# XXVII. *New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for* 1808. *By* Humphry Davy, *Esq. Sec. R. S. Prof. Chem. R. I.*[*]

## 1. *Further Inquiries on the Action of Potassium on Ammonia and on the Analysis of Ammonia.*

The most remarkable circumstances occurring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogene, and the conversion of a part of the potassium into potash.

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hydrogene, or a new species of inflammable aeriform substance, the basis of nitrogene?

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxygene, it generates pure water only, and

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[*] The account of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809. The paper was ordered to be printed March 16, 1809. At that time, having stated to the Council that I had since made some new experiments on this matter, and on the subjects discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of the former observations for publication.

its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hydrogene.

Another query which I put is, has nitrogene a metallic basis which alloys with the metals employed in the experiment?

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combusion in oxygene, and by the action of water.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hydrogene alone was evolved.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered in its appearance, and did not effervesce in the slightest degree when thrown into water.

That the nitrogene which disappears in this experiment is absolutely converted into oxygene and hydrogene, and that its elements are capable of being furnished from water, is a conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of error could escape.

The results, though not perfectly uniform, have all been of the same kind as those described in page 55. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12.5 cubical inches of well dried ammonia. From 5.5 to 6 cubical inches of hydrogene were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1.5 to 2.5 grains of potassium regenerated.

The quantity of ammonia in the products, varied from a portion that was scarcely perceptible to one twelfth or one thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise, more potassium was revived; and the unabsorbable elastic fluid, and particularly the hydrogene in smaller proportion.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogene to the hydrogene diminished as the process advanced.

The first portions contained considerably more nitrogene in proportion, than the gases evolved during the electrization of ammonia, and the last portions less.

I shall give the results of an experiment, in which the gases produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29.8, thermometer 65° FAHRENHEIT.

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

5.8 cubical inches of hydrogene were produced.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and half filled with hydrogene, the adaptors connected with the mercurial apparatus contained .8 of common air.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7.5 cubical inches. It contained .6 of ammonia, 7 of the residuum detonated with 4½ of oxygene gas left a residuum of 4.

The second portion, equal to 3 cubical inches, contained no ammonia. 7.2 measures of it, detonated with 3.8 of oxygene, left a residuum of 3.5.

The third portion was equal to 5 cubical inches; at this time the tube was white hot; it contained no ammonia, 8.5 of it detonated with 4.5 of oxygene diminished to 2.5.

The last portion was a cubical inch and half, collected when the heat was most intense. 4.5 measures, with 3.75 of oxygene, left a residuum of 2.8.

The iron tube contained, after the experiment, (as was ascertained by admitting hydrogene when it was cool), 2.7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained .8 of a similar gas.

The tube contained potash in its lowest part, and in its upper part potassium, which gave by its action upon water 1¾ cubical inch of hydrogene.

As the largest quantity of hydrogene is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gases being in the nascent state, its power of action upon them would be greatest, it occurred to me, that if nitrogene was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29.5, thermometer 70° FAHRENHEIT.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray.

The iron tube and the adaptors (having together a capacity equal to 4.3 cubical inches) contained common air.

The gas was collected in three portions, there was no absorbable quantity of ammonia in either of them.

The first portion, that produced before the tube became red, was eight cubical inches. 10.25 of it detonated with 3.5 of oxygene diminished to 8.

The second portion equalled five cubical inches; $9\frac{1}{3}$ of this, with 5 of oxygene, left a residuum of $3\frac{3}{4}$.

Of the third portion, 2 cubical inches and $\frac{1}{3}$ came over. 9 of it, detonated with 5 of oxygene gas, left a residuum of 1.4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hydrogene 2.3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogene to the hydrogene, in the case in which the olive-coloured substance is distilled with potassium, than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogene, and the addition of oxygene to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed in page 55, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxygene of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs in different cases different quantities of oxygene and of moisture; during the intervals of the removal of the different portions of gas likewise, some globules are lost.

In instances when the heat has been more rapidly raised, I have generally found more potassium destroyed, and less nitrogene in proportion in the aeriform products. In such cases likewise, the loss of weight has been much greater; the gases have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely that small quantities of the dark gray substance described in page 50 of this Volume, are sometimes carried over undecompounded in the operation.

In some late experiments, I substituted for the iron tube, a tube of copper, which had been bored from a solid piece, and the sides of which were nearly a quarter of an inch in thickness. My object in using this tube was not only to prevent the heat from being too rapidly communicated to the fusible substance, but likewise to be secure that no metallic oxide was present, for though the iron tubes had been carefully cleaned, yet still it was possible that some oxide, which could not be separated from the welded parts, might exist, which of course would occasion the disappearance of a certain quantity of potassium.

I shall give the results of one of the processes, which I regard as most correct, made in the tube of copper. The barometer was at 30.5; thermometer was at 59° Fahrenheit.

The tube contained two cubical inches and half, and was filled with hydrogene.

6 grains of potassium, which had absorbed 13 cubical inches of ammonia in a copper tray were employed.

The adaptors connected with the mercurial apparatus and the stop-cocks, contained .7 of atmospherical air.

The gas given off was collected in two portions.

The first portion was equal to 11 cubical inches. It contained .8 of ammonia, 11 of the residuum, detonated with 8 of oxygene, left 8.

The second portion equalled 2 cubical inches. They contained no ammonia. 10 of this gas, with 8 of oxygene detonated, left a residuum of 10.

There remained in the tube and adaptors 1.1 cubical inch of gas.

The quantity of hydrogene produced by the action of the potassium, which had been regenerated, equalled 4.5 cubical inches.

In this experiment the heat was applied much more slowly than in any of those in which the iron tube was used, and even at the end of the operation, the temperature was little more than that of cherry red.

In the upper part of the stop-cock there was found a minute quantity of gray powder, which gave ammonia by the operation of moisture.

In no case, in which I have used the copper tube in like processes of slow distillation, has there been less than 4 grains of potassium revived; and the proportion of nitrogene to the hydrogene in the gas evolved has been uniformly much greater than in processes of rapid distillation in the tubes of iron; but the whole quantity of elastic matter procured considerably less.

Copper has a much stronger affinity for potassium * than

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* Copper heated in potassium speedily dissolves, and diminishes its fusibility; but potassium requires a white heat to enable it to combine with iron. In another experiment, in which I distilled the fusible substance in an iron tray, contained in the copper tube, a considerable quantity of copper, that had been dissolved, was found in the state of powder deposited upon the tray, or loose in the bottom of the tube.

iron. It occurred to me as probable, that this attraction, by preventing the potassium from rising in vapour at its usual temperature, and likewise by the general tendency of such combination to give greater density, might occasion a diminution of its action upon the nitrogene in the nascent state. Ammonia has a strong attraction for the oxide of copper, and it consequently is not unlikely that the fusible substance may combine with metallic copper, and that this compound may not be entirely destroyed in the distillation. And assuming this, it may be conceived that the loss of hydrogene partly depends upon some combination of the basis of ammonia with copper.

I had a tube, of the capacity of $2\frac{1}{4}$ cubical inches, made of wrought platina, cemented by means of fine gold solder. The fusible substance was obtained (as usual from six grains of potassium) in a tray of platina, where it was brought in contact with a large surface of platina wire; the distillation was slowly conducted; but before the temperature of the tube had approached to that of ignition, it dissolved and gave way at the points where it was soldered, and a violent combustion took place. Only 7 cubical inches of gas were collected; but of this, allowing for the hydrogene that filled the tube, nearly $\frac{3}{8}$ were nitrogene.

I am making preparations for performing the experiment in a bored tube made from a single piece of platina, and likewise in tubes made of other metals, and I hope to be able, in a short time, to have the honour of laying the results before the Society.

I shall make no apology for bringing forward the investigation in its present imperfect state, except by stating that my motive for so doing, is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

The different phenomena presented by the processes of distillation in different metallic tubes, may lead to new explanations of this intricate subject, and though the facts cannot be easily accounted for, except on the supposition that nitrogene is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced, for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied that every source of error has been avoided, and that every circumstance has been examined and reasoned upon.

All conclusions on the action of potassium on ammonia, are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. HENRY, that excellent chemist, has stated that he conceives I have rather underrated the quantity of nitrogene in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

In the new trial, I used mercury which had been recently boiled in the tube for electrization; the ammonia was introduced after being long dried by caustic potash, from a receiver in which it had not been generated, and which had likewise been inverted over boiling mercury. The gas left no perceptible residuum, when absorbed by water deprived of air by boiling. In this process, 15 measures of ammonia expanded, so as to fill 27 measures; and the hydrogene by detonation with oxygene, over water freed as much as possible from air, proved to be to the nitrogene as 73.8 to 26.2. In the experiment three explosions were made, the oxygene being deficient in the first two; so that no nitrogene could have been condensed in the form of nitric acid.

Except when precautions of this kind are employed, as I have before noticed, no accurate data can be obtained respecting the proportions of permanent gases obtained from ammonia by electricity.

When the gas is generated and decomposed over the same mercury, there is always a greater expansion than the true one; and when the mercury is not boiled in the tube, and when common water is used, the nitrogene will be always over-rated, unless this error is counteracted by an opposite error, that of detonating with an excess of oxygene.*

Dr. HENRY had the kindness to send me the apparatus, in which he conceived, at that time, that he had witnessed the formation of water in the decomposition of ammonia by elec-

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* It will be seen by Dr. HENRY's letter, which immediately precedes this communication, that in repeating his processes, since this paper was written, he has gained results almost precisely the same as those indicated in the text; and there is every reason to believe, that 100 of ammonia in volume uniformly become 180, when decomposed by electricity, and that the gas produced consists in 100 parts of 74 hydrogene and 26 nitrogene.

tricity, by his ingenious method of applying hygrometrical tests.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think that the appearance of moisture even offers a decided proof of the existence of loosely combined oxygene in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hydrogene or nitrogene, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume will be capable of containing more aqueous vapour.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hydrogene and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the inquiry, attempt to discuss.

The question of ammonia being analogous to other salifiable bases in its constitution, is determined by the phænomena presented by the amalgam from that alkali; and if the conversion of nitrogene into oxygene and hydrogene should be established, it would appear that both hydrogene and nitrogene must be different combinations of ammonium with oxygene, or with water.

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* Bakerian Lecture, 1807, p. 40.

## II. *Further Inquiries respecting Sulphur and Phosphorus.*

I have stated, in the last Bakerian Lecture, that hydrogene is produced from sulphur and phosphorus in such quantities, by VOLTAIC electricity, that it cannot well be considered as an accidental ingredient in these bodies. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hydrogene in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxygene.

On the idea, that sulphur and phosphorus are deprived of some of their oxygene by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state; deoxygenated, as far as is compatible with their existence in contact with water.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus, with one fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed were collected in a filter.

The substance which separated from the sulphuret, was of a dark gray colour,* and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a non-conductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder) for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly non-conducting.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxygene they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I

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* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

intend to try tubes of porcelain, in a further investigation of the subject.

It is evident that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe that they are less oxygenated. It may, I know, be said, that it is possible that they are merely combined with more hydrogene, and that the sulphur in this state is analogous to the hydrogenated sulphur of Berthollet, and to the alcohol of sulphur of Lampadius.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the substance produced seemed to be merely in that form, in which, according to the able researches of Dr. Thomson, it is combined with water; and notwithstanding the ingenious experiments of M. A. Berthollet and M. Robiquet,[*] the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. John Davy, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of M. M. Desormes and Clement; but when the charcoal had been well burnt, there was no such residuum produced. It was found, that the same

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[*] *Annales de Chimie*, 1807, page 144, 148.

charcoal might be employed in a number of processes till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations. In all cases mixtures of * sulphuretted hydrogene gas and hydrocarbonate were evolved.

I particularly examined a specimen of the liquor which had been obtained in the last process from charcoal that had been often used. It was a non-conductor of electricity, and when the VOLTAIC spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hydrogene.

Supposing the liquor to contain hydrogene in considerable quantities, I conceived that it must be decomposed by oxymuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxygene. This circumstance, and the fact that no hydrate of sulphur or muriatic acid gas is formed by the operation of oxymuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor, contains less oxygene than in its common state. This idea has likewise occurred to

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* Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3.5. These were detonated with 5.5 of oxygene; the whole diminution, was to 6, of this residuum 2.5 appeared to be carbonic acid.

Dr. MARCET, who is engaged in some experiments on the subject, and from whose skill and accuracy, further elucidations of it may be expected.

### III. *Further Inquiries respecting carbonaceous Matter.*

On the idea which I have stated, page 73, that the diamond may consist of the carbonaceous matter combined with a little oxygene, I exposed charcoal intensely ignited, by VOLTAIC electricity,* to nitrogene, conceiving it possible that if this body was an oxide, containing oxygene very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

The charcoal, which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire to which the charcoal was attached. The gas had increased in volume one sixth; but this was owing to the evolution of carburetted inflammable gas from the charcoal, the nitrogene was unchanged in quantity, and far as my examination could go, in quality. The points of the charcoal where the heat had been intense, were rather harder than before the experiment.

I have mentioned, page 102, that charcoal, even when strongly ignited, is incapable of decomposing corrosive sublimate. When charcoal, in a state of ignition, is brought in

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* The apparatus was the same as that referred to page 59. The power employed was that of the battery of 500 belonging to the Royal Institution.

contact with oxymuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oxymuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogene. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hydrogene of the charcoal upon the oxymuriatic acid, and the combination of the gas so produced, with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oxymuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogene. My view in making this experiment, was to ascertain whether some new combination of carbonaceous matter with oxygene might not be formed in the process, and I hoped likewise to be able to free charcoal entirely from combined hydrogene, and from alkaline and earthy matter, supposing they existed in it, not fully combined with oxygene. That hydrogene must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum.*

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* Charcoal, over which sulphur has been passed, as in the experiments, page 465, as has been shewn by M. A. BERTHOLLET, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

## IV. *Further Inquiries respecting muriatic Acid.*

The experiments on muriatic acid, which I have already had the honour of laying before the Society, shew that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oxymuriatic acid are not correct. They prove that muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one-third to one-fourth of water, and that oxymuriatic acid is composed of the same substance, (free from water) united to oxygene. They likewise prove, that when bodies are oxydated in muriatic acid gas, it is by a decomposition of the water contained in that substance, and when they are oxydated in oxymuriatic acid, it is by combination with the oxygene in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid with the oxydated body.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters that have been experimented upon, except carbonic acid, and with all oxides (including water), and all inflammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the metals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in chemistry.

I have lately made several new attempts to procure uncombined dry muriatic acid; but they have been all unsuccessful.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

I have stated, page 79, that a sublimate is formed by the combustion of the olive-coloured oxide of boracium in oxymuriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hydrogene gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hydrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indications of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.