V. *On the Action of Platina and Mercury upon each other.* By Richard Chenevix, *Esq. F. R. S. M. R. I. A.* &c.

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Freyberg, June 3d, 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 enquiry 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 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 Mons. 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, Mess. 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 Mess. 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.

Mess. 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 Mess. ROSE and GEHLEN to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their result 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

* *Neues Algemeines Journal der Chemie herausgegeben von Hermstadt,* KLAPROTH, RICHTER, SCHERER, TROMSDORF, und GEHLEN. *Ersten bandes funftes heft.*

current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following. They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret 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 ninety-one grains; and fifty grains of this, exposed to a strong heat, left three-eighths 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 Mess. ROSE and GEHLEN presented to those metals was combined with potash. Now, in the course of docimastic lectures annually 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 hydrosulphurets, 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 Mess. 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, shews 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.

Mess. 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 Mess. 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 Mess. 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 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 crucible. 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 Mess. 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 coak of Mess. 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 authorised 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 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 abovementioned, 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.

Mess. 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 authorised, 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 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. Mess. 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 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 Mess. 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 phenomena 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, Mess. 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 Mess. 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 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.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which does not happen when it is alone. I have tried to produce the same effect with other metals and platina, but I have not observed any thing similar. It is therefore fair to conclude, that when a solution of platina is precipitated in a metallic state

by a solution of green sulphate of iron, either silver or mercury is present.

The precipitation of a mixed solution of platina and silver requires no further caution than to free the salt of platina as much as possible from muriatic acid; for as I observed in my former Paper, the effect of nitrate of silver poured into muriate of platina, is to produce a precipitate, not of muriate of silver, but of a triple muriate of platina and silver. It was by this experiment that I then proved the affinity of these two metals; for when silver is not present, muriate of platina is among the most soluble salts. The best method of presenting the three solutions of platina, silver, and green sulphate of iron to each other, is first to pour the filtered solution of the last into the solution of platina, and then, after mixing them thoroughly together, to add the solution of silver by degrees, and to stir them constantly. In this, as in all similar operations, the presence of all acids, salts, &c. excepting those necessary for the operation, should be avoided; and if proper proportions have been used, and all circumstances attended to, the precipitation of these two metals will be very complete.

But the precipitation by a solution of mercury requires to be further considered, as the state of oxidizement of this metal, as well as the acid in which it is dissolved, produces a considerable modification in the result. In the first place the oxide, at the minimum of oxidizement, dissolved in muriatic acid, is unfit for the experiment; and even the red oxide dissolved in the same acid, or corrosive sublimate, is not the most advantageous. When a warm solution of the latter is poured into a mixed solution of platina and green sulphate of iron also

warm, as in the case of silver, these substances are brought into contact under the most favourable circumstances. Yet even thus the precipitation is slowly and imperfectly formed, often not till several hours have elapsed; and sometimes a very great deficiency of weight is observed, between the quantities used and those recovered directly by this method. If a solution of nitrate of mercury be used, the effect is produced more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former Paper.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This triple salt of platina and mercury shall be presently examined. From this it is evident that to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green sulphate of iron is superfluous.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is not sensibly acted upon by the same reagent. The insolubility of muriate of silver might be alleged as the cause of this, if I had not tried the experiment by pouring nitrate of silver into green muriate of iron, in which case all the substances were presented to each other in solution. The result was not reduction, but muriate of silver and nitrate of iron. This fact rests upon a much more extensive basis than mere mechanical circumstances; and, if pursued with the attention it deserves, it would lead us into the wide expanse of complicated affinities and their relations. From reasoning alone we should be disposed to think that an acid, so easily decomposed as the nitric, would be sufficient to prevent the reduction of a metal which it can dissolve. But on the one hand it can spend its oxygen

upon a part of the oxide of the green sulphate of iron, while on the other its affinity for oxide of silver is not powerful enough to retain it, when there is another part of the oxide of iron present to deprive it of oxygen. But the affinity of muriatic acid for oxide of silver, one of the strongest at present known, is sufficient to counterbalance all the other forces. There are many other instances of the same kind.

If then a solution of green sulphate of iron be brought into contact with either soluble or insoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be dissolved in nitric acid, and green sulphate of iron be added, the mercury is precipitated in the metallic state.

These experiments are much stronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general consideration but in their application to the present subject. They most materially modify and are indispensable to the accuracy of the results I formerly stated; but I was not aware of them at the time I first engaged in the investigation of this subject. I can also now explain a very material difference between some proportions observed by M. RICHTER and myself in an experiment which that chemist had made as a repetition of one of mine.

I had poured a solution of green sulphate of iron into a solution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate consisting of 100 of gold and 774 of mercury. M. RICHTER repeated, as he terms it, this experiment; that is, he used 100 of gold and 300 of mercury, and

obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his *method* of *repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat. If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate

of iron cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

Exper. 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat little more is left in general than the original platina; and sometimes even a diminution may be observed; for as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used. Before the precipitate has been exposed to heat it is dissolved

more easily than platina by nitro-muriatic acid; and the solution when nearly in a neutral state gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a solution of green sulphate of iron.

Exper. 2. When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count MUSSIN PUSHKIN to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this Paper.

Exper. 4. If sulphur be added to the ingredients recommended by Count MUSSIN PUSHKIN, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the

button which results from the operation, is generally more considerable.

Exper. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exper. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exper. 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exper. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes

place is independent of every substance except the metals themselves.

Exper. 9. One of the most delicate tests that I have observed in chemistry is recent muriate of tin, which detects the presence of the smallest portion of mercury. When a single drop of a saturate solution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a saturate solution of recent muriate of tin are added, the liquor becomes a little turbid, and of a smoke-gray colour. If these 500 grains of liquid be diluted with ten times their weight of water, the effect is of course diminished, but still it is perceptible. I had on a former occasion observed the action of recent muriate of tin upon a solution of platina. If a solution of recent muriate of tin be poured into a mixed solution of platina and mercury, not too concentrated, it can hardly be distinguished from a simple solution of platina. But if too much mercury be present the excess is acted upon as mercury; and the liquor assumes a darker colour than with platina alone.

From all these experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic state by green sulphate of iron. By *Experiments* 1 and 2, it is proved, 1st, That platina can protect a considerable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increase the action of nitro-muriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in such a manner as not to be separated by the degree of heat necessary to fuse the compound, since after the fusion it retains that property, which is essentially characteristic of the presence of mercury in a solution of platina. The 8th *Experiment* proves

that the action of mercury upon platina is not confined to the metallic state; but that these metals can combine and form an insoluble triple salt with an acid which produces a very soluble compound with platina alone. The 9th *Experiment* shows that platina can retain in solution a certain quantity of mercury, and prevent its reduction by a substance which acts most powerfully to that effect, when platina is not present. That part of the general position therefore which is the object of this Paper is proved, if these experiments, upon being repeated by other chemists, shall be found to be accurate.

One or two of the above experiments seem to be in contradiction to some that I have stated in my Paper upon palladium; for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but confining myself to my present object, I shall wave all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those, which produce in any substance the greatest deviation from its usual properties.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is dissolved in nitro-muriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed, than can determine the precipitation of the entire quantity of platina;

yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury, and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that

either my own observations, or those of others have given me cause to alter my opinion. I will add that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results, yet the only means which I can, after all, prescribe for succeeding, is perseverance.

To ascertain whether the opinion of Mess. FOURCROY and VAUQUELIN, that the new metal was the principal ingredient in palladium had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Mess. VAUQUELIN and FOURCROY have promised the Institute of France a continuation of their researches, and M. RICHTER concludes his paper with

saying that he will return to the subject. From the labours of such persons some great and important fact must issue, and I hope that the present subject will not be excluded from their consideration. The facts contained in this Paper cannot be submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail unless proper precautions are used: that I have never operated upon less than one hundred grains; and that the results, which I have stated, however simple they may appear, have been the constant labour of some weeks.

POSTSCRIPT.

Since this Paper was written Dr. WOLLASTON has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. WOLLASTON. I have not yet seen a copy of his Paper; but I shall merely observe here that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events the amalgamation to which platina is submitted before it reaches Europe is sufficient to account for a small portion of palladium.