VI. "On the Artificial Formation of the Diamond." By J. B. Hannay, F.R.S.E., F.C.S. Communicated by Professor G. G. Stokes, LL.D., D.C.L., Sec. R.S. Received April 15, 1880.

In a preliminary notice, which the Royal Society has done me the honour of publishing in the "Proceedings," I gave a very short sketch of the work I have done which led me to a reaction whereby hard crystalline carbon has been produced. I have now the honour of laying a detailed account of the methods and results before the Society. As far back as September, 1879, I was searching for a solvent for the alkali metals, and tried experiments with many liquids and gases, but invariably found that when the solvent reached the permanently gaseous state chemical action ensued. This was the case even with hydrocarbons, the metal combining with the hydrogen and setting free the carbon. Paraffin spirit, boiling at 75°, was first used in experimenting, and the spirit contained a considerable amount of olefines; but even these unsaturated hydrocarbons seemed to be split up in like manner. The experiments were conducted in thick tubes from 1 to 1·5 millims. internal, and 10 to 15 external, diameter, and made of hard glass. In some cases when the carbon was set free, a considerable proportion of the hydrogen seemed to have combined with the higher olefines and paraffins, rendering them gaseous, most of the gas, however, being formed by the reaction discovered by Thorpe and Young, where a high paraffin splits up into lower olefines and paraffins. This reaction, I would remark in passing, is not always the only one to take place, as a mixture of lower paraffins and olefines on being heated under very high pressure sometimes yield a small quantity of a higher or even nearly solid paraffin, as well as gaseous products. An attempt was made to obtain solid paraffin in quantity by this method, but only traces were obtained white enough for use.

The alkali metal which decomposes the hydrocarbon retains a quantity of pure hydrogen, which may be seen by exhausting it by the Sprengel pump. A piece of sodium was exhausted in the molten state for five hours by the Sprengel pump, and when no more hydrogen had been evolved for an hour, a piece was placed in a tube with paraffin spirit and heated for two hours, and when a considerable quantity of carbon was deposited, as much of it was removed as could be conveniently obtained and again exhausted, when 32 times its volume of hydrogen was extracted from it. This was repeated several times, and quantities of hydrogen, varying from 17 to 25 times the volume of the sodium, obtained. The carbon deposited on the tube is of a hard scaly nature, and when the sodium is slowly oxidized and

dissolved in water, some very hard scales of carbon are often obtained. This was then the reaction on which my work was built. As potassium is a metal of stronger affinities, I thought that an examination of its action on paraffin would yield somewhat better results, but in this I was disappointed. Sometimes its action was very great, but it seemed to combine with some of the substance in the tube, and formed black compounds, having no hard carbon amongst them. Some of the experiments did yield a little, but on the whole it was not so good as sodium. Lithium was next tried and yielded results which were much more hopeful. When the same paraffin spirit was used the lithium floated on it, but by melting the metal in the bottom of the tube it adhered to the sides, and did not float to the top unless any portion became detached. The appearance shown by lithium is sometimes very beautiful, a small globule of the metal when it has liquefied showing, before it begins to act energetically upon the hydrocarbon, a very beautiful play of colours. After this has gone on for a little a more rapid reaction sets in as the temperature rises, and carbon is deposited somewhat more plentifully. Sometimes a piece of the metal will float until it becomes coated with carbon and it then sinks. The carbon so obtained is harder than when sodium is employed, and will often scratch glass easily. I thought that if I could by increased temperature dissolve the nascent carbon in the metal I might obtain diamond; but after very many trials I did not succeed in doing so. Many of my scientific friends who had witnessed my experiments thought that a gaseous solvent for carbon might be found, and as this was the direction of my general work, I abandoned these metallic experiments and went into a more direct examination of the question of gaseous solution. As by far the greater number of substances with which we are acquainted are found by the intervention of water and are only soluble in water, I thought that an examination of the solvent effects of water under different circumstances would be likely to yield important results. I found that hard glass tubes resisted the action of water after it had passed its critical point for a time sufficiently long to enable an observer to see whether any great change had taken place in the solution contained in the tube. By these means it was found that many chlorides, sulphates, and nitrates were as soluble in water-gas as in water, if not more so, and that at least no sudden change of solubility was observable at the critical point. By such a process, however, only qualitative results could be obtained, and a series of experiments was tried, using various volumes of water with a fixed proportion of silica in iron tubes, and the solvent power of the water estimated in a more accurate manner. An account of this work will, I hope, be laid before the Society shortly; in fact, as soon as I have the necessary time for completing the work and writing it up. The general result obtained from these experiments was that the solvent

power of water was found to be determined by two conditions: 1st. Temperature or molecular *vis viva;* and 2nd. Closeness of the molecules on pressure, which seems to give penetrative power. From these observations it will be seen that if a body has any solvent action on another and does not act upon it chemically, such solvent action may be indefinitely increased by indefinitely increasing the temperature and pressure of the solvent. In nature, the temperature has been at one time higher than we can obtain artificially, and the pressure obtained by a depth of 200 miles from the surface is greater than can be supported by any of the materials from which we can form vessels. It will thus be seen that, whereas in nature almost unlimited solvent power could be obtained, we are not as yet able to reproduce these conditions artificially. Could pressure alone increase solvent power then much might be done, but pressure only acts by keeping the molecules close together when they have great *vis viva*, and this latter is only obtained by high temperature.

As glass tubes were quite out of the question when a red heat and very high pressure were required, iron tubes were resorted to, and a series of attempts made to dissolve carbon by various gaseous solvents. The difficulty of closing iron tubes as compared with glass tubes caused me to try various methods, which I shall describe here. Tubes were made of strong hydraulic tubing, 20″ long, 1″ thick, and ½″ bore. These were fitted with a plug, screwed with a strong screw fitting very well. There was placed in the tube some powdered charcoal from which all the inorganic matter had been removed by immersion in hydrochloric and hydrofluoric acids and washing with water, and then sufficient paraffin spirit to fill the tube two-thirds of its volume. The plug was screwed in with a lute composed of silicate of soda and manganese dioxide, but after heating the tube in a reverberatory furnace for four hours it was found to be impossible to remove the plug, so the end had to be bored out. There was neither liquid nor gas in the tube, the luting having leaked. Another tube similarly filled was fitted with a plug with a copper washer, the end of the tube, plug, and washer being polished, but this also leaked and no result was arrived at. Baryta, clay, asbestos, and other substances, wet with silicate of soda, were all tried with the same result—leakage. A silver washer kept comparatively tight, but only on one occasion. It was thus seen that screw-closing would give no reliable results, so another method was tried. A ball of iron, fitting the tube tightly, was placed in it after the materials had been introduced. The end of the tube was then narrowed by compression between rollers and turned smooth inside. The iron ball was then drawn up by a wire attached and luted by silicate of soda and fine manganese dioxide. It was expected that the pressure would only serve to make the closing more secure, but, on heating, the iron yielded and the ball was driven out with a loud

explosion. After trying several other methods of closing—outside screwing and filling the mouth with molten metal on the top of a clay plug being amongst them—I came to the conclusion that nothing would suffice but welding up the open end. This has been, when carried out efficiently, invariably successful, and in all my later experiments I have used it alone. It requires great skill on the part of the workman, and it is only one man in a hundred who can perform the operation with invariable success. The furnace used in these experiments was a reverberatory one, 6 feet long (internal measurement) and 2 feet broad; fire-place, 15 inches; bridge, 9 inches; hearth, 4 feet. The roof sloped down towards the flue and the spent gases had exit at the level of the hearth, thus carrying the flame down as it receded from the fire, in order to have the hearth of one temperature. The walls were 13 inches thick, and the roof formed of 4-inch fire-clay covers.

Three tubes, $20'' \times 1'' \times \frac{1}{2}''$ bore, were filled as follows:—

No. I. 3 grms. sodium, $\frac{3}{4}$ full paraffin spirit.
" II. " " $\frac{2}{3}$ " "
" III. " " $\frac{3}{5}$ " "

On heating them in the reverberatory furnace, No. I exploded before a visible red-heat had been obtained, so the temperature was not allowed to rise any higher, and Nos. II and III allowed to lie for four hours and then slowly cooled. On being bored open next day, No. II contained a little scaly carbon, but No. III contained almost none, and nearly all its liquid had been converted into gas, which rushed out on boring it open. It was noticed by the workmen that the inside of the tube was harder to bore than the outside, and I thought, as I found out afterwards rightly, that the iron had been carbonised and converted into steel. It seemed, then, that the free carbon had been taken up by the iron. The same two tubes were welded up again, rather more than half-full of liquid, and slowly heated, but before they came to as high a temperature as they had been subjected to formerly, they exploded together. Two more were filled, welded, and heated, but again they burst simultaneously. It appeared, then, that those tubes were too weak, so two were made $20'' \times 2'' \times \frac{1}{2}''$ bore. On trying to weld them when two-thirds full, the liquid got hot, and gave off enough vapour to carbonise the white-hot plug and made a bad weld, so that they had to be kept cool by trickling water upon them immediately below the hot part. Several tubes were lost in closing before the workman became deft enough at closing them. Two were at length obtained well closed, and were heated, but again, before a red-heat was reached, they exploded simultaneously, smashing the roof of the furnace. It seemed on examining the tubes that one had gone off first, and struck the other such a blow as caused it to burst, as one had a mark near the

middle as from a blow with a hammer, and was bent a little. It became plain, then, that two should not be heated together, at least where they might strike each other on explosion. Then, again, as the iron took up the carbon set free, I considered that the reaction might be favoured by adding some carbon to the liquid in the form of lamp-black, so that the liquid would be kept always saturated. A tube, $20'' \times 2'' \times \frac{1}{2}''$ bore was filled, as before, to about three-fourths of its volume, and about half a gram of lamp-black added. This was heated to just below a red-heat for six hours and allowed to cool slowly. On being cut open there was a considerable yield of scaly carbon, and the sodium, on being dissolved, left a few pretty hard scales, along with ferric oxide, spongy iron, &c. This was encouraging, and another tube was filled in the same way, but it burst on heating. An experiment was then tried with paraffin spirit and lamp-black alone, only about 2 grms. of lamp-black being added to the tube, three parts filled with hydrocarbon. This experiment went successfully, and on opening the tube after the outrush of gas it was found that nearly all the lamp-black had been absorbed by the iron. This showed that my conjecture was right about the disappearance of the carbon. Two divisions were then built upon the furnace-hearth, so as to divide it into three spaces, and three tubes of the above dimensions, and filled as follows, were put in them:—

No. I. 3 grms. naphtha, $\frac{1}{2}$ grm. lamp-black, $\frac{1}{2}$ full paraffin spirit.
,, II. ,, ,, ,, ,, $\frac{2}{3}$,, ,,
,, III. ,, ,, ,, ,, $\frac{3}{4}$,, ,,

On heating, No. II burst with a loud explosion, but did not harm Nos. I and III, but on opening these next day they were found to have leaked, so that there may have been no pressure inside them at the time. Other two, two-thirds filled with liquid and solids as above, also burst; but as I was absent I do not know at what temperature they were.

It seemed plain that the tubes were much too frail, and although they were made from "Lowmoor" iron, well hammered, and the tube bored out of the solid, they invariably burst lengthwise, showing a reedy structure. I determined then to try tubes on the coil principle; so two were constructed out of the toughest bar iron, made solid, and bored out afterwards. The dimensions were $20'' \times 2\frac{3}{4}'' \times \frac{1}{2}''$, and they were heated to a just visible red-heat, and contained 3 grms. sodium, $\frac{1}{2}$ grm. lamp-black, and two-thirds full of paraffin spirit. The heat was kept up eight hours, and the tubes allowed to cool in the furnace. Both kept tight, and yielded some hard scaly carbon, but nothing else. One was tried with lithium, and a better yield of carbon obtained, and it was also harder. Two more tubes of the same dimensions, with lithium, burst, and so were lost. It now became evident that much

stronger tubes were necessary for this reaction, or that some other reaction would require to be found.

I would here mention a fact which, although not directly connected with our subject, is of great interest to students of chemical physics. When introducing the alkali metal into the tube it was often necessary to push it down with a rod, and in one or two cases when I had mislaid the iron rod for the purpose I used a glass rod, of which the end had not been rounded by fusion. In consequence, some small particles of glass became detached, and being lead glass they were heated to such a temperature that they were softened, and in some cases completely melted by the heat. Whenever this had taken place the piece of glass had cavities in it, and these cavities were partially filled with liquid or compressed gas, generally with a portion of each. In the same piece of glass the bubbles were of different size, and often filled to different heights with liquid, just as we find liquid carbonic acid and water filling in different proportions the cavities in the same quartz crystal. How these cavities in the glass are formed, and how the liquid gets into them, I cannot at present determine, but even little spheres of glass purposely introduced likewise developed cavities containing liquid on being fused under pressure. As this is a part of the subject I am investigating, the results being easily produced, I shall reserve its full discussion until I have examined it from various points of view, and varying the substances employed. I think it right to mention it here, however, as I have so little leisure and so many interesting discoveries just touched upon, and each one more tempting as a field of labour than the other, that it may be some time before I can have full data on the subject.

The iron used in making the tubes is what is known as "Lowmoor" iron, a very pure and strong quality, and a portion removed from the interior of a tube which has been used gave, on analysis, 2·17 per cent. of carbon, showing to what an extent carbonisation had gone on.

Having obtained results from this process of a kind which showed that diamond was unlikely to be formed by its agency, I reverted to the original idea of solution of carbon in a gaseous menstruum, and from some experiments I had been carrying on with the view of finding some commercial use for "bone oil," I concluded that the distillate from bone oil containing the nitrogenous bases would be most likely to yield such a solvent. Bone oil, the nitrogenous distillate obtained in the manufacture of bone char, and for a plentiful supply of which I am indebted to Messrs. John Poynter and Sons, of Glasgow, was distilled, and the portion boiling between 115° and 150° was taken and rectified over solid caustic potash, and latterly over sodium. When satisfied that it was free from moisture, oxygen, and sulphur, a tube, $2\frac{3}{4}'' \times 20'' \times \frac{1}{2}''$ bore, was three parts filled, and some charcoal powder added, and the whole welded up solid. I found that the nitrogenous

liquid was even worse to work with than the hydrocarbon, as on coming into contact with the hot iron it burnt it away at once, and as the tube was of great diameter it was extremely difficult to keep the lower part cool. For welding it had to be arranged so that it was standing in a tub of ice, and the top projecting through the bottom of the forge, and heated until it was at a welding heat, with as little delay as possible. When a tube was obtained welded up solid it was heated to a dull red-heat for 14 hours, and allowed to cool; on opening the tube there was a very great out-rush of gas, and the carbon was to a certain extent dissolved, and some minute portions of it very hard. Still, under the microscope it presented little difference in appearance from the wood charcoal employed, some of the features, however, being obliterated, and it had a bright appearance. Another tube of the same dimensions and contents was closed up in the same manner, but after eight hours' heating it burst with a loud explosion. I had noticed that a tube which had been once used and been partially carbonised would not stand a second heating, and for this reason I had no belief in the power of cast-iron or steel to withstand the great pressure at a red-heat. Nevertheless, as many of my friends had urged upon me to try these materials, I had a cast-iron tube made, $3\frac{3}{4}'' \times 24'' \times \frac{3}{4}''$ bore, and filled two-thirds of its volume with bone oil distillate and carbon, and then welded up. We succeeded after a little trouble in making a good weld, and the tube was then slowly raised to a dull red-heat in the furnace. It had not been heated for more than an hour when it exploded with a great noise, and knocked down the back and one of the ends of the furnace, leaving the whole structure a wreck. The tube had broken into small fragments, and was quite unlike the malleable iron tubes which generally tore up. Thinking that it was perhaps a bad casting, I tried another, but it leaked all over, and emptied itself before the temperature was nearly up. A third tube of the same material burst like the first, but as I had built up the furnace with large blast-furnace blocks, it was not blown down. Cast-iron being inadmissible, experiments were then made with steel. I had several tubes made of this material by the best firms in the kingdom—made by the three methods, Bessemer, Siemens, and the crucible method—but they had the same faults as cast-iron, although to a less degree. The difficulty in making a good weld in cast-iron and steel tubes makes their employment in such experiments as these a matter of inconvenience. Out of five tubes made of steel, some of which were made of the very toughest material manufactured by Messrs. Cammell and Co., only one held in the substance completely. Three burst in the furnace, and one had leaked by its porosity. The top of the furnace, by the continued shocks of explosions, fell in at the bursting of the last of the steel tubes. The continued strain on the nerves, watching the temperature of the furnace, and in a state of tension in case of an

explosion, induces a nervous state which is extremely weakening, and when the explosion occurs it sometimes shakes one so severely that sickness supervenes.

It appeared that as the bone oil had so hardened the carbon, if it acted upon nascent carbon it might harden it so much as to produce diamond. An experiment was accordingly tried in which bone oil distillate and paraffin spirit were mixed, so that when an alkali metal was made to act upon it the decomposition of the hydrocarbon might yield carbon which would be crystallized by the action of the nitrogenous liquid. The proportions used were 90 per cent. bone oil, and 10 per cent. paraffin spirit, with lithium as the metal. The tube used was a coil-tube of Lowmoor iron, 4 inches in external, $\frac{3}{4}$-inch internal diameter, and 24 inches long. Three grms. of lithium was employed, and the tube filled three parts of its length with the mixed liquids, a little lamp-black added, and welded up. It was heated 14 hours to a dull red-heat, and then bored open. A very high pressure was found to exist inside the tube, and as the material was at the other end of the tube it was removed by a long punch. It was found to be a mixture of carbon and lithium compounds, with some cyanides. Some of the carbon was very hard, but could be crushed by agate, and would not scratch it; but there was mixed with it (as with many of the other experiments) a few grains of silica. This was, perhaps, introduced in the welding, as a little silica had to be employed here, and a little may have been added with the lamp-black, which was never quite free from sand or other accidental impurity. The silica particles were easily removed by placing the substance on the stage of the microscope, and examining by polarized light, when all the particles which showed rotary power were removed.

The results obtained by the use of the two liquids being so much more satisfactory, further experiments were undertaken in this direction, and a series of four tubes filled with varying proportions. The tubes had the dimensions $20'' \times 3\frac{3}{4}'' \times \frac{1}{2}''$ bore, and had 3 grms. of lithium introduced into each and then filled as follows:—

No. I.	80 per cent. bone oil,		20 per cent. paraffin spirit			5 grms. of lamp-black added to each.
„ II.	40 „	„	60 „	„		
„ III.	20 „	„	80 „	„		
„ IV.	10 „	„	90 „	„		

These were heated separately in the furnace, with the result that Nos. I and III burst, and Nos. II and IV withstood the pressure. No. II on being bored open gave off a great volume of gas, but on removing the contents nothing but lithium compounds and soft carbon was obtained. No. IV also evolved much gas, but the solid matter was very hard and contained some hard particles of carbon, but no diamond. It seemed therefore probable that some such proportion of

ingredients might yield successful results, and another series of five, ranging from 30 per cent. to 10 per cent. of bone oil distillate, was prepared, but not one of them gave any results. One by one the tubes exploded, and the furnace had to be reconstructed at the fourth experiment. I thought I should either have to abandon the attempt or begin experiments of a very expensive nature, using large tubes and a large furnace, as 20-inch tubes of a greater diameter than 4 inches could not be closed when three parts filled—at least by welding. As some of them, however, seemed to stand, I determined to make some further trials with the apparatus I had at my disposal; so another tube, $20'' \times 4'' \times \frac{1}{2}''$ bore was filled, using 4 grms. of lithium and a mixture of bone oil, carefully rectified, 90 per cent., and paraffin spirit 10 per cent., using these proportions because I had never had any results with a high percentage of bone oil, the tubes so filled having burst. The tube was closed with great difficulty, being three-parts full of liquid, and then heated to a visible red-heat for fourteen hours, and allowed to cool slowly. On opening the tube a great volume of gas was given off, and only a little liquid remained. In the end of the tube which had been the upper end in the furnace, the tube lying obliquely, there was a hard smooth mass adhering to the sides of the tube, and entirely covering the bottom. As I had never obtained all the solids in one piece before, I wished to examine it, and so had the other end of the tube cut off, exposing the hard mass. It was quite black, and was removed with a chisel, and as it appeared to be composed principally of iron and lithium it was laid aside for analysis. I was pulverising it in a mortar, when I felt that some parts of the material were extremely hard—not resisting a blow, but hard otherwise. On looking closer, I saw that these were mostly transparent pieces imbedded in the hard matrix, and on triturating them I obtained some free from the black matter. They turned out to be crystalline carbon, exactly like diamond. I shall describe further on the analyses, &c., but will here go on with the account of my further experiments. Two tubes were filled in the same manner as the last, but one burst on heating, and the other had leaked so that there was no reaction. Two more tubes were prepared, but were spoiled on welding, and on cutting off the carbonised portion the remainder was too short to work. After much trouble three tubes were obtained, well closed, in which the three alkali metals were inclosed with liquid containing 20 per cent. bone oil and 80 per cent. paraffin. All three stood, and, on opening, only the potassium one had leaked to any extent. The results were not good, however, the sodium tube containing only soft scaly carbon, and the other two very little better. The reaction did not seem to have proceeded in the same manner in the lithium tube as before, as the mass was soft and friable. Still, lithium seemed to yield the best results, so it was adhered to in the

further experiments. A list of disasters now awaited me. Eight tubes failed through bursting and leaking, and one of the explosions, when two were being heated together, destroyed a part of the furnace and injured one of my workmen. Besides this, two tubes were spoiled in welding. However, I had four experiments after this, all withstanding the pressure, and in one of these, with 10 per cent. bone oil and 90 per cent. paraffin spirit, a small quantity of diamond was found. The contents of this tube were different from the other successful one, being much looser and not in the same hard mass as the first. In another series of six experiments two were at first thought to have been successful, but I afterwards found that one of them was not so, the transparent matter being siliceous, but insoluble in cold hydrofluoric acid, although it dissolved on boiling. The uncertainty and great expense involved in using these forged coils of iron with tubes bored out of the solid induced me to again try steel, and Messrs. Cammell and Co. having prepared some tubes for me, I tried them, but with the same results—they exploded into fragments at a red-heat. And herein they are much more dangerous than coiled tubes, because the latter seldom fly into fragments, but just tear open a little. A further unforeseen danger in using steel tubes was discovered. One which had stood the heating very well was being bored, and when the inner skin was cut so that the gas rushed out, the whole exploded, endangering the life of the workman who was boring, but as he was standing at the end of the tube and the pieces flew laterally he was not hurt. I have performed over eighty experiments, and have only obtained three results of a successful nature. The identification of the crystalline pieces as carbon was easy enough, but I have been anxious to find whether they are pure carbon or a compound with some other element, and to that end the following experiments were conducted.

A portion of the substance from the first successful experiment was weighed out after it had been freed from all foreign matter adhering to it, and placed in a very small platinum boat made of a strip of thin foil, the ends of which were wrapped round two stout platinum wires which were sealed into a wide glass tube. The carbon particles were transferred to this boat after being weighed, and the tube connected by india-rubber stoppers with an oxygen gasometer on the one side and a series of potash bulbs on the other. The oxygen was dried over solid caustic potash before entering the tube, and again after leaving the potash bulbs. The carbon (14 mgrms.) having been weighed out, the potash bulbs were weighed, and a current of oxygen passed through the apparatus, and the platinum wires connected with a battery strong enough to heat the foil to a bright red-heat. After a few minutes the oxygen was stopped and the bulbs weighed, when it was found that they had gained 1 mgrm. On repeating this operation no gain was found, the moisture having been entirely driven off by the

first treatment. The carbon was now placed in the boat and a slow current of oxygen started, then the bulbs connected and the current made to pass through the platinum until all the diamond had been burnt, when the current was stopped and the oxygen allowed to pass for fifteen minutes more, when the bulbs were detached and weighed. They were then reconnected and the gas passed for other ten minutes to find whether all the carbonic acid had been expelled and reweighed. They weighed 0·2 mgrm. less than before. The numbers were as follow:—

Potash bulbs before combustion.....	43·8308	
„ „ after „	43·8776	
		·0468
Drying tube before combustion.....	26·4294	
„ „ after „	26·4328	
		·0034
		·0502

This gives a composition of 97·85 per cent. of carbon, which is a pretty fair approximation to pure carbon. However, to determine whether or not this was not the case, some further experiments were tried. A small quantity of the carbon was placed on the platinum boat and burnt in oxygen without any of the gas being allowed to pass out of the apparatus, and the mixed gases so obtained transferred to a eudiometer, and the carbonic acid and oxygen absorbed. It was then found that a residue amounting to about 3 per cent. by volume of the carbonic acid was left unabsorbed by alkaline pyrogallate solution. This proved to be nitrogen. A blank experiment was done, but it gave only a minute bubble of nitrogen. Another experiment was performed with the following results:—

Total volume.....	183·7°		
After absorption of CO_2	148·5°	CO_2=	35·2
After „ „ O	1·1°	O	147·4
		N	1·1

This plainly shows that nitrogen was present from some cause or another, and as every precaution was taken in transferring the gas from one vessel to another, and as the blank experiment showed nothing, I am inclined to believe that the carbon, or at least some portions of it, contained nitrogen chemically combined. The numbers above given are degrees on the eudiometer tube, and are not more than one-third of a cubic centimetre each. Their exact value was of no consequence in the experiment, and the tube was only calibrated by comparing one part with another, and not with an absolute measure.

From the fact that no diamond was found when nitrogen compounds

were absent, and from the fact that the mixed product (for only a portion of the 14 mgrms. was clear diamond) contains nitrogen, I am inclined to believe that it is by the decomposition of a nitrogenous body, and not the hydrocarbon, that the diamond is formed in this reaction. The experiments are, however, too few, and the evidence too vague, to draw any conclusions, as there are even very few negative experiments from which anything can be learned, most of the results being lost by explosion. I intend, when my other work—which I laid aside for the diamond experiments—is finished, to begin a series of experiments on the decompositions of carbon compounds by metals, to find whether a more easily controlled reaction may not be discovered.

VII. "Further Note on the Spectrum of Carbon." By J. Norman Lockyer, F.R.S. Received May 11, 1880.

The preliminary discussion of a considerable number of photographs of the spectra of various carbon compounds has brought to light a relationship which I think may be worthy of notice in the Proceedings; it was noticed orally in connexion with the paper read before the Society on April 29th.

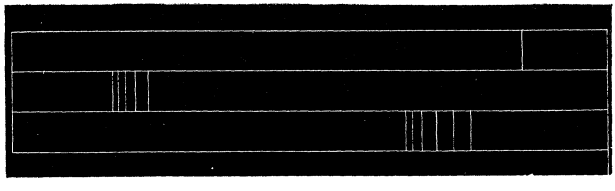
A comparison of the photographs of the various carbon compounds observed under various conditions enabled us to isolate the lines in the blue and ultra-violet portions of the spectrum (wave-lengths 4300–3800).

In this manner the constant lines seen in the photographs of the spectra of CCl_4, $C_{10}H_8$, CN, CHI_3, CS_2, CO_2, CO, &c., have been mapped, and the coincident lines and flutings thus marked.

The phenomena thus seen with more or less constancy are a blue line, with a wave-length of 4266; a set of blue flutings, extending from 4215 to 4151; and another set of ultra-violet flutings, which extend from 3885 to 3843 (all approximate numbers).

Fig. 1.

Action of three different temperatures on a hypothetical substance, assuming three stages of complete dissociation.



In a photograph of the spectrum of the electric arc (with a weak

FIG. 1.

Action of three different temperatures on a hypothetical substance, assuming three stages of complete dissociation.