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and the incipient stage of phthisis; but in the subsequent stages the only chance is, I speak from wide experience, the practice I have here recommended.

8. Inflammation of the lungs is prevented by the hydro-azotic gas.

The lad is now before me; is fat, looks well, and has been cured a twelvemonth.

IV. *On the Action of Platina and Mercury upon each other.* By RICHARD CHENEVIX, Esq. F.R.S. M.R.I.A. &c.

Freyberg, June 3, 1804.

ON the 12th of May 1803, I had the honour of presenting a paper to the Royal Society, the object of which was to discover the nature of palladium, a substance just then announced to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of inquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

The first doubts were manifested in England; and Dr. Wollaston very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium if I had not mentioned it in this place.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be

* From the *Transactions of the Royal Society* for 1805.

made upon them, and M. Guyton was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Messrs. Vauquelin and Fourcroy then undertook the subject, and they were led by it to the confirmation of the recent discovery of M. Descotils. The existence of a new metal which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowledge as simple.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. Proust to M. Vauquelin, in which M. Proust expresses his astonishment concerning all he has read upon palladium, Messrs. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Messrs. Fourcroy and Vauquelin have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. Fourcroy has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Messrs. Rose and Gehlen* were the first among the German chemists who instituted experiments upon palladium; and M. Richter has also published a paper on the same subject.

The first attempt of Messrs. Rose and Gehlen to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their re-

* *Neues Allgemeines Journal der Chemie herausgegeben von Hermstadt, Klaproth, Richter, Scherer, Tromsdorf, und Gehlen. Ersten bandes fünftes heft.*

sult was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

Their third experiment was what they have called a repetition of that in which I had obtained palladium by passing a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following:—They dissolved 150 grains of platina with 450 of mercury, and added a solution of hydro-sulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was redissolved. The liquor was then filtered, and to that part of it which contained the redissolved precipitate an acid was added. From this process they obtained a yellow precipitate weighing 91 grains; and 50 grains of this, exposed to a strong heat, left 3-8ths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been redissolved; and the result of the experiment was complete failure.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former paper.

The sulphuretted hydrogen gas which Messrs. Rose and Gehlen presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered

delivered by M. Vauquelin at the *Ecole des Mines* in Paris, when he was professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydro-sulphurets, was redissolved by adding an excess of them.

It is moreover well known that there is a strong affinity between potash and the oxide of platina, and also that when those substances are brought together in solution, a triple salt, but little soluble, is the result. It was to avoid these difficulties that I had employed uncombined sulphuretted hydrogen gas; for the method adopted by Messrs. Rose and Gehlen appearing to me to be the application of two divellent forces, I presumed that it would produce a separation. The result of their experiment, which, it appears from their paper, they had not anticipated, shows the necessity of the precaution I had used. The operation which they performed to unite platina and mercury was, in fact, nearly the reverse of that which they supposed they had repeated from me, and might have been applied perhaps with a better prospect of success towards the decomposition of palladium.

Messrs. Rose and Gehlen seem, in many parts of their paper, to question my having fused platina; and inform us, that although they had exposed this metal in the furnace of the royal porcelain manufactory of Berlin, in which Wedgewood's pyrometer ceased to mark the degree of heat, they could not accomplish its fusion. Many of my friends in England have, however, seen the buttons which I obtained, and which were not few in number. The flux which I had used was borax. But no mention is made in any one of the operations of Messrs. Rose and Gehlen of borax having been employed.

In many of their attempts they obtained an irregular and porous mass, which of course was of a specific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of specific gravity, which I had observed, was owing to the same cause. It is true, not only that I had very often obtained such a mass, but that I had frequently also observed no diminution whatsoever in the specific gravity of the button which resulted from my operations. But all those upon which I had founded the conclusions alluded to by Messrs. Rose and Gehlen were performed in the following manner, and have been repeated since. A Hessian crucible was filled with lamp-black, and the contents pressed hard together. The
lamp-black

lamp-black was then hollowed out to the shape of the crucible as far as one-third from the bottom, leaving that much filled with the compressed materials; this lining, which adhered strongly to the sides of the crucible, was made extremely thin in order not to obstruct the passage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick mass of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fused, or varied according to other circumstances at pleasure. In general the axis of the cylindrical hole was about three or four times the diameter of the basis. After withdrawing the rod, the crucible was about half filled with borax. Upon this was placed the metal to be fused; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of course perpendicular to the axis of the cylindrical excavation at the bottom of the cover. More borax was then added to cover the piece of metal, and another quantity of lamp-black was pressed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this state it was exposed to heat in a forge, in which, upon another occasion, I had, in the presence of Messrs. Hatchett, Howard, Davy, and others, completely melted a Hessian crucible lined and prepared in the same manner. The fuel which I used was the patent coke of Messrs. Davey and Sawyer. In the present experiments I moderated the heat so as not materially to injure the crucible, and, upon taking it out of the fire, the lining was generally found so compact and so firm that it remained in a solid mass after the crucible was broken. When the metallic cylinder occupied the space at the bottom, it was natural to suppose that it had been fused; because in no other state but that of liquidity could it have run into the mould. In order, however, to prevent all objections, I had the precaution to make the hole of a different diameter from the metallic cylinder, and to observe whether the necessary change in the shape of the latter ensued. If, after such a test, repeated as often as required, I perceived that the metal did not vary in its specific gravity, I thought myself authorized to conclude that it was exempt from air.

M. Richter says that he had hoped to have put himself in possession of a considerable piece of palladium by repeating, with minute accuracy, the process which I had recommended as the best. He precipitated a mixed solution of platina and mercury by a solution of green sulphate of iron; and, after varying the subsequent operations, to which he submitted

submitted the product he had obtained by this method, he was led to the following important conclusions, amongst others of less consequence:—1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly, That mercury is capable of entering into combination with platina, so that it cannot afterwards be separated by fire. From the first of these conclusions it is evident that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

In addition to the chemists above mentioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attempts to form this combination; and M. Klaproth, in a letter to M. Vauquelin, published in the *Annales de Chimie* for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Messrs. Rose and Gehlen, as well as M. Richter, had conceived from my paper a reliance on the success of their experiments, which no words of mine had authorized, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance. M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure; and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my paper to the society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to have

have been one thousand. Amongst these, I had four successful operations. I persevered, because, even in my failures, I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Messrs. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been performed very often, these authors would not have neglected to mention it. M. Richter states his merely as preparatory to more extensive researches; and M. Tromsdorff, as well as M. Klaproth, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. Ritter, the celebrated Galvanist of Jena. M. Ritter had ascertained the rank which a great number of substances hold in a Galvanic series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next: Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led M. Ritter into a new and general train of reasoning, and induced him to undertake the examination of a great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher, and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose,
that

that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. Ritter has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail; and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it; yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago, professor Lampadius, in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain; and, after many fruitless attempts, abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest was excited by an experiment so likely to fail. Some time after this, Messrs. Clement and Desormes obtained the same result, and attempted to produce the substance a second time. They performed a vast number of experiments; but their success bore no proportion to their diligence and zeal. They published an account of their process and its consequences, but gained little credit, as no person was fortunate enough to produce the same substance. Many disbelieved the experiments altogether, and denied the existence of such a combination; whilst others, less inclined to doubt, attributed its formation to fortuitous circumstances

which might never again occur together. In February 1804, professor Lampadius, in distilling some pyritized wood, though with a different intent, obtained the same substance. As he had it now in his power to observe the phænomena that attended its formation, he discovered, and has communicated to the world, a method of producing it which never fails. Since his late paper upon the subject, as the necessary precautions can be followed by every chemist, Messrs. Clement and Desormes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation of what professor Lampadius terms his sulphur-alcohol is no longer a result of chance, or accounted for by being supposed one of those subterfuges to which human pride resorts, in order to spare itself the confession of human weakness.

The observation of any new fact becomes a matter of general concern, and truly worthy of philosophic contemplation, then only when its influence is likely to be extended beyond the single instance to which it owes its discovery. Whether water were a simple body or a compound, could have been of little importance as an insulated fact; but, connected with the vast chain of reasoning it gave rise to, it opened a new field for genius to explore. If in the present case our researches were to be confined merely to ascertaining whether palladium were a simple metal or a compound, all the advantages likely to arise from the facts observed during the inquiry would be lost; and an object of the most comprehensive interest would thus sink into a controversy concerning the existence of one more of those substances which we have dignified with the name of elements. It was in this point of view that Messrs. Richter and Ritter considered the subject as far as they went, and a few facts are stated in my first paper in support of the opinion that palladium is but a particular instance of a general truth.

By taking the reasoning on this subject, then, in its widest extent, we shall be led, I think, to the following conclusion,—that metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance, and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their

their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis, therefore, does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

[To be continued.]

V. *An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water. By HUMPHRY DAVY, Esq. F.R.S. Professor of Chemistry in the Royal Institution*.*

I. *Preliminary Observations.*

THIS fossil was found many years ago by Dr. Wavel, in a quarry near Barnstaple: Mr. Hatchett, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous schist. When first made known, it was considered as a zeolite; Mr. Hatchett, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. Babington, from its physical characters, and from some experiments on its solution in acids, made at his request by Mr. Stockler, ascertained that it was a mineral body as yet not described, and that it contained a considerable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

II. *Sensible Characters of the Fossil.*

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common centre, and inserted on the surface of the schist; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (appa-

* From the *Transactions of the Royal Society* for 1805.