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### IV. The Bakerian lecture for 1809. On some new electrochemical researches on various objects, particularly the metallic bodies, from the alkalies, and earths, and on some combinations of hydrogen

Humphry Davy Esq.

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*negative signs* would thus become unnecessary, and no confusion could arise from their use; while on the other hand, every true friend of science and accuracy, will naturally adhere to the divisions *in use*, as answering their intended ends, although not in the best or most perfect manner; and set their face against imperfectly contrived changes.

It would have been desirable, in the last note at bottom of page 420, that Mr. R. Walker had pointed out a material circumstance affecting the uses of mercurial and alcoholial thermometers, viz. the very different periods of time necessary, for each to act in, or acquire and indicate the temperature of any medium under experiment.—See the *Monthly Magazine*, vol. xvii. p. 213.

I am, sir,

Yours, &c.

July 3, 1810.

LONDINENSIS.

IV. *The Bakerian Lecture for 1809. On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* By HUMPHRY DAVY, Esq. Sec. R.S. F.R.S.E. M.R.I.A.\*

[Continued from vol. xxxv. p. 415.]

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

ONE of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German

\* From *Philosophical Transactions* for 1810, Part I.

chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data; the early discovery of Priestley on the passage of gases through red-hot tubes of earthen-ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity\*. To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubical inch; it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about  $\frac{1}{10}$  of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than  $\frac{1}{4}$  of the volume of the water, *i. e.*  $\frac{1}{4}$  of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2.6 of hydrogen, and 3.4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at

\* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. i. page 301.

the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled  $\frac{2\frac{1}{2}}{100}$  of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure;—a result which seems to show, that nitrogen is not formed during the electrical decomposition and recombination of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion; that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated\*. The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the

\* Nicholson's Journal, August 1809, p. 258.

other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about  $\frac{1}{8}$  of a cubical inch each, and filaments of asbestos were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestos was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestos with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese,

ganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearl-ash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition

of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal\*. The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about  $\frac{1}{30}$  of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about  $\frac{1}{10}$  of the volume of the water. It proved to contain about  $\frac{1}{10}$  oxygen,  $\frac{2}{10}$  hydrogen, and  $\frac{6}{10}$  nitrogen.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some

\* *Mém. d'Arcueil*, tom. ii. page 485.

time,



time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gases when they were mixed in equal proportions, was to rather less than  $\frac{1}{4}$ , which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at 29.5<sup>in.</sup>, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30.25<sup>in.</sup>, and thermometer 52° Fahrenheit; when it equalled 2.3. The gas proved to be about  $\frac{1}{4}$  nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen, hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompose nitrogen, by agents which I con-

ceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant: as soon as the contact with the potassium was made there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decomposed in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal  $\frac{1}{4}$  of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was  $\frac{1}{11}$  of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to  $\frac{1}{4}$  of the nitrogen, was produced. It cannot be said that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the  
form

form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenour of these inquiries cannot be considered as strengthening in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical

cal inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3.6 cubical inches; which consisted of 2.5 hydrogen, and 1.1 nitrogen. The potassium produced in the operation was sufficient to generate 3.1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hasenfratz \*, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2.3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions †, a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the *naphtha*, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the *naphtha*: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451 ‡, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me,

\* *Journal des Mines*, Avril 1808, p. 275.

† *Phil. Mag.* vol. xxxiii. page 8.      ‡ *Ibid.* vol. xxxiv. page 339.

against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation ; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation ; but I was aware that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance ; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen : and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity ; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia : the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture,

moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to  $-15^{\circ}$ , there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73.74 to 27.26; the proper corrections being made, and the precautions before referred to being taken\*.

Assuming the common estimations of the specific gravity of ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as  $\frac{1}{11}$  or  $\frac{1}{12}$  of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long

\* Philosophical Transactions 1809, page 459. M. Berthollet, Jun. in the second volume of the *Memoirs of Arcueil*, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to over-rate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

continued

continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30·5<sup>th</sup> barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches ..... 29·8 grains.

For hydrogen, ditto ..... 2·27

For ammonia ..... 18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of  $\frac{1}{3}$ \*; and if the smallest proportion be taken, the loss will be nearly  $\frac{1}{3}$ .

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is possible, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if ammonia be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

\* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1 grains, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4 = 1, and at the rate of 180, 133 of hydrogen weighing 3·01 and 47 of nitrogen weighing 14, and 18·4—17 = 1·4.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of  $-20^{\circ}$  Fahrenheit, in a close tube I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from  $\frac{1}{10}$  to  $\frac{6}{10}$  of hydrogen produced.

As it does not seem possible to obtain an amalgam in an aniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases\*.

If

\* Even in common air, the amalgam evolves hydrogen and ammonia; nearly in these proportions, and in one experiment which I lately tried, there



If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at  $\frac{1}{1000}$ ; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 30 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about  $\frac{1}{1000}$  of new matter, on the antiphlogistic view, and about  $\frac{1}{900}$  on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied

there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

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to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

[To be continued.]

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V. *Description and Analysis of the Meteoric Stone which fell at Weston, in North America, the 4th December 1807.* By DAVID BAILIE WARDEN, Esq. Consul-general of the United States at Paris\*.

#### DESCRIPTION.

THIS aerolite presents, in general, the same characters as those hitherto examined. It is enveloped with a thin, black, and uneven crust. The mass is principally composed of a granular substance, which breaks easily; it has an earthy appearance and a gray cinereous colour, which, in certain parts, passes to a whitish gray.

Those portions which possess this last tint, and which are as if glued in the mass, have a round form, so that they are distinguished by circular or oval spots which interrupt the general colour. Its specific gravity is about 3.3: the sharp parts cut glass.

In observing the fractured parts of the stone, we there perceive: 1°. Particular globules which are easily detached; little cells in which they are placed, and of which the substance is like that of the stone itself, except that its grain is more compact, and its fracture smoother.

In exposing it to a strong light, we see traces of a lamellar tissue: 2°. grains of metallic iron, which, by polish, assume a whiteness, yield to the hammer, and attract very sensibly the magnetic needle: 3°. grains of oxidized iron of the colour of rust: 4°. metallic particles extremely small, of a silver white colour, which seem to be of iron; and this opinion is strengthened, when we recollect that the native iron of Kamerdorf, and that of pseudo-volcanic origin, present, in certain parts, a silver white colour.

I have not seen any mark of sulphurated iron, although I found it by the analysis, as will hereafter appear.

All the fragments of this stone have a magnetic property, but without polarity; and the iron, which is very visible in certain parts, is so disseminated in all others where it escapes the eye, that the property of which there is ques-

\* From *Annales de Chimie* of March 1810.

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