

XX. *An Inquiry concerning the chemical Properties that have been attributed to Light.* By Benjamin Count of Rumford, F. R. S. M. R. I. A.

Read June 14, 1798.

IN the Second Part of my Seventh Essay, (on the propagation of heat in fluids,) I have mentioned the reasons which had induced me to doubt of the existence of those chemical properties in light that have been attributed to it, and to conclude, that all those visible changes produced in bodies by exposure to the action of the sun's rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the heat which is generated, or excited, by the light that is absorbed by them.

As the decision of this question is a matter of great importance to the advancement of science, and particularly to chemistry, and as the subject is in many respects curious and interesting, it has often employed my thoughts in my leisure hours; and I have spent much time in endeavouring to contrive experiments, from the unequivocal results of which the truth might be made to appear. Though I have not been so successful in these investigations as I could wish, yet I cannot help flattering myself, that an account of the results of some of my late experiments will be thought sufficiently interesting to merit the attention of the Royal Society.

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Having found that gold, or silver, might be melted by the heat (invisible to the sight) which exists in the air, at the distance of more than an inch above the point of the flame of a wax-candle, (see my Seventh Essay, Part II. page 350.) I was curious to know what effect this heat would produce on the oxides of those metals.

*Experiment No. 1.* Having evaporated to dryness a solution of fine gold in aqua regia, I dissolved the residuum, in just as much distilled water as was necessary in order that the solution (which was of a beautiful yellow colour) might not be disposed to crystallize; and, wetting the middle of a piece of white taffeta riband,  $1\frac{1}{2}$  inch wide, and about eight inches long, in this solution, I held the riband, with both my hands, stretched horizontally over the clear bright flame of a wax candle; the under side of the riband being kept at the distance of about  $1\frac{1}{2}$  inch above the point of the flame. The result of this experiment was very striking. That part of the riband which was directly over the point of the flame, began almost immediately to emit steam in dense clouds; and, in about 10 seconds, a circular spot, about  $\frac{3}{4}$  of an inch in diameter, having become nearly dry, a spot of a very fine purple colour, approaching to crimson, suddenly made its appearance in the middle of it, and, spreading rapidly on all sides, became, in one or two seconds more, nearly an inch in diameter.

By moving the riband, so as to bring, in their turns, all the parts of it which had been wetted with the solution to be exposed to the action of the current of hot vapour that arose from the burning candle, all those parts which had been so wetted, were tinged with the same beautiful purple colour.

This colour, which was uncommonly brilliant, passed quite

through the riband, and I found the stain to be perfectly indelible. I endeavoured to wash it out; but nothing I applied to it, and among other things I tried super-oxygenated marine acid, appeared in the smallest degree to diminish its lustre. The hue was not uniform, but varied from a light crimson to a very deep purple, approaching to a reddish brown.

I searched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the most beautiful purple enamel, which, in the sun, had a degree of brilliancy that was sometimes quite dazzling.

*Experiment No. 2.* A piece of the riband which had been wetted with the aqueous solution of the oxide, was carefully dried in a dark closet, and was then exposed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the solution, (and which on drying had acquired a faint yellow colour,) was tinged of the same bright purple colour as was produced in the last-mentioned experiment, when the riband was exposed wet to the action of the heat.\*

*Experiment No. 3.* A piece of the riband which had been wetted with the solution, and dried in the dark, was now wetted with distilled water, and exposed wet to the action of the ascending current of hot vapour which arose from the burning candle: the purple stain was produced as before, which extended as far

\* We shall hereafter find reason to conclude, that the success of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which existed in the hot current that ascended from the flame of the candle.

as the riband had been wetted with the solution, but no farther.

I afterwards varied this experiment in several ways, sometimes using paper, sometimes fine linen, and sometimes fine cotton cloths, instead of the silk riband; but nearly the same tinge was produced, whatever the substance was that was made to imbibe the aqueous solution of the metallic oxide.

Similar experiments, and with similar results, were likewise made with pieces of riband, fine linen, cotton, paper, &c. wetted in an aqueous solution of nitrate of silver; with this difference, however, that the tinge produced by this metallic oxide, instead of being of a deep purple, inclining to crimson, was of a very dark orange colour, or rather of a yellowish brown.

In order to discover whether the purple tinge, in the experiments with the oxide of gold, was occasioned by the *heat* communicated by the ascending current of hot vapour, or by the *light* of the candle, I made the following experiment, the result of which, I conceive to have been decisive.

*Experiment No. 4.* A piece of riband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame.

The riband was dried, but its colour was not in the smallest degree changed.

When it was held a few seconds within about  $\frac{1}{8}$  of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced.

The heat which existed at that distance from the flame, *on the side of it*, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flatted, such as is used in making silver lace.

The objects I had in view in the following experiments, are too evident to require any particular explanation.

*Experiment No. 5.* Two like pieces of riband were wetted at the same time in the solution, and suspended, while wet, in two thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed naked to the direct rays of a bright sun; while B was inclosed in a cylinder of paste-board, painted black within and without, and closed with a fit cover, and consequently remained in perfect darkness.

In a very few minutes, the riband in the phial A began sensibly to change its colour, and to take a purple hue; and, at the end of five hours, it had acquired a deep crimson tint throughout.

The phial B was exposed in the window, in its dark cylindrical cover, three days; but there was not the smallest appearance of any change of colour in the silk.

*Experiment No. 6.* Two small parcels of *magnesia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a shilling,) were placed in heaps, in two China plates, A and B, and thoroughly moistened with the before-mentioned aqueous solution of the oxide of gold. Both plates were placed in the same window; the moistened earth in the plate A being exposed naked to the sun's rays; while

that in the plate B was exactly covered with a tea-cup, turned upside down, which excluded all light.

The *magnesia alba* in the plate A, which was exposed to the strong light of the sun, began almost immediately to change colour, taking a faint violet hue, which by degrees became more and more intense, and in a few hours ended in a deep purple; while that in the plate B, which was kept in the dark, retained the yellowish cast it had acquired from the solution, without the smallest appearance of change.

*Experiment No. 7.* A small parcel of *magnesia alba*, placed on a china plate, having been moistened with the aqueous solution of the oxide of gold, and thoroughly dried in a dark closet, was now exposed, *in this dry state*, to the action of the direct rays of a very bright sun.

It had been exposed to this strong light above half an hour, before its colour began to be *sensibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with distilled water, it changed colour very rapidly, and soon came to be of a deep purple tint, approaching to crimson.

*Experiment No. 8.* A piece of white taffeta riband, which had been wetted with the solution, and thoroughly dried in the dark, was suspended in a clean dry phial of very fine transparent glass; and the phial, being well stopped with a dry cork, was exposed to the strong light of a bright sun.

After the riband had been exposed, in this manner, to the action of the sun's direct rays about half an hour, there were here and there some faint appearances of a change of its colour; but it showed no disposition to take that deep purple hue which

the riband had always acquired, when exposed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with distilled water, and exposing it again, *while thus wetted*, to the sun's rays, it almost instantly began to change colour, and soon became of a deep purple tint; but, though I examined the surface of the riband with the utmost care, and with a good lens, both during the experiment and after it, I could not perceive the smallest particle of *revived gold*, nor did I see any vestige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated several times, and always with results which led me to conclude, (what indeed was reasonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a state of crystallization*.

The heat which is generated by the absorption of the rays of light must necessarily, *at the moment of its generation* at least, exist in almost infinitely small spaces; and consequently, it is only in bodies that are *inconceivably small* that it can produce durable effects, in any degree indicative of its extreme intensity.

Perhaps the particles of the oxide of gold dissolved in water, are of such dimensions; and it is very remarkable, that the colours produced, in some of my experiments on white ribands, by means of an aqueous solution of the oxide of gold, are precisely the same as are produced from the oxide of that metal, by enamellers, in the intense heat of their furnaces.

As the colouring substance is the same, and as the colours produced are the same, why should we not conclude that the effects are produced in both these cases by the same means, that is to say, by the agency of heat? or, in other words, and

to be more explicit, by exposing the oxide in a certain temperature, at which it becomes disposed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the results of the following experiments afford still more satisfactory information, respecting the intensity of the heat generated in all cases where light is absorbed, and the striking effects which, under certain circumstances, it is capable of producing.

The facility with which most of the metallic oxides are reduced, in the dry way, by means of charcoal, shows that, at a certain (high) temperature, oxygen is disposed to quit those metals, in order to form a chemical union with the charcoal, or at least with some one of its constituent principles, if it be a compound substance; and hence I concluded, that gold might be revived, *in the moist way*, by means of charcoal, from a solution of its oxide in water, were it possible, under such circumstances, to communicate to the charcoal, and to the oxide, *at the same time*, a degree of heat sufficient for that purpose.

To see if this might not be done by means of light, I made, or rather repeated, the following very interesting experiment.

*Experiment No. 9.* Into a thin tube of very fine colourless glass, 10 inches long, and  $\frac{6}{10}$  of an inch in diameter, closed hermetically at its lower end, I put as many pieces of charcoal, about the size of large peas, as filled the tube to the height of two inches; and, having poured on them as much of the aqueous solution of nitro-muriate of gold as nearly covered them, exposed the tube, with its contents, to the action of the direct rays of a very bright sun.

In less than half an hour, small specks of revived gold, in all



its metallic splendour, began to make their appearance here and there on the surface of the charcoal; and, in six hours, the solution, which at first was of a bright yellow colour, became perfectly *colourless*, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The surface of the charcoal was, in several places, nearly covered with small particles of revived gold; and the inside of the glass tube, in that part where it was in contact with the upper surface of the contained liquid, was most beautifully gilded.

This gilding of the tube was very splendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greenish blue colour, without the smallest appearance of any metallic splendour.

From the colour, and apparent density of this cloud, I was induced to conclude, that the gilding on the glass was less than *one millionth part of an inch* in thickness.

This interesting experiment was repeated six times, and always with nearly the same result. The gold was completely revived in each of them, and the solution left perfectly colourless; in most of the experiments, however, the sides of the glass were not gilded, all the revived gold remaining attached to the surface of the charcoal.

In two of these experiments, I made use of pieces of charcoal which had been previously boiled several hours in a large quantity of distilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the solution, to prevent them from imbibing any air; and, in different experiments, the solution was used of different degrees of strength.

I plainly perceived that the experiment succeeded best, that is to say, that the gold was *soonest revived*, in those cases in which the solution was *most diluted*: one of the experiments, however, and which succeeded perfectly, was made with the solution so much condensed, that it was nearly at the point at which it became disposed to crystallize.\*

On examining, with a good microscope, the particles of revived gold which remained attached to the surface of the charcoal, after it had been dried, I found them to consist of an infinite number of small scales, separated from each other; not very highly polished, but possessing the true metallic splendour, and a very deep and rich gold colour.

The gold which attached itself to the inside of the glass tube, was in the form of a ring, about  $\frac{1}{10}$  of an inch wide, (badly defined however below,) and adhered to the glass with so much obstinacy, as not to be removed by rinsing out the tube a great number of times with water; it had, as has already been observed, a very high polish, when seen by reflected light.

Those who enter into the spirit of these investigations, will easily imagine how impatient I must have been, after seeing the results of these experiments, to find out whether gold could be revived from this aqueous solution of its oxide by means of charcoal, *without the assistance of light*, and merely by such a degree of equal heat as could be given to it in the dark. To determine that important question, the following experiment was made.

\* This agrees perfectly with the results of similar experiments made by the ingenious and lively Mrs. FULHAME. (See her Essay on Combustion, page 124.)

It was on reading her book, that I was induced to engage in these investigations; and it was by her experiments, that most of the foregoing experiments were suggested.

*Experiment No. 10.* A cylindrical glass tube,  $\frac{6}{10}$  of an inch in diameter, and 10 inches long, closed hermetically at its lower end, and containing a quantity of a diluted aqueous solution of the oxide of gold, mixed with charcoal in broken pieces, about the size of large peas, was put into a fit cylindrical tin case, which was nicely closed with a fit cover; and the glass tube, with its contents, so shut up in the dark, was exposed two hours, in the temperature of  $210^{\circ}$  of FAHRENHEIT'S scale.

On taking the glass tube out of its tin case, I found the solution *perfectly colourless*, and the revived gold adhering to the surface of the charcoal.

On repeating the experiment, and using the solution nearly saturated with the oxide, the result was precisely the same; the solution being found perfectly colourless, and the revived gold adhering to the surface of the charcoal.

I own fairly, that the results of these experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothesis, respecting the causes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypothesis of mine, I hope and trust that I never shall be so weak as to feel pain at the discovery of truth, however contrary it may be to my expectations; and still less, to feel a secret wish to suppress experiments, merely because their results militate against my speculative opinions.

It is proper I should observe, that the charcoal used in this last-mentioned experiment had been boiled two hours in distilled water, by which means its pores had been so completely filled with that fluid, that the pieces of it that were

used were specifically heavier than water, and sunk in it, to the bottom of the containing vessel.

Having been so successful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moist way*, I lost no time in making similar experiments with the oxide of silver.

*Experiment No. 11.* A solution of fine silver, in strong nitrous acid, was evaporated to dryness, and the residuum redissolved in distilled water.

A portion of this solution, (which was perfectly colourless,) diluted with twice as much distilled water, was poured into a phial containing a number of small pieces of charcoal; and the phial, being well closed with a new cork stopple, was exposed to the action of the sun's rays.

In less than an hour, small specks of revived silver began to make their appearance on the surface of the charcoal; and, at the end of two hours, these specks became very numerous, and had increased so much in size, that they were distinctly visible to the naked eye, at the distance of more than three feet. They were very white, and possessed the metallic splendour of silver in so high a degree, that when enlightened by the sun's beams, their lustre was nearly equal to that of very small diamonds.

The phial, which was in the form of a pear, and about  $1\frac{1}{2}$  inch in diameter at its bulb, was very thin, and made of very fine colourless glass; the aqueous solution was also perfectly transparent and colourless; and, when the contents of the phial were illuminated by the direct rays of a bright sun, the contrast of the white colour of these little metallic spangles with the black charcoal to which they were fixed, and their

extreme brilliancy, afforded a very beautiful and interesting sight.

As the air had been previously expelled from the charcoal, by boiling it in distilled water, it was specifically heavier than the aqueous solution of the metallic oxide, and consequently remained at the bottom of the bottle.

*Experiment No. 12.* A phial, as nearly as possible like that used in the last experiment, and containing the same quantity of diluted aqueous solution of nitrate of silver, and also of charcoal, was inclosed in a cylindrical tin box, and exposed one hour to the heat of boiling water, in an apparatus used for boiling potatoes in steam, for the table.

The result of this experiment was uncommonly striking. The surface of the charcoal was covered with a most beautiful metallic vegetation; small filaments of revived silver, resembling fine flattened silver wire, pushing out from its surface, in all directions!

Some of these metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were easily detached from the surface of the charcoal, to which they seemed to adhere but very slightly.

These experiments were repeated several times, and always with precisely the same results.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of small scales, adhering firmly to the surface of the charcoal. May not the difference of the forms under which gold and silver are revived from their oxides, in this process, be owing to the difference of the specific gravities of those metals?

The following experiments, which were first suggested by an accident, were made with a view to investigate still farther the causes of those effects which have been attributed to the supposed chemical properties of light.

Having accidentally put away two small phials, each containing a quantity of aqueous solution of the oxide of gold and sulphuric ether, in each of which the ether had extracted the gold completely from the solution, as was evident by the yellow colour of the solution having been transferred to the ether, and the solution being left colourless; in one of the phials, which happened to stand in a window in which there was occasionally a strong light, (though the direct rays of the sun never fell on it,) I found, in about three weeks, that the oxide was almost entirely reduced; the revived gold appearing in all its metallic splendour, in the form of a thin pellicle, swimming on the surface of the aqueous liquor in the phial, and the colour of the ether which reposed on it having become quite faint; while no visible change had been produced in the contents of the other phial, which had stood in a dark corner of the room.

As these appearances induced me to suspect, or rather strengthened the suspicions I had before conceived, that the separation of gold from ether, under its metallic form, when a solution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther investigation of that matter.

*Experiment No. 13.* Into a small pear-like phial, of very fine transparent glass, I put equal quantities of an aqueous solution of the muriatic oxide of gold and sulphuric ether; and the phial,

which was about half filled, being closed with a good cork, well secured in its place, was exposed to the action of the direct rays of a bright sun.

A pellicle of revived gold, in all its metallic splendour, began almost immediately to be formed on the surface of the aqueous liquid, and soon covered it entirely; and, at the end of two hours, the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourless*.

On shaking the phial, the metallic pellicle which covered the surface of the aqueous liquid was broken into small pieces, which had exactly the appearance of leaf gold, possessing the true colour, and all the metallic brilliancy, of that metal.

On suffering the phial to stand quiet, the aqueous liquor and the ether separated, and most of the broken pieces of the thin sheet of gold descended to the bottom of the phial: the remainder of them floated on the surface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly transparent and *colourless*.

By the length of time which was required for the ether and the aqueous liquid to separate, I thought I could perceive that the ether had lost something of its fluidity; but, as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I saw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been suffered to cool, there was no appearance of any considerable quantity of air, or other permanently elastic fluid, having been either generated or absorbed, during the experiment.

Finding that the oxide of gold might be so completely and

so expeditiously reduced, by means of ether, I conceived it might be possible to perform that chemical process, *in the moist way*, by means of essential oils; and this conjecture proved to be well founded.

*Experiment No. 14.* Upon a quantity of a diluted aqueous solution of nitro-muriate of gold, in a small pear-like phial, about  $1\frac{1}{2}$  inch in diameter at its bulb, was poured a small quantity of ethereal oil of turpentine, just as much as was sufficient to cover the aqueous solution to the height of  $\frac{2}{10}$  of an inch; and the phial, being well closed with a good cork, well secured, was exposed one hour to the heat of boiling water in a steam-vessel.

The gold was revived, appearing in the form of a splendid pellicle, of a bright gold colour, which floated on the surface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourless as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it reposed, had entirely lost its yellow colour.

On shaking the phial, its contents were intimately mixed; but, on suffering it to stand quiet, the oil of turpentine soon separated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourless.

On shaking the phial, *before it had been exposed to the heat*, and mixing its contents, and then suffering it to stand quiet, the oil of turpentine, on taking its place at the top of the aqueous solution, was not found to have acquired any colour; nor was the bright gold colour of the solution found to be at all impaired. When sulphuric ether was used, instead of the oil of turpentine, the effect was in this respect very different.

To find out whether the oil of turpentine used in this expe-



periment, and which had acquired a deep yellow colour, had lost that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous solution of the oxide into the phial, and, shaking the phial, exposed it, with its contents, to the heat of boiling water.

After it had been exposed to this heat about two hours, I examined it, and found, that though a considerable quantity of gold had been revived, yet the aqueous liquid still retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial, I now added about half as much distilled water, and, mixing the whole by shaking, I exposed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourless*.

On repeating this experiment with oil of turpentine, and varying it, by using a solution of the oxide of *silver*, (an aqueous solution of nitrate of silver,) instead of that of *gold*, the result was nearly the same: the metal was revived, and the oil of turpentine acquired a faint greenish-yellow colour.

I also revived the oxides of gold and of silver with *oil of olives*, by a similar process, with the heat of boiling water. The oil of olives used in these experiments lost its transparency, and became deeply coloured; that used in the reduction of the oxide of silver, taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold, being changed to a yellowish-brown, with a purple hue.

In the experiment with the oxide of silver, the inside of the phial, in the region where the oil reposed on the aqueous solu-

tion, was beautifully silvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, small detached pellicles of revived metal were visible in the oil, and adhered in several places to the inside of the phial, forming bright spots, in which the colour of the metal, and its peculiar splendour, were perfectly conspicuous.

*Experiment No. 15.* As carbon is one of the constituent principles of spirit of wine, as well as of essential oils and sulphuric ether, I thought it possible that I might succeed in the reduction of the oxide of gold, by mixing alcohol, with an aqueous solution of nitro-muriate of gold, and exposing the mixture, in a phial well closed, to the heat of boiling water; but the experiment did not succeed.

By pouring upon this mixture a small quantity of oil of olives, and exposing it again to the heat of boiling water, the gold was revived.

Is it not probable, that the reason why the oxide was not reduced by alcohol, is the mobility of those elements, which ought to act on each other, in order that the effect in question may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to rest on the surface of the aqueous solution, without mixing with it.

I wished to have been able to have collected and examined the elastic fluids, which probably were formed in most of the preceding experiments; but my time was so much taken up with other matters, that I had not leisure to pursue these investigations farther.

In order to see what effects would be produced by the heat generated at the surface of an opaque body, of a nature different from those hitherto used in the reduction of the metallic oxides,

and one that is little disposed to form a chemical union with oxygen, (*magnesia alba*,) when, being immersed in an aqueous solution of the oxide of gold, the rays of the sun were made to impinge on it, I contrived the following experiment.

*Experiment No. 16.* I took four small thin phials, A, B, C, and D, of very fine glass, and, putting into each of them about five grains of dry *magnesia alba*, I filled the phial A, nearly full, with a saturated aqueous solution of the oxide of gold.

I filled the phial B, in like manner, with some of the same solution, diluted with an equal quantity of distilled water; and the phials C and D were filled with the solution still farther diluted.

These phials, open or without stoppers, were exposed one whole day to the action of the direct rays of a bright sun, their contents being often well mixed together, during that time, by shaking.

The contents of all these phials changed colour, more or less, but they acquired very different hues. The contents of the phial A became of a very deep rich *gold colour*, approaching to *orange*, the earthy sediment being throughout of the same tint.

The contents of the phial B, which were at first of a light straw colour, first changed to a light green, and then to a greenish blue. The phial having been suffered to stand quiet several days, in an uninhabited room, in a retired part of the house, the solution became nearly colourless, and the sediment was found to be of a dirty olive colour.

The colour of the contents of the phials C and D was changed nearly in the same manner; and, having been suffered to stand quiet two or three days, to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There

was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint, that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business, that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.