

XXV.—*Experiments on Oxidation by means of Charcoal.*

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It has been known to Chemists ever since the memorable experiments of Theodore de Saussure, that charcoal possesses the property of condensing many times its own volume of various gases, and in some instances, as in that of ammonia, or hydrochloric acid, from 80 to 90 volumes; but this property of carbon was considered as a mere physical property, until Dr. John Stenhouse's papers appeared in the "Chemist," for 1855 and 1857, in which he clearly demonstrated that carbon was a conveyer of oxygen to the various gaseous products emanating from substances in a state of putrefaction, and that all the putrid gaseous matters and vapours evolved from an animal buried in charcoal were so completely oxidized, that no unpleasant odour was perceptible.

These interesting results induced me to make a series of experiments with the view of ascertaining if, under such circumstances, the oxidation was carried to its maximum or not, and to what extent this mode of oxidation was applicable to organic compounds.

To carry out these views, I prepared pieces of boxwood charcoal as near 1 cubic inch in size as possible. These were treated with slightly diluted hydrochloric acid, and afterwards washed with distilled water, so as to remove from them all mineral matters, especially alkaline carbonates, as these might have interfered with the results, since acids are generally produced when oxidation is carried to its maximum. To ascertain the oxidizing power of condensed oxygen, a cube was dried and heated to redness, and whilst still red-hot, was introduced into a cylinder standing over mercury, and containing a measured volume of pure oxygen. It generally required 24 hours before condensation of the oxygen ceased, and it was only then that the gas to be oxidized was added, and the whole left until no further absorption was apparent. The

charcoal was then removed, and carefully tested for the products of oxidation formed.

Before however examining the action of charcoal containing condensed oxygen upon gaseous compounds, and especially organic substances, I considered it advisable to ascertain that no carbonic acid was produced by the contact of purified charcoal with oxygen. I therefore examined both fresh charcoal and some which had been in contact with oxygen, and the result in both cases was perfectly satisfactory, for no carbonic acid had been formed or existed in the cube of charcoal.

1. I then brought moist sulphurous acid into contact with charcoal containing oxygen as described above, and found that it was very rapidly converted into sulphuric acid.

2. A cylinder graduated into 100 divisions, being filled with oxygen, and the hot charcoal introduced, it was observed that after 24 hours the absorption was equal to 44 divisions. Moist sulphuretted hydrogen was then introduced, so as to replace the volume absorbed, or up again to 100 divisions, and in 24 hours the absorption was 72 divisions; so that the charcoal had absorbed afresh, 28 volumes of oxygen under the influence of sulphuretted hydrogen. I further ascertained that considerable quantities of sulphuric acid had been produced through the oxidation of the sulphuretted hydrogen.

3. Several experiments were made with the view of oxidizing ammonia, but I regret to say without any definite result; but although no production of nitric acid could be observed, I would not affirm that ammonia was not partially oxidized, for in this case as in the former one, the total absorption of gas was greater after the addition of ammonia gas, than it was with oxygen alone.

These results and those cited in the case of the oxidation of sulphuretted hydrogen, prove that Saussure was incorrect in stating that a gas possessed the property of driving out of charcoal another gas which had previously been absorbed by that substance, and that this was especially the case when charcoal was placed in contact with a gas more soluble in water than the one previously condensed.

4. In the case of gaseous phosphoretted hydrogen, prepared by heating phosphorus with a strong alcoholic solution of potash, the absorption was rapid, and, as in the preceding cases, a greater volume of the mixture was condensed than when the

charcoal was in contact with oxygen alone. In this instance a considerable quantity of phosphoric acid was also formed.

5. A second series of experiments was made with the view of producing organic acids from their alcohols by means of oxygen condensed in charcoal. The cylinder was filled with oxygen and a cubic inch of purified charcoal, red-hot, was introduced. After 24 hours, a few drops of methylic alcohol were passed into the cylinder by means of a pipette, and after 72 hours, no further absorption being noticed, the charcoal was removed, and a liquid extracted from it, which was found to reduce rapidly silver solution, thus tending to prove that formic acid had been formed; but there is a doubt if this acid was the real product of oxidation, as we could not reduce the salts of mercury, nor detect the presence of an acid by means of litmus.

6. Positive results were, however, obtained with ethylic alcohol when I operated in exactly the same manner as with methylic alcohol; for in this instance considerable quantities of acetic acid were produced, which I proved by converting a part of it into cacodyle, and the rest into acetic ether.

7. I was also successful in obtaining valerianic acid from amylic alcohol.

8. Lastly, a series of experiments was undertaken with the view of ascertaining what was the action of condensed oxygen upon some of the hydrocarbons, when the following results were arrived at:—

Ethylene (C_2H_4). Some of this gas was brought in contact with oxygen and charcoal, as other gases were in the preceding experiments, and when the absorption had ceased the products of oxidation were examined. Having failed to detect any solid or liquid products of oxidation (excepting water), such as oxalic acid, aldehyde, &c., the charcoal was placed into a small flask containing *boiled* water, and the gases evolved from it on boiling were passed through lime-water, when a copious precipitate of carbonate of lime was formed, showing that ethylene had been oxidized into carbonic acid and water.

9. I then tried propylene (C_3H_6), prepared by heating iodide of allyl ($\text{C}_3\text{H}_5\text{I}$) with mercury and hydrochloric acid, and here again carbonic acid was produced, but no acetic acid, although I fully expected that such a compound would have been formed.

10. Amylene (C_5H_{10}) was treated in the same way, and also gave rise to carbonic acid; but I must add that, in this

case, some other product was generated, for although the smell of amylene was completely destroyed, still there was an odour of one of the amylic ethers; which of them, however, could not be ascertained, as the quantity produced was too small.

The difference of oxidizing power with which condensed oxygen acts on certain carburetted hydrogens, or on the hydrates of the same, their alcohols, is well worth the attention of chemists; for whilst in the latter case it acts by substitution, replacing the hydrogen of the organic substance, in the former it acts in its true character (*viz.*, as the most powerful comburent agent known), oxidizing the carbon and hydrogen of the organic substance to its maximum, thus converting the carbon into carbonic acid, and the hydrogen into water.

These experiments show that oxygen condensed by charcoal, is a most powerful oxidizer; and the question arises, whether the oxygen, which we may assume is liquefied on the surface of the charcoal, acts as a mere solvent of the other gaseous or liquid products brought into contact with it, and thereby becomes susceptible of acting upon the elements composing such substances; or whether charcoal, which has condensed oxygen by mere molecular attraction, has still the additional power of condensing or liquefying a second gas if brought in contact with it, thereby bringing each substance or compound into such a condensed state, that chemical action can easily ensue, and give rise to the results mentioned in this paper. I must say that I am in favour of the latter view, and I hope, shortly, to be able to bring before the Society, a series of results which will substantiate my views upon this important chemical subject.
