow substance, or second stratum, has a pungent taste, leaving an impression of sweetness. It has a very strong but agreeable smell. When mixed with the alcohol, and diluted with water, it has very much the flavour of the better kinds of highland whisky. It readily dissolves in water, and communicates to that fluid its peculiar flavour.

The pale, yellowish green substance, which composes the
uppermost stratum, has a strong and very offensive smell, and a very sharp nauseous taste. It dissolves in alcohol, to which it communicates its peculiar flavour; its disagreeable smell is considerably heightened by this combination. It dissolves in water, though less readily than the substance last treated of. The compound, when much diluted and heated, has very much the flavour of the low wine of our lowland distillers, at the time it issues from the still.

The two last mentioned substances, or those of which the two upper strata are composed, when mixed together and greatly diluted with water, have very nearly the flavour of alcohol. They have rather more volatility than water; for when half a solution of them has been distilled over, the distilled part has a much stronger smell than that which remains in the retort.

It may be proper to mention, that from the circumstance of my sense of smell having been for some time extremely obtuse, I have been under the necessity of trusting to others for the facts regarding the flavour of these new substances and mixtures; from the uniformity of the reports, however, which I have received from different persons, I have no doubt that these facts are correct.

Besides that from which I filled the thermometer in the first experiment, I have operated on alcohol of the specific gravities 802, 797, and 784; the specific gravity of the last was taken when its temperature was 60 deg. and it is probably the most concentrated that has ever been obtained. But with alcohol of all these different strengths, the general results were similar. In alcohol obtained from different sources, the proportions of the impurities were different, both with regard to the pure alcohol, and to one another, but I have met with none that did not contain both.

From these experiments I think it is ascertained;

1st. That the strongest alcohol which we are able to obtain, may be frozen by the method alluded to.

2d. That this alcohol contains at least two foreign substances, which are highly volatile, and, so far as is known, can only be separated by freezing.

3d. That it is to those substances that alcohol owes its peculiar
cular flavour, and that, according as the one or other predominates, the flavour of the alcohol is agreeable, or otherwise.

Last autumn I resumed this subject, and my attention was chiefly directed to the habitues of these impurities with the chemical re-agents. This I found attended with considerable difficulties, none of the least of which was to procure a sufficient quantity of these impurities in a separate state. The series of experiments I proposed to myself on this subject have not yet been completed; but I may remark, that the result of some of those I have made, promises to afford practical hints of considerable importance to those brewers whose products are intended to afford spirituous liquors.

From this notice it will be observed, that I have scarcely yet entered on the wide field of inquiry, for cultivation of which, the method alluded to appears to offer so powerful an instrument. Alcohol only has been subjected to experiment; it was the only liquid which had resisted all attempts to reduce it to the solid state by the abstraction of caloric. If these experiments be correct, we may now pronounce it a general law, to which there is no exception, that all liquids with which we are acquainted may be reduced to the solid state by a suitable abstraction of caloric. Whether all gases may be susceptible of reduction to the solid state, by abstraction of caloric, remains to be ascertained; although, as I have mentioned, analogy renders it in the highest degree probable.

The examination of the singular substances, which alcohol prepared by Ritcher's process contains, has drawn me aside from the course of experiments I prescribed to myself, and taken up that time which I intended to have devoted to the examination of the effects of cold on the gaseous bodies. Whether I shall proceed to these bodies, or resume the examination of the habitues of the alcoholic impurities with the re-agents, will much depend on the leisure which I can obtain; but to whichever of them I may direct my attention, I shall not fail to give the earliest information of the result to the Institute.

\textit{Annotation.}—W. N.

Remark upon As Mr. Hutton's experiments and observations, and perhaps more
more or less of his method, were communicated to his friends, it is to be regretted that he has not described it in this notice; which would, at least, have secured him against the pretensions of those who, from conjecture or otherwise, might perform the same. Without departing from the respect due to an inventor, I consider it to be quite allowable for me to make a few remarks in this place, for the gratification of such of my readers as may not be familiar with the general subject.

If we except the direct cooling process, by communication with bodies at a lower temperature, and the few instances, if any, wherein cold can be said to be produced by chemical union, without change, as to the state of aggregation, we can look to no other means of depressing the temperature of bodies, within our knowledge, but such as may be founded upon their augmentation of capacity for heat, when they pass from the solid to the fluid, or from the fluid to the gaseous state. In the first of these two methods, certain bodies, such as snow and salt, one at least being in the solid state, are mixed and combine; and if the combination be not congealable at (or its freezing point be lower than) the heat of the surrounding or neighbouring bodies, the compound will be fluid, and will take from those bodies all that heat which its increased capacity as a fluid demands, for the maintenance of that state; and consequently those bodies will be cooled,—and one limit of this process will be at the freezing point of the compound, below which it cannot go; though from the heat of the surrounding bodies, it may be prevented from arriving at that point.

But many of the freezing mixtures, at present known, seem to have their point of congelation far beneath any temperature we can practically look to; and, therefore, a very considerable part of the process of cooling by means of them has been directed to the prevention of the effect of foreign heat, by first cooling the ingredients, and surrounding the vessels with other cooling materials. Whether these precautions have been as much varied and applied, as the circumstances appear to demand, may, with justice, be doubted.

In the second method, by the evaporation of a fluid, such as water in various economical processes, and alcohol and ether in philosophical experiments, the rapidity with which the gaseous state is assumed, under like circumstances, governs the result;
result; and this rapidity will be prodigiously increased by keeping off the surrounding pressure, as in Professor Leslie's experiment. Whether there be any practical limit of temperature, below which these or all volatile or fluid bodies could be prevented from assuming the gaseous state, is, I think, beyond the reach of our inquiries.

Freezing processes may be improved by discovery or selection of a freezing mixture, and by absorbing the extraneous heat, either by Walker's or Leslie's method.

These cursory remarks upon cooling processes, may lead us to infer, in the way of conjecture, that Mr. Hutton's process may consist in the discovery or use of one of the most powerful freezing mixtures, and preventing the influence of the surrounding heat by a judicious application of the means similar to those pursued by Walker;—or, much rather, that instead of this last, he may have applied Professor Leslie's process as to the external cooling, by evaporation of ether in vacuo, to a vessel containing his freezing mixture. The apparatus for doing this, or for effecting his purpose otherwise, would demand a display of skill which we may reasonably expect will add to the philosophical reputation of Mr. Hutton.

III.

Some Remarks on the Use of Nitrate of Silver, for the Detection of minute Portions of Arsenic. By Alex. MarCet, M. D. F. R. S.*

The interesting account of the poisonous effects of arsenic, presented to the Society by Dr. Roget, and published in the second volume of the Medico-Chirurgical Transactions†, the author has recommended, for the detection of this poison, a test which I pointed out to him, and which, from a variety of experiments, which we tried together, with a view to ascertain

* Read to the Medical and Chirurgical Society of London, in December last, and by them published. It is here inserted, not only on account of its intrinsic value, but because it bears reference to Mr. Sylvester's paper in our thirty-third volume.—N.

† I take this opportunity of stating, at Dr. Roget's request, that the patient, whose case he there related, completely recovered her health, and has remained well ever since.
its comparative merits, we were induced to consider as the most effectual of all the tests hitherto used for that purpose. The method consists simply in adding, in succession, to the fluid suspected to contain arsenic, minute quantities of solutions of ammonia and of nitrate of silver; by which means, if the smallest quantity of arsenic be present, a dense yellow precipitate will be produced.

All the particulars respecting this mode of detection having been fully stated by Dr. Roget, with such references to former writers on the subject as the case required, it would be quite superfluous to enter into any further detail on this head. My object in resuming the subject, the practical importance of which need not be pointed out, is to communicate to the Society the result of an inquiry which I have made on the nature of the yellow precipitate, the appearance of which is assumed as denoting the presence of arsenic, and to answer some objections which have been made against this test by Mr. Sylvester, of Derby, in a paper on metallic poisons, recently published in Nicholson's Journal*.

The yellow compound in question has the following properties:

If, after being well washed with distilled water, it be suffered to stand for some time in an open vessel, it gradually passes to a brown colour; but it does not, like nitrate of silver, become black on continuing this exposure.

It is readily soluble in dilute nitric acid. It also dissolves on adding an excess of ammonia at the moment of its formation; but after it has been separated and dried, it is no longer sensibly soluble in ammonia.

If a small quantity of this precipitate be exposed to the heat of a lamp on a slip of laminated platina, a white smoke arises from it, and metallic silver remains attached to the platina. The reduction of the silver, in the form of a globule, is still more distinct and striking, if a little carbonaceous matter be mixed with the precipitate, and the blowpipe applied.

When the yellow precipitate, inclosed in a tube, is exposed to the heat of a lamp, the white smoke condenses on the cold part

part of the tube, in minute octaedral crystals of arsenious acid.

It is an arsenite of silver.

It appears, therefore, that the precipitate in question is a combination of white arsenic (arsenious acid) and silver, or an arsenite of silver; and it is inferred that its formation, when ammonia and nitrate of silver are added to a mixture containing arsenious acid, is owing to a double elective decomposition of the arsenite of ammonia, by the nitrate of silver, in consequence of which arsenite of silver is formed, and separates as an insoluble precipitate from the nitrate of ammonia which remains in the solution. The addition of ammonia is necessary, because arsenic acid alone cannot decompose nitrate of silver; but in Fowler's solution, in which the arsenic is already combined with an alkali, the decomposition takes place at once, without any addition of ammonia. The fixed alkalies, therefore, can accomplish a similar purpose; but ammonia has this advantage, that it does not, when added singly, decompose nitrate of silver, a circumstance which, in using the fixed alkalies, might occasion some confusion.

With regard to Mr. Sylvester's objection, I shall, previous to my offering any remarks upon it, state it in his own words.

"If ever muriatic acid be present," says this gentleman, "the test is then wholly useless, as a muriate of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing."

This danger of ambiguity, however, though applying in some degree to the process in question, and well deserving to be noticed, will be found to have been greatly overrated; and

* It is necessary, as Dr. Roget has observed in the paper already quoted, that the quantity of ammonia should not be too large, for in that case the precipitate is redissolved. But, even then, it may be made to reappear, by the addition of nitric acid in sufficient quantity to saturate the alkali. In this case, however, the precipitate is not permanent, owing, I find, to its being soluble in the nitrate of ammonia which is formed in the process. Carbonate of ammonia has also the property of producing and redissolving the precipitate.

The fixed alkalies in excess have not the power of redissolving the precipitate.
there are such easy and obvious means by which this ambiguity can be entirely removed, that it can make no solid objection to the utility of the test.

There cannot be the least doubt, as Mr. S. observes, but that whenever nitrate of silver is added to a solution containing muriatic acid, a precipitate of muriate of silver must be the consequence. But if the nitrate of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts.

If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriate of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner, and becomes even more characteristic from a comparison with the white precipitate, the appearance of which differs from this in every respect.

By this method, I believe that every objection to the test will be removed; and in order to anticipate all ambiguity, and to avoid any complication or practical difficulty in its application, I would propose to modify the process in the following manner:

To the suspected fluid, previously filtered, add, first, a little dilute nitric acid, and, afterwards, nitrate of silver, till it shall cease to produce any precipitate. The muriatic acid being thus removed, whilst the arsenious acid (if any, and in whatever state,) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain.
### IV. METEOROLOGICAL JOURNAL

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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.

1812. Twelfth Month. 25. A very slight fall of snow. 27. A little snow last night. 30. 31. Windy night: small rain at intervals.

1813. First Month. 1. Small rain at intervals. 3. Misty morning. 5. Windy. 6. Windy: small rain. 7. Very misty, a. m. dark and cloudy, p. m. About 8, some lightning, which was soon followed by a shower. 9. Hoar frost: at 9, a. m. thick air, with Cirrostratus and Cirrocumulus: sounds come freely from the city, with the wind at S. S. W. Sleet and rain followed within an hour. 13. Overcast, a. m. thin sleet and rain. 14. Cloudy. 9. A little snow, a. m. 22. Clear, p. m. A fine red burst in the horizon at sun-set.

RESULTS.

Winds variable.

Barometer: greatest observed height, 30.52 in.; least 29.30 in.

Mean of the period 30.022 inches.

Thermometer: greatest height 50°; least 23°.

Mean of the period, 36.085.

Evaporation 0.56 inches. Rain and snow 0.51 inches.

Plaistow,
First Month, 23, 1813.

L. Howard.
EXPLOSIVE COMPOUND OF CHLORINE AND AZOTE.

V.


To Mr. Nicholson.

SIR,

In the beginning of December, 1812, we learned from some of the newspapers, and from other sources, that a new explosive compound had been discovered at Cambridge, by Mr. Burton; that it was supposed to be a compound of chlorine and azote; that its explosive properties were of the most terrible kind, and had occasioned a very serious accident to Sir H. Davy, who was examining it; that the contact of oil would cause it to explode; that it was formed by exposing a solution of nitrate of ammonia to chlorine gas; and lastly, that the application of a freezing mixture during its formation was advantageous.

Such is the sum of the information which we then obtained, and which stimulated us to undertake a number of experiments with this compound; we have not since procured any further information respecting it, excepting such as we have derived from our own experiments. We state this, in order that your readers may have the means of distinguishing from among our experiments, those few which are not original.

We shall now proceed to relate our experiments, beginning with those which concern the formation of the compound.

The mode which we adopted for forming it, was, in every instance, to fill with chlorine gas, over warm water, glass receivers of the capacity of about sixteen cubic inches; and to transfer these into small basins, containing the ammoniacaal saline solutions. We soon found that the compound could be formed with solutions of other ammoniacaal salts besides the nitrate: those which we have successfully employed for obtaining it, are the following:

Other ammon. Salts which form the compound.

Sulphate of ammonia,

Phosphate do.

Muriate do.

Nitrate do.

Oxalate.
Oxalate of ammonia,
Muriate of zinc, with excess of ammonia,
Muriate of ammonia, and iron by sublimation.

Those with which we did not succeed in forming it are the
undermentioned.

Carbonate of ammonia,
Triple muriate of platina and ammonia,
Sulphate of copper, with excess of ammonia,

We wished to ascertain whether any other solution contain-
ing azote might be substituted for the solution of ammoniacal
salt. The solution which we tried with this view was one of
nitrate of lead at a minimum, but we could not obtain by its
means any of the explosive compound. We have not yet made
any other experiments of this nature.

There are certain bodies which, if present during the pro-
cess for forming the explosive compound, prevent its formation,
or at least prevent it from appearing. Of this description of
bodies we have observed the following:

Sulphur, in solution in the ammonia,
Do. in powder within the receiver,
Charcoal in fine powder, adhering to the inte-
rior moist surface of the receiver,
Carbonic acid gas, equal in volume to one-
third the chlorine gas,
Atmospheric air, do. do.
Hidrogen gas, equal in volume to the chlorine
gas.

With respect to the temperature best adapted for the forma-
tion of the compound, our experiments lead us to quite an op-
posite conclusion from what has been published. The em-
ployment of a freezing mixture, instead of being advan-
tageous, we have found to be the reverse, as we have never
succeeded in obtaining the compound when the solution and
the gas were at a temperature below 32°. In these instances,
a thin crystalline icy film, was observed to line the sides of
that part of the receiver containing the gas, and unless this was
dissolved again by raising the temperature, no explosive com-
pound was produced. On the contrary, when we have em-
ployed solutions of ammoniacal salts at the temperature of 90°,
the explosive compound has been abundantly and quickly
formed.

Others which
did not.

Nor did ni-
trate of lead,
at the mini-
umm.

Sulphur, char-
coal, carbacid,
atmos. air,
and hidrog.
gas prevent
the compound
being formed.
formed. In one experiment we heated the solution to 180°, and observed, in ten minutes after, when about half the gas was absorbed, and the temperature had lowered to 125°, that the receiver above the fluid was covered with the explosive compound, which trickled down to the surface of the solution in minute globules, which converged from all parts of the circumference of the circle forming the surface of the solution to the centre of that circle where they accumulated into larger globules. This phenomenon, which had a very beautiful appearance, seemed to us to be owing to a distillation of the compound from the central or hottest part, and a condensation at the exterior or coldest part of the receiver. This distillation ceased when the temperature had lowered to 110°, and the explosive compound then formed a film on the surface of the solution.

The phenomena attending the formation of the compound, are the following:

As soon as the receiver of chlorine gas is placed in the solution of the ammoniacal salt, an absorption of the gas commences, and the solution rises slowly in the receiver. An action is apparent on the surface of the solution, which resembles small filaments reaching to the depth of about one-tenth of an inch. These filaments, on close inspection, appear to be composed of extremely minute bubbles of gas, ranged in a line one above another to the surface. When about one-fourth of the gas has disappeared, some of the explosive compound may generally be observed on the surface of the solution in a thin film; the surface then looks oily, and appears divided, so as to give the idea of a map. As the solution rises in the receiver, the quantity of the explosive compound increases; and it then collects into one or two flattened globules, which when they become very bulky, fall through the solution to the bottom. The whole of the gas is absorbed. The solution, after the formation of the compound, contains free muriatic acid, and also some of the compound in solution, if we may judge from its smell and yellow colour. We are not aware, that there are any other appearances during the formation of the compound, which are material to notice.

The following appears, from our experiments, to be the theory of the formation of the explosive compound,

When
When an aqueous solution of muriate of ammonia is brought into contact with pure chlorine gas, one part of the chlorine is dissolved in the solution, and there decomposes the ammonia of the salt, by combining with its hydrogen, (with which it forms muriatic acid,) and sets free its azote, to combine with another part of the chlorine, with which it forms the explosive compound. The compound which is at first formed in this manner, is not visible because it is soluble in chlorine gas, and there is at first an excess of that present; but in proportion as the quantity of this gas diminishes by combining with the elements of the ammonia, the explosive compound appears, and is deposited by the gas, generally on the surface of the solution, but sometimes considerably above it on the upper part of the receiver. The former effect is most likely to take place when the upper part of the receiver is in the form of a dome, or circular; the latter, when it is in that of an inverted cone, or funnel shaped. The relative temperatures of the surface of the solution, and of that of the top of the receiver, have also, as might be expected, a considerable influence in determining where the compound shall be deposited. Its natural situation, from its high specific gravity, is at the bottom of the solution; but unless it is in large quantity, or has been agitated, it remains where it is formed, on the surface of the fluid; preserving that situation by taking a flattened spherical form, like that which a heavy oil assumes on the surface of water.

The explanation above given of the formation of the compound from solution of muriate of ammonia, applies equally when solutions of any other salt, formed of an incombustible acid and of ammonia, are employed; the nature of the incombustible acid (with the exception of the carbonic) being of no importance, the only use of the acid being to prevent, by engaging the ammonia, the rapid action which the chlorine gas would exert on that alkali in an uncombined state: the existence of it in that state would also be incompatible with that of the explosive compound. This last assertion may appear extraordinary to those who know that the explosive compound may be formed by confining chlorine gas over a solution of pure ammonia; but it is nevertheless true; for in this case the explosive compound, although apparently formed from pure ammonia, is, in fact, formed from the muriate of that alkali; which
which muriate is one of the products of the exposure of pure ammonia to chlorine gas.

Two different results are obtained from the mutual action of chlorine and ammonia, depending on the proportions of the two bodies presented to each other. Thus, when the quantity of ammonia present in a free state, is more than the chlorine gas can decompose and neutralize, the whole of the chlorine gas goes to the formation of muriate of ammonia, and no explosive compound is formed, but in its stead azotic gas is found at the termination of the experiment, equal in volume to one-third of that of the chlorine gas employed. Thus the only products of the experiment, under these circumstances, are the muriatic acid of the muriate of ammonia, and the azotic gas.

But when the quantity of chlorine gas present is more than is necessary to bring the ammonia to a neutral state; or, which is still better, when the ammonia has been previously neutralized by an acid, the azote, instead of remaining after the experiment in a state of gas, is found combined with the superabundant chlorine, forming the explosive compound. Thus the products of the experiment, conducted in this way, are, the muriatic acid which remains in the solution, and the explosive compound.

In the case first stated, the chlorine combines with one of the elements of the ammonia only, viz. the hidrogen; in that last described, it combines with both, viz. the hidrogen and the azote.

We shall here relate an experiment made with the intention of ascertaining the proportions of chlorine and azotic gases, which, in a condensed state, form the explosive compound.

Two globules of the explosive compound produced from equal quantities of chlorine gas, and apparently of the same size, were decomposed; the one by potash dissolved in water, the other by solution of pure ammonia; the gases from each were collected and measured; that from the first was 0.8 of a cubic inch, and that from the last 1.1.

Phosphorus was heated in both; in that produced over the solution of potash it burnt, and caused its volume to diminish to 0.66; in that produced over the solution of ammonia, it did not burn, and caused its volume to increase to 1.3.

Now,
Now, if we suppose the two portions of gas, after the action of the phosphorus, to be in the same state, i.e. to be phosphuretted azotic gas, each containing, with respect to their volume, the same proportion of phosphorus, it will not be necessary, for the following calculation, to make any correction for the augmentation in bulk occasioned by the phosphorus; and as the circumstances of temperature and pressure were the same with both, neither will any corrections be necessary for those circumstances—we may, therefore, consider the comparative volumes of azotic gas produced in the two experiments, as represented by 66 and 130, and their difference as 64, being the excess of azotic gas produced over the ammoniacal solution. If we multiply this by 3, (the volume of chlorine gas necessary to produce 1 part of azotic gas from ammonia) we shall have 192, which will represent as gas the quantity of chlorine in one of the globules. And the quantity of azote, brought to the state of gas from the other, being, according to the first experiment, 0.66, makes the composition of the explosive compound to be nearly three parts of chlorine gas to one of azotic gas, condensed to a degree which we have not yet estimated.

We do not state this analysis as deserving much confidence—it must be frequently repeated before we can put any faith in it ourselves.

Our principal motive in describing the above experiment, before we have had an opportunity of repeating it, is to shew an easy and practicable mode of analysing the compound.

It may be proper now to describe some of the physical properties of the explosive compound.

Its colour is that of bees' wax; it is very fluid; it sinks, although with extreme slowness, in a solution of red sulphate of iron of the specific gravity of 1.578. Hence we conclude, that it must be of the specific gravity of about 1.6. It disappears after some time, even under the surface of water, or of the solution in which it was formed; but evaporates almost instantaneously when exposed to the air; it then diffuses its peculiar and penetrating odour through the surrounding atmosphere, which then affects the eyes in a very painful manner, causing them to shed tears. Its action on the lungs, however, we conceive to be much milder and less prejudicial than that of chlorine.
EXPLOSIVE COMPOUND OF CHLORINE AND AZOTE.

chlorine gas, as we have experienced very little inconvenience in this respect from standing close to a solution, from the surface of which the compound was, diffusing itself into the atmosphere.

Very volatile, but may be kept in a close vessel.

The volatility of the compound is so great, as to present a considerable obstacle to preserving it; we have, however, found, that by limiting the quantity of air or of fluid which can come into contact with it, and at the same time preventing the escape of vapour by pressure, it can be kept for any length of time. We have accomplished this by introducing the compound into small tubes, closed at one end, about nine inches long, being first filled with some of the solution. The compound should occupy at least half an inch from the bottom of the tube, and some of the solution should afterwards be taken out to leave room for a little air, and to allow of the open end of the tube being hermetically sealed before the blowpipe. When any of these tubes are afterwards broken, the escape of compressed vapour is so considerable, as to occasion a loud report.

Difficult to transfer, because so volatile.

In our first experiments with the explosive compound, we experienced considerable difficulties in transferring it from one vessel to another, as we had no better mode than that of introducing into the solution and under the compound, a small spoon of tinned iron; the motion which this communicated to the compound often carried it to the surface, where it extended itself and disappeared, by dissolving in the atmosphere. In order to remedy this, and other inconveniences attending on this method, we invented a little instrument which we have found to answer our most sanguine expectations; it is formed of a small glass tube, of the size of a large writing quill, open at one end, and closed at the other, in the manner of a test tube, with the exception of a small circular hole in the centre. This tube is to be used as a syringe, the piston of which is to be formed of cotton, wound round a piston rod of wood or copper; by raising or depressing which, the explosive compound may be drawn into, or ejected from, the tube with the greatest facility. The peculiar advantages of this instrument are, its taking up the compound with so small a quantity of the solution, and with so much celerity, and its retaining it when he
EXPLOSIVE COMPOUND OF CHLORINE AND AZOTE.

A precaution very necessary to be taken in the use of this instrument is, that it be clean, or at least free from oil, grease, or any combustible matter, which might, by causing the compound to explode, occasion a serious accident. This precaution is also very necessary with respect to all other vessels with which the compound may come into contact. Another general precaution, which we strongly recommend to those who may make experiments with this compound, is, to wear a mask on the face, and gloves on the hands. We conceive it also very proper to state, that although the results of upwards of two hundred different experiments which we have made with this compound are in favour of the conclusion, that it will not explode without the contact of a combustible body, or the application of a temperature exceeding 200°; yet three explosions have taken place, the causes of which remain unknown to us, as we were not aware of the compound being in contact with any other body than cold water. These explosions were, therefore, completely unexpected by us; but fortunately, they did not occasion any accidents of a serious nature.

The effects of different temperatures on the compound we considered as very deserving of investigation, for which reason we made the following experiments:

A globule of the explosive compound was introduced into a small tube filled with water, it immediately fell to the bottom. The tube, with its contents, was then placed into a mixture of snow and nitric acid, into which a thermometer was also placed. The mercury fell to —10°; the water in the tube was of course solidified, but the compound retained its fluidity, and was not altered in any respect.

A globule of the compound was introduced into a tube, closed at one end, of the form represented in fig. 4.

It was not frozen at —16 deg. At 160 deg. it came over by distillation.
pl. V: it was previously filled with the solution from which the compound was formed.

The globule is represented at $a$; the bent part of the tube was then placed in a vessel of water, and its open end immersed into a wider tube, also filled with some of the same solution. The vessel containing the water was then heated. When the temperature approached to 160°, it began to distil; at 160° the distillation was rapid, the compound being converted to vapour in the bent tube, which served as a retort, and was condensed and collected in that which was disposed as a receiver—much gas was given out during the process. The double curvature of the retort tube was to prevent any of the compound being floated over by bubbles of gas attaching themselves to it. This precaution we found was very necessary.

A globule of the compound covered with water, contained in a little spoon of tinned iron, was with the spoon introduced into a quantity of water heated to 200°—this temperature was not sufficient to make it explode; it merely occasioned its vaporization.

But it did very violently at 212°. The last experiment was repeated, varying only the temperature of the water, which, in this instance, was 212°; the compound immediately exploded violently.

These experiments prove, that the explosive compound does not assume the solid form at — 16°, that it may be distilled at or below 160°, and does not explode but at a temperature above 200°, suddenly applied.

Our next object was to ascertain whether, when the natural pressure of the atmosphere was taken off, or diminished, from the explosive compound, it would still retain the fluid form, or whether it would assume the elastic state—with this view we made the following experiment:

A tube, 3½ inches long, closed at the bottom, had another tube of smaller bore, but of the same length, and open at both ends;
ends, placed within it; both tubes were then filled with mercury, excepting about one-fourth of an inch at the top. This one-fourth of an inch was afterwards filled with the following—

1st. A small glass cup containing the explosive compound covered with a drop of muriate of lime. This cup moved freely within the tube.

2d. Muriate of lime in solution, surrounding and rising above the glass cup.

3d. A glass stopper, ground to the tube, and closing it accurately.

The inner tube was then raised thirty inches above the level of the mercury in the outer one. The column of mercury in the inner tube descended seven inches, leaving a column of twenty-three inches only. These seven inches were occupied by the explosive compound in a state of vapour: but as a little of it still remained in the cup, not converted to that state, a temperature of about 100° was applied to it—this caused it to disappear, and the vapour, after cooling, then occupied another inch, the mercurial column being reduced to twenty-two inches. The tube being now lowered until the mercury within and without it were of the same level, the explosive compound reappeared. There remained, however, seven-eighths of an inch of permanent gas, which an accident prevented us from examining; but we are inclined to believe, that this small quantity was produced when, on lowering the tube, the mercury rose into that part which had been occupied by the vapour, and the sides of which had been wetted with the liquid muriate of lime, which, notwithstanding that it was very concentrated, had probably absorbed some of the vapour, as we observed some bubbles of gas rising through the mercury from that portion of the metal which was in contact with the humid surface.

We repeated the above experiment in the hope, that by applying heat sufficient to make the vapour explode, we might, by this means, analyse the compound. We, therefore, exploded the vapour by surrounding the glass tube with part of a gun barrel heated nearly to redness; but at the instant of the explosion the tube was shattered. We, however, propose to repeat this experiment with a tube of greater strength.
We have made a great many more experiments with the explosive compound; but as this communication is already of considerable length, we shall reserve the account of them for the next number of your Journal.

We are, Sir,
Your most obedient, humble Servants,
R. PORRET, Jun.
W. WILSON.
RUPERT KIRK.

London, 16th Feb. 1813.

VI.

A Statical Blow Pipe, with Remarks by C. L.

The mercurial pump of Haskins, described by Desaguliers in his lectures; the water bellows of Hot blower in your 1st vol. octavo; the statical lamp, Edelcrantz, in your 5th volume, together with the gauge, in Wodzis, most ingenious apparatus for heating water by waste steam, in your 2nd vol., are among the useful applications of a fluid substituted instead of packing, or leathering, for a moveable piece of the nature of a piston. On the present occasion, I send you an application of the same description to a blow pipe which acts upon the principle of the regulating piston in large works.

The body of the instrument, BB EE, consists of a cylinder, having another interior cylinder, of rather smaller diameter, securely joined to the outer one, at the lower rim of the former, so that both cylinders are concentric, and both open at top; the edge of the inner cylinder being rather the highest. The outer cylinder is set airtight in the foot EF, and communicates with the lower space D, which has connection with the mouth tube C, and the blow pipe D, by channels which have no other issue. A is a metallic cylinder, closed at the end A, and open at the other, which is the lower end. The letter C denotes a weight connected with the top of A by an inflexible wire or stem. This weight may be changed for one either greater or less, according to the intended force of the blast which is governed by it. The diameter of the cylinder A is such, that it may be inserted mouth downwards in the space between the other two cylinders; and if mercury be then poured into that space to about half its depth, the inte-
nal part of the apparatus will have no communication with the external air, except through G or F, and the mercury will stand on a level on both sides of A. But if the mouth be applied at G, and air blown in, it will be emitted again at F, with a velocity which will be greater the greater the pressure; but it will not be in the power of the operator to carry this pressure beyond a precise and steady limit. For the first effect will be to depress the mercury within the tube A, and at the same time to elevate the outer column or ring; and as soon as the difference between the heights of the internal and external mercury shall have become equal to that of a column of mercury, having the same base as that of the internal part of A, any farther effort will only cause A to ascend; and as soon as this ascent shall have carried the lower rim nearly to the height of the internal mercury, the air will make its escape through the mercury, by means of a notch made in the rim to determine the place of escape.

Chemists will perceive, that though this instrument possesses facility and precision of action, and is rendered a snug portable apparatus by a cylindrical cap which covers the whole by screwing on at EE; yet in point of invention it cannot claim to differ much from the modern gas holders. You have proved to us, on a former occasion, (Journal, quarto series, II. 35.) that the difference of 4-tenths of an inch of mercury was as much as blowing by the mouth can support or maintain; and this blast is sufficient for all the purposes of mineralogy and glass blowing. It may also be noted, that a reaction blow pipe, having a packed piston to re-act in a cylinder, and, I believe, another working piston with a valve and proper fittings in the same cylinder, was made many years ago by the celebrated Ramsden. But his compounded apparatus differs in many respects from the subject of the present description.

I am, Sir,

Your obliged Reader,

C. L.