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XIX. *Experiments and Obſervations on Charcoal: By* Joſeph Prieſtley, *LL. D. F. R. S.*

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AMONG the original experiments, publiſhed in the Hiſtory of Electricity, was an account of the conducting power of charcoal. This ſubſtance had been conſidered by electricians, in no other light than that of mere perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and though thoſe attempts were ineffectual, the failure of ſucceſs was attributed to other cauſes than that of charcoal being no electric ſubſtance; ſo fixed was the perſuaſion, that water and metals were the only conducting ſubſtances in nature. The conſideration of the chemical properties of charcoal, which are, in many reſpects, remarkably different from thoſe of the wood from which it is made, might have led them to ſuſpect, that ſince, after its being reduced to a coal, it was become quite another thing from what it was before, it might poſſibly differ from wood in this property; but this confideration had not been fufficiently attended to.

In the account of my former experiments on charcoal, I obferved, that there were very great differences in the conducting power of charcoal, and particularly of wood charcoal, though I could not determine on what circumftances in the preparation, &c. thofe differences depended. I thererefore expreffed a wifh, that fome perfon, who had conveniencies for making chemical experiments, would profecute the inquiry, as one that promifed, not only to afcertain the caufe of the conducting power of charcoal, but perhaps of conducting power univerfally. Not hearing that any chemift or electrician has attended to this bufinefs, I have, at length, refumed the fubject, though not with every advantage that I could have wifhed. I have, in a great meafure, however, fucceeded in the principal object of my inquiry; and I fhall now lay before this Society, the refult of my experiments and obfervations.

I fhall begin with correcting a miftake I lay under at the time that I made the former experiments. Having been informed by perfons, who attend the making of pit charcoal, that it was confiderably increafed in bulk after the procefs; I imagined that all other fubftances received an increafe of bulk, when they were reduced to a coal; but the firft experiments that I made, convinced me of my miftake. All vegetable fubftances are confiderably contracted in all their dimenfions, by the procefs of coaling, and the more perfect this procefs is, that is, as will be explained hereafter, the greater heat is applied in the

the courſe of it, the greater is the diminution. I have even reduced pieces of wood to little more than one fourth of their original length and breadth, in a common fire, by the uſe of a pair of hand bellows only. And this was the caſe equally with wood of the firmeſt texture, as ebony; that of a middle texture, as oak; and that of the looſeſt, as fir, &c.

As moiſture, and, I believe, ſmall degrees of heat or cold, affects wood much more ſenſibly acroſs the fibres than along them, it might have been ſuppoſed, that when wood was reduced to a coal, by the application of a greater degree of heat, the ſame rule would have been obſerved; but I found very little difference in this reſpect. To aſcertain this circumſtance, I took, from the ſame board, two pieces, each 2 ½ inches in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the ſame crucible, I found that the former meaſured 2.05 inches, and the latter 2.15. Their conducting power could not be diſtinguiſhed.

A more particular account of the degree, in which wood is ſhortened in coaling, will be ſeen afterwards, when the variations in this reſpect are compared with the variations in the power of conducting electricity.

To my great ſurprize, I found animal ſubſtances not reduced in their dimenſions by the proceſs of coaling. This, at leaſt, was the caſe with ſome pieces of ivory, ſeveral inches in length, and a piece of bone. They bore a very intenſe heat for many hours, and came out of the crucible conſiderably diminiſhed in weight, but hardly ſo much as diſtorted ftorted in their fhape, as is remarkably the cafe with wood, and, I believe, all vegetable fubftances.

In examining mineral fubftances, I found that my information, mentioned above, was juft. Coals are very much inlarged in their dimenfions by charring; but the experiment muft be made with great care, to judge of this circumftance; for, unlefs the operation be very flow, the coal will retain nothing of its former fhape, having been made in fome meafure fluid by the heat. The infide of all pieces of pit charcoal is full of cavitities, and there is generally a very large one in the center of every piece; fo that, the dilatation is nothing like the extenfion of fibres; but is produced by the elafticity of the new formed vapour, in forcing its way out, while the fubftance is foft.

With refpect to the main object of my inquiry, I prefently fatisfied myfelf, that the conducting power of charcoal depends upon no other circumftance than the degree of heat, that is applied in the procefs of making it. I had not fufpected this; but numberlefs experiments clearly proved it. Taking an iron pot filled with fand, and putting into it pieces of wood, cut out of the fame plank, marking them, and carefully noting their places in the pot, I always found that thofe pieces came out the beft conductors, that had been expofed to the greateft heat. The refult was the fame when I made coals of bits of wood, placed one above another, in a gun barrel, one end of which was made red hot, and the reft gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them

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the ſtrongeſt conducting power, by repeating the proceſs of coaling, either in a crucible, or a gun barrel, covered with ſand, and kept in an intenſe heat.

I could not find that the mere continuance of the ſame degree of heat had any effect with reſpect to the conducting power of charcoal.

Macquer and other chemiſts define charcoal to be wood burned, without being ſuffered to flame; but, with reſpect to it's conducting power, and, I make no doubt, with reſpect to all other eſſential properties alſo, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun barrels, and in crucibles, ſlightly covered with ſand, and have let the inflammable vapour that exhaled from them take fire, at various diſtances from the ſubſtances; and I have alſo put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all theſe caſes have found the charcoal equally good. In the laſt method, indeed, very little of the ſubſtance is preſerved; but the little that doth remain, after it has ceaſed to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But one can hardly be ſure that the ſame degree of heat is given to every part of a piece of wood, except it be expoſed to it for ſome time; and in an open fire, urged with a pair of bellows, the wood waſtes as faſt as it is red hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards leſſened. A partial conſuming of it in an open fire doth not affect the remainder, as I obſerved in the account of my former experiments.

I had imagined, that the ſolidity of ſubſtances converted into charcoal would have had a very conſiderable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lighteſt woods conducted, as far as I could perceive, as well as thoſe that were made from the moſt ſolid, if they had been expoſed to the ſame degree of heat in the proceſs. Fine ſhavings of fir, the thin coats of an onion, the lighteſt ſort, and every other vegetable ſubſtance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, alſo, that the moment a piece of wood was become black with heat, it was, to all intents and purpoſes, a real charcoal, and, along with other properties of charcoal, would conduct electricity, more or leſs: but I found, by coaling ſeveral pieces very ſlowly, that they would not conduct in the leaſt degree, not only when they were made ſuperficially black, but likewiſe when they were black quite through, and had remained a long time in the heat that made them ſo; ſo that no eye could diſtinguiſh them from the moſt perfect charcoal.

I have ſometimes found charcoal in ſuch a ſtate, that it would aſſiſt the paſſage of an exploſion along its ſurface, when it would not conduct a ſhock any other way.

In order to ſatisfy myſelf in what proportions the diminution of weight, the decreaſe of bulk, and the conducting power of wood and charcoal, correſponded to to one another. I took ſeveral pieces from the ſame plank, and having carefully weighed and meaſured them, converted them into coals very ſlowly, and by a gradual increaſe of heat, on an iron plate, held on the fire, turning them conſtantly, to prevent their catching fire. The following were the reſults.

A piece of very old dry oak, weighing 12 grains, and which conducted in the imperfect manner that wood generally does, from the moiſture it contains, was, after the loſs of about one grain, no conductor at all; and it continued the ſame as baked wood, till it was reduced to 4 grains, when it was black quite through; and even then, no part of it conducted, except one corner, where it had catched fire.

Another piece I carefully weighed and meaſured ſeveral times, in the courſe of the proceſs. At firſt it weighed 12 grains, when its dimenſions were 2 inches and .45. At 8 grains they were 2 and .4; at 5 .5 gr. 1 .91 and .4; at 3 .5 gr. 1 .8 and .35. It was now become an imperfect conductor. I then urged it with a ſtrong heat, in a crucible, and taking it out, it weighed 1 .75 gr. and meaſured 1 .6, and .3. It was now a perfect conductor; and though I afterwards kept it in a very intenſe heat ſeveral hours, by which it was reduced to 1 gr. and meaſured 1 .6, and .3, its conducting power was not ſenſibly increaſed; but it was become very brittle or friable.

It appears from theſe experiments, that theſe pieces of wood were reduced to about $\frac{1}{4}$ of their weight before they would conduct at all; though, at the ſame time, they were diminiſhed in length (*i. e.* along the fibres) only $\frac{1}{10}$. The breadth and thickneſs could

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not be meaſured with ſufficient accuracy in theſe ſmall pieces. To make them perfect conductors, they were reduced to about $\frac{1}{10}$ in weight, and $\frac{1}{2}$ in length.

A variety of circumſtances led me to conclude that the cauſe of blackneſs, and of the conducting power in charcoal, is the oil of the plant, made empyreumatic, and burnt to a certain degree. I therefore conclude that theſe properties are ſome way connected with that part of the inflammable principle, otherwiſe called phlogiſton, that is fixed and united to the earth of the plant, when the union is ſtrengthened by an intenſe heat.

The ſand, with which I covered the ſubſtances that I converted into coals, and alſo the pipe clay which I ſometimes put over them, contracted a blackneſs like charcoal, and would often conduct pretty well. Sometimes they would conduct a ſhock. This muſt have been owing to the oil they received from the ſubſtances out of which it was expelled by the heat. In the experiment of the gun barrel fitted with pieces of wood, mentioned above, the uppermoſt pieces were not in the leaſt burnt. They could hardly have been hot; yet, having contracted a ſuperficial blackneſs, from the vapour of the oil expelled from the piece below them, they would even conduct a ſhock, though not in the moſt perfect manner.

Sometimes thoſe ſubſtances that had no phlogiſton themſelves, but received it in conſequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately; but would be made to do ſo by being expoſed

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to a greater heat, which more thoroughly burnt the oil with which their pores were filled.

I put a piece of common pipe into a crucible, in which I was burning ſome turpentine (which will be mentioned below); and it came out black quite through, like a pipe in which tobacco has been frequently ſmoaked. In this ſtate it would not conduct at all; but, putting it into a crucible, covered with ſand, I treated it in the ſame manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiſton would have eſcaped, and the pipe would have been left white as at firſt.

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiſton contained in the oil, and on the degree of heat with which it was burned, I took ſeveral methods to give vegetable ſubſtances more of this principle; or at leaſt endeavoured to make them retain more of it than they uſually do, in the proceſs of coaling. But I had no apparent ſucceſs in thoſe experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it ſtand *in vacuo* a day and night, in which time it ſeemed to diſcharge a great quantity of air; after which, I let into the receiver the air, and thereby forced the oil into its pores. But the coal from this wood was not ſenſibly better than others. The application of heat may perhaps expell the phlogiſton in ſuch a manner, that the reſiduum, being fully ſaturated, can retain no more than a certain proportion. I made coals of other pieces of wood, when they were covered with cement; and I alſo coaled ſeveral pieces together, that they might receive phlogiſton from one another; but, in both caſes, without any ſenſible improvement in the quality of the coal.

In order to prevent the eſcape of the phlogiſton belonging to the ſubſtance to be reduced to a coal, I put ſome pieces of wood into a gun barrel, and corked it as cloſe as I could, at the ſame time covering the cork with cement. In this caſe the rarefaction of the exhaling vapour never failed to drive the cork out; but it muſt have been after a conſiderable reſiſtance to its eſcape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in ſubſtances that conduct ſo well as theſe can be accurately tried, at leaſt none that can be applied in this caſe. The charcoal I can make in a common fire, by the uſe of a pair of hand bellows, I cannot diſtinguiſh, with reſpect to its conducting power, from the moſt perfect metals, gold and ſilver; either by the length of the electric ſpark, the colour of it, or the ſound of the exploſion. I make no doubt but that wood, in the proceſs of coaling, may eaſily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this ſubject, by conſidering the degree of heat that is neceſſary, either to unite the phlogiſton to its baſe, or to ſeparate them, both in the caſe of wood, and the different metals. Lead is very eaſily calcined,

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and it is alſo known to conduct electricity very imperfectly. Iron ſoon turns to ruſt; and its conducting power I found to be very ſmall, in compariſon with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is neceſſary to calcine or revivify a metal, we may perhaps conclude, that the conducting power of the charcoal will be ſuperior to that of the metal. As it may be poſſible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than ſilver or gold would bear, without being diſſipated in vapour; it may even be poſſible to make charcoal that ſhall conduct electricity better than thoſe moſt perfect metals.

Had there been any phlogiſton in water, I ſhould have concluded, that there had been no conducting power in nature, but in conſequence of ſome union of this principle with ſome baſe. In this, metals and charcoal exactly agree. While they have the phlogiſton, they conduct; when deprived of it, they will not conduct.

I believe, however, that all vegetable, or animal ſubſtances, that contain phlogiſton may be reduced to a coal; and if the heat applied in the proceſs be ſufficient, that coal will conduct electricity. Fleſh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or ſeeming approach, I ever made towards retaining more phlogiſton than uſual, in wood reduced to a coal, was by the ſlowneſs of the proceſs. For I always found that, if the heat was applied very gradually, leſs volatile phlogiſton,

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i.e. leſs inflammable air, was expelled; and therefore I ſuppoſe that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was firſt coaled by a ſudden heat.

I took two pieces of dry oak, the contiguous parts of the ſame ſtick, each weighing exactly 14 grains. One of theſe I heated ſuddenly. It yielded 8 ounce meaſures of inflammable air, and then weighed 2 grains. The other I heated ſlowly, but as vehemently, at the laſt, as the other. It yielded only 1½ ounce meaſures, and weighed 3 grains.

I repeated the ſame experiment ſeveral times, and always with nearly the ſame reſult.

Examining the conducting power of the pieces of charcoal, made with theſe different circumſtances in the proceſs, I could not diſtinguiſh which were better. Perhaps a more accurate method of trying them might ſhow, that thoſe which were coaled ſlowly were the better conductors; unleſs, which is not improbable, the goodneſs of the conducting power conſiſt in the completeneſs of the union that is produced between the inflammable principle and its baſe, which will depend upon the degree of heat only, and not on the quantity of phlogiſton thus united to the earth.

N. B. To catch the inflammable air, ſet looſe in making charcoal, I put the ſubſtances into a gun barrel, to which I luted a long glaſs tube, and to the tube I faſtened a bladder, out of which the air was carefully preſſed.

As metals and charcoal agree in conſiſting of phlogiſton united to an earthly baſe, and alſo in conducting electricity, I suspected that these two different substances might also agree in their readiness to expand by heat. Mr. Smeaton was so obliging as to assist me in my attempts to ascertain this circumstance, by the application of his excellent pyrometer. Though we could not make the experiment with all the exactness that we could have wished, yet the result of near thirty trials was uniformly in favour of the greater degree of expansion, by heat in the charcoal, than in wood of the same kind, as we imagined, out of which it was made. In general, the expansion of the charcoal was about double to that of the wood.

It is evident that a certain degree of heat makes wood and charcoal expand, and also that a greater degree of heat makes them contract. I wish we had an instrument to ascertain the precise degree of heat, at which the expansion ceases, and the contraction begins; and whether the two effects be produced by the same gradation.

In the course of these experiments on charcoal, I met with a substance, the conducting power of which is singular, and exhibits a beautiful appearance. In order to see what would remain after burning a quantity of turpentine in a glass tube, I covered it with sand, in a crucible, in the same manner in which I used to make charcoal; and, after letting it continue a sufficient time, in a very hot fire, and the flame had long ceased, I examined the tube, and found that it had been melted; but, instead of any thing like charcoal, or the least blackness, I observed that the tube was uniformly lined with a whitish glossy matter, that I could not scrape off. Upon

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trying whether it would conduct electricity, I found it tranſmitted the ſmalleſt ſhocks, to a conſiderable diſtance; and, what appeared very remarkable, the path of the exploſion was luminous all the way, and ſeemed to conſiſt of a prodigious number of ſmall ſeparate ſparks, ſcattered to a great diſtance, exhibiting ſuch an appearance as would be made by firing gunpowder ſcattered careleſsly in a line. The exploſion very much reſembled the firing of a ſquib. To compare it to another electric appearance, it was like the exploſion paſſing through a thin ſurface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microſcope, it muſt, in fact, have been full of interſtices, and the electric ſparks could only be viſible in paſſing from and conducting one particle to another.

In this experiment, I often got pieces of glaſs very imperfectly covered, with intervals in the white coating very large and viſible; but, though I expoſed the ſame pieces of glaſs to catch more of this matter, I never could get a coating of it ſo thick, but that, in tranſmitting the electrical exploſion through it, it exhibited the ſame luminous appearance, as if there were interſtices in the circuit.

I got the ſame matter from oil of turpentine, and oil of olives; but not from bees-wax, or ſpermaceti oil; perhaps not from any animal ſubſtance.

In order to obſerve the progreſs of this incruſtation, I poured oil of turpentine on ſome flat pieces of glaſs, and burnt them on an iron plate, in the open fire, the heat being moderate; but the effect was a

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black covering, like ſoot, which would not conduct in the leaſt. But theſe ſame pieces of glaſs, thus covered with the black coating, being put into a crucible full of ſand, and urged with a ſtrong heat, came out white, and conducted exactly as before.

With a leſs degree of heat the black covering wa changed to white, but it did not adhere ſo firmly to the glaſs, as when the heat had been greater; though it adhered more cloſely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In ſome caſes I have found this whitiſh matter to be diſperſed by ſeveral exploſions, as Mr. Franklin found gilding with leaf gold to be.

In whatever manner the pieces of glaſs were covered, the coating vaniſhed when it was made red hot in an open fire; and the glaſs that remained would not conduct, any more than it did before. This circumſtance exactly reſembled the eſcape of phlogiſton from charcoal and metal, burnt in the open air.

In a microſcope, this whitiſh matter looked exactly like metal, or rather ſome of the ſemi-metals, having a bright poliſh, tho' it ſoon became, as it were, tarniſhed.

To try whether it was metal, I dipped the pieces of glaſs that were covered with it in the acids, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glaſs, but covers it quite ſuperficially.

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It was not in the leaſt affected by the magnet. Upon the whole, the matter that forms this coating of the glaſs ſeems to be a kind of charcoal, only white inſtead of black.

Conſidering that metals reſemble charcoal, in that they conſiſt of an earth united to phlogiſton, and that charcoal will not conſume without burning in the open air (there being, probably, ſomething in the atmoſphere with which it can unite, on the principle of chemical affinities, the moment it is ſeparated from the metallic baſe) I imagined that metals might not calcine or vitrify except in the ſame circumſtances, and the event verified my conjecture.

I took a certain quantity of lead, and having put it into an open crucible, obſerved that it was all vitrified in 10 minutes; but the ſame quantity of lead, covered with pipe clay, and ſand, was kept ſeveral hours in a much hotter fire, and was hardly waſted at all, the bottom of the crucible only being ſlightly glazed; it having been impoſſible wholly to exclude all acceſs of air, and ſome being neceſſarily in contact with it when the proceſs began. Treating charcoal in the ſame manner, I could never prevent ſome loſs of weight, when the crucible was kept in a very hot fire, for ſeveral hours.

As, by this proceſs, lead will bear a much greater degree of heat than would calcine or vitrify it in the open air, I ſhould think it probable, that lead thus prepared muſt have the phlogiſton more cloſely united to its earthy baſe, and be thereby a better conductor than common lead; ſince this is the caſe with charcoal thus treated. Perhaps lead, and other baſe metals, may have their quality altered, and be improved in other reſpects alſo by this proof; though they ſhould not be changed into gold by it. The ſpecific gravity is not changed by this proceſs; ſo that, alas! it is ſtill lead.