

The burette is shaken to distribute the liquid over its sides and more of the liquid is introduced as the absorption proceeds. When absorption is complete the liquid is withdrawn as before, the tube is rinsed with successive small quantities of water, and the gas finally measured again over water in the cylinder M.

The obvious errors of the apparatus are the entrance of air about the cork K, and separation of gases from the liquids used in the burette when the level of the liquid in the latter is reduced. A soft, well fitting rubber stopper will remove the first objection and as an additional precaution the mouth of the burette is kept below the surface of the water whenever it is not necessary to manipulate the burette. In practice the entrance of external air through leakage is inappreciable during the time of the analysis.

The second source of error is avoided by using such quantities of absorbing liquids that the diminution of pressure within the burette may be slight. During the time of greatest exhaustion, that is during the withdrawal of liquids, the current tends to carry minute bubbles, that may separate downwards and into the bottle, and with a good vacuum (20-25 inches of mercury) in the latter, the time is too short to permit of any important volume of gas separating from the liquid.

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## THE CONVERSION OF CARBON MONOXIDE TO CARBON DIOXIDE BY ACTIVE (*i. e.* NASCENT) OXYGEN.

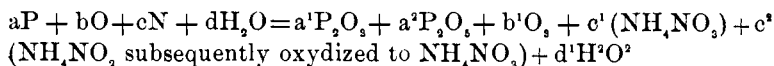
BY DR. ALBERT R. LEEDS.

In the early part of the year 1879, I was led by certain theoretical considerations to regard it as eminently probable that carbonic oxide would be converted by nascent oxygen into the highest stage of oxidation, and that by properly devising the nature of the experiments, and adequate precautions in the execution of their details, the direct oxidation of carbon monoxide to dioxide by nascent oxygen at ordinary temperatures could be demonstrated. Repeated trials amply justified these anticipations. The final experiment, some account of which will be found in the Jour. Amer. Chem. Soc., I, 232, was performed as follows: A 5-liter jar, containing 5 sticks of phosphorus and a little water, was filled with 2500 cc. of carbonic oxide, and the remaining space with air. The carbonic

oxide was generated from potassium ferrocyanide and sulphuric acid, and washed with water, caustic potassa, and barium hydrate. On transferring it to the jar, it was again washed through a similar train of purifiers, a final V-tube filled with barium hydrate solution remaining pellucid, proving absence of any trace of carbon dioxide. The air was purified in the same manner. At the expiration of 18 hours, the gases remaining above the moist phosphorus in the jar were drawn, first through a moist cotton filter, then through a potassium iodide solution, and finally through two baryta waters. The faint rose coloration which the iodide solution struck with starch water, was found on titration to correspond to 0.04 mgrms. ozone in the total volume of gas drawn over. The last baryta was absolutely pellucid, the first had absorbed 14 mgrms. carbonic acid. The carbonic acid estimation was made by decomposing the barium salt, and weighing the carbonic acid evolved, for fear that some traces of oxides of phosphorus might have escaped from the purifiers, and entered into combination with the baryta.

Later on, further details as to precautions employed in these experiments will be given, in connection with the adverse criticisms to which they have been recently subjected.

Somewhat later in the same year, a related field of experiment was entered upon. This was to determine conclusively, whether or no the triatomic molecule of oxygen was capable of effecting what the individual atom of oxygen, at the moment of dissociation of the molecule of oxygen, had been demonstrated to be capable of doing. It should be premised, that the decompositions and recombinations which occur when phosphorus is oxydized at the expense of, and with simultaneous reduction of the oxygen molecule, are complicated in their nature. An operose determination of the factors of this reaction (Jour. Am. Chem. Soc., I, 146 and II, 37), has shown that it may be represented by the equation



The actual values of the coefficients would depend, as was shown in another place, upon the temperature at which the reduction of the oxygen molecule is effected, and the values, a, b, c, d, of the members of the equation. But the important point established was that ozone, hydrogen peroxide, and nitro compounds of ammonia, were always formed, and their quantities always bore a certain relation to one another. Moreover, that the explanation of these

facts could be found only in the supposition of the formation of active oxygen, and the subsequent generation of ozone was no more an essential feature of the reaction than the contemporaneous and invariable production of hydrogen peroxide, and ammonium nitrite and nitrate.

In some earlier experiments, which had been performed with great care by Profs. Remsen and Southworth,\* the conclusion had been arrived at that carbonic monoxide was not oxidized to carbon dioxide by ozone.

As above stated, I was induced to renew the investigation,† inasmuch as certain preliminary experiments, which I had performed, had shown that carbonic oxide might undergo conversion into carbonic acid, under the same circumstances which brought about the oxidation of the oxygen molecule to the state of ozone. These were notably two: 1st. The oxidation of carbonic oxide by nascent oxygen, evolved in an atmosphere of moist air over phosphorus, as detailed above. 2d. The simultaneous formation of carbonic acid and ozone in a mixture of carbonic oxide and oxygen, subjected to the influence of the silent electrical discharge. Without pausing to rehearse *in extenso* the experiments by which this latter point was established, it will suffice to state that a quantitative determination resulted in showing that after  $1\frac{1}{2}$  liters of carbonic oxide had been submitted to the action of the silent discharge, in company with oxygen, 0.0271 grms. (misprinted 0.0271 mgrms. loc. cit.) of carbon dioxide had been formed.

Moreover, I had been enabled, by the use of a modified form of Siemens' ozonizer, to make use of large volumes of oxygen, raised to a higher degree of ozonation than the ozonized gas hitherto employed, and I thought that the theoretic importance of the subject warranted the institution of a fresh attempt to discover whether, with ozone of this degree of concentration, no oxidation of carbon monoxide occurred.

The actual experiment was conducted as follows:—

Oxygen, previously freed from every trace of carbon dioxide, was ozonized by passage through an ozonizing battery to the extent of 72 mgrms. of ozone per liter. It was then passed in excess, together with carbon monoxide likewise completely purified, through a tube one meter in length, which was placed *beyond* the ozonizer. But although the current of mingled ozonized oxygen

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\* Am. Jour. Sci., III. 11, 136.

† Jour. Amer. Chem. Soc., I, 450.

and carbon monoxide was made to flow with a very slow current, and though the experiment was continued for many hours, the baryta water through which the mixed gases finally made their escape, remained entirely pellucid. In other words, oxygen containing a very large percentage of free ozone, is not capable of effecting the oxidation of carbon monoxide.

Commenting upon this result, the inference was drawn:—"In this case, the energy of combination of the two bonds, presumably free, of the carbon monoxide molecule, appears to be of less valency than the energy with which any one atom in the ozone molecule, is retained by the other two. In the two former instances" (the oxidation of carbon monoxide by nascent oxygen, and the generation of carbon dioxide in a mixture of carbon monoxide and oxygen submitted to the silent discharge) "the ozone molecules are in the process of formation, and in the pre-existent stage (in which the oxygen molecule undergoes dissociation,) the liberated oxygen atom is seized upon by the unsaturated carbonic oxide."

Subsequently\* these views were still further generalized in an essay which had for its object the quantitative investigation of the various factors in the complicated reactions connected with the oxidation of phosphorus in moist air. It had previously been shown† that the amount of ozone produced by the oxidation of phosphorus under given circumstances, is constant for any particular temperature, and consequently, the generation of ozone may be graphically represented by a curve, the production at 6° C., being nil, rising to a maximum at 24°.25° C., and then rapidly diminishing as the temperature approaches 44° the melting point of phosphorus. As additional proof of the constancy of the phenomena occurring in the oxidation of phosphorus in moist air, it was established in the progress of this essay, that the amounts of ozone and hydrogen peroxide bear a constant relation to one another and to the air passed over. A similar proposition holds true of the phosphoric and phosphorous acids, and of the ammonia and nitric acid—the amounts of the latter bodies being in the ratio requisite to form ammonium nitrate. Later on it was shown that, neglecting the small amounts of hydrogen peroxide, which were held back by the wash-waters through which the escaping gases pass, the ratio of the hydrogen peroxide generated, was to that of the ozone as something over one to three, under the conditions of that particular experiment.

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\* Jour. Am. Chem. Soc. II., 34.

† Jour. Am. Chem. Soc. I., 8.

These various results caused me again to reiterate the wide generalization which had been brought forward in the earlier articles concerning the production of ozone under the conditions studied :—that it was connected with the uneven quantivalences of the elements taking part in the reaction. “If this hypothesis be true, then we should anticipate the development of ozone whenever oxidation of a perissad occurred at temperatures compatible with the stability of the ozone molecule. Even at the temperature of combustion of hydrogen, this is supposed by C. Than to be the case. He explains in this manner the presence of the ozone, which he states he has detected in the combustion of hydrogenous substances generally, and its absence in the combustion of carbon.”

“In entering into new combinations, the oxygen molecules must undergo temporary resolution into their constituent atoms. These while *en route* to take up new positions in other combinations, and animated by their atomic energy, or energy of the nascent state, may either oxidize the oxygen molecule, or the nitrogen molecule, or the molecule of water. In the first place, ozone would be produced; in the second, regarding water as the basic body and NNO as the nitryl, there might be formed, as Hunt has indicated, ammonium nitrate; in the third, hydrogen peroxide.”

This recapitulation is here given in order to recall the gradual evolution of the hypothesis of the necessary existence of active or nascent oxygen, as a body distinct from ozone, in order to explain observed phenomena. According to these views, the oxidizing effect of palladium-hydrogen, as noted by Hoppe-Seyler, at a later period in the history of these researches,\* was a particular case of the oxidation of a perissad (in this case hydrogen) at a temperature especially compatible with the stability of the ozone molecule. The intervention of nascent hydrogen, or active hydrogen, is not needed to explain the production of hydrogen peroxide when hydrogenium or palladium-hydrogen comes in contact with water. Hydrogenium is a perissad metal endowed with the most eminent deoxidating or reducing powers. When it reduces the molecule of oxygen, a temporary resolution of the molecule into its constituent atoms must necessarily take place. In other words, hydrogenium stands in the same relation to the production of hydrogen peroxide, as phosphorus does when oxidized in presence of moist air. Like the phosphorus it directly reduces the oxygen molecule, and sets free the oxygen

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\* Activation of oxygen by nascent hydrogen. Ber. der deutsch Chem. Gesell., XII, 1551.

atom. This active oxygen so formed, is the efficient cause of all the subsequent phenomena. It starts the series of chemical changes, and is the principal and all important factor, as related to which and resulting from which, the generation of the ozone, hydrogen peroxide, and ammonium nitrate and nitrite, are three correlative and necessarily dependent, but inevitably and indissolubly connected, consequences. According to this hypothesis, the generation of ozone is not the central fact around which the possible formation of active oxygen and the other phenomena are grouped, but it is a secondary phenomenon and is of no more importance so far as the theory of the reaction is concerned, than the simultaneous development of hydrogen peroxide, and, in presence of nitrogen, of nitrogen compounds.

The foregoing history of previous investigations is given, in order to render intelligible the attitude in which I stood with regard to the statements recently made by Profs. J. Remsen and E. H. Keiser in a paper "On the Conduct of Moist Phosphorus and Air towards Carbon Monoxide" (*Amer. Chem. Jour.* IV, 454). It will be further necessary only to state that my views are not the same as those advanced by Hoppe-Seyler, as may be seen from the different induction which I drew from Hoppe-Seyler's own experiments. For in his original experiments this investigator did not suspect the necessary presence of peroxide of hydrogen, as an indispensable factor in the reactions observed. Subsequently (*Ber. der deutsch. Chem. Gesell.*, XIV, 976) I pointed out that this must be the case, and on performing the experiment, quantitatively determined the amount of peroxide formed. The only reasons for previously anticipating, and actually performing the experiments confirmatory of the presence of hydrogen peroxide, were the theoretical considerations connected with the development of active oxygen above enunciated. Ignoring these results, M. Traube in a later issue of the *Berichte* (XV, p. 659) brought forward the formation of hydrogen peroxide under these circumstances, as his own observation, and has founded upon this production of hydrogen peroxide a hypothesis concerning the validity of which, and concerning the experimental verification of the experiments upon which it is based, I shall defer writing until the publication of a subsequent article.

After recounting certain preliminary experiments, the crucial one, by which Remsen and Keiser hold that they have conclusively established the non-conversion of carbon monoxide to dioxide in the presence of moist air and phosphorus, and so far as the evidence

afforded by this experiment is concerned, the non-existence of active oxygen, is stated (by them *loc. cit.*), as follows:

“An apparatus was finally constructed in which these were entirely excluded, or, at least, so protected that the ozone could not possibly reach them. Although a number of different forms of apparatus were tried, we need only describe that which we finally decided upon as the best. This consisted of a flask of from three to four liters capacity, provided with a doubly perforated cork stopper. Through this there passed one glass tube reaching to the bottom of the flask, and another reaching only half way. Outside the flask the shorter tube was connected with the apparatus intended to remove the carbon dioxide from the air, and the longer tube was bent twice at right angles, and then passed through the stopper of a V-tube about eight inches high. Between this V-tube and a second like it, connection was made by means of a doubly bent glass tube. In the flask there were placed usually two or three sticks of phosphorous, each three or four inches long, and enough pure water to somewhat more than fill the neck when the flask was inverted. The V-tubes were filled with ignited asbestos, and there was then added some mercury, so that when the tubes were inverted, in which position the entire apparatus was placed when in use, the metal covered the corks with a layer from three-quarters of an inch to an inch in thickness. The connecting tubes passed, of course, in each case through the layer of mercury. The vessel containing the clear baryta water was also connected with the last V-tube by means of a mercury joint similar to those above described. The baryta water was protected from the action of the air by placing before it a small V-tube containing potassium hydroxide, and this last tube was connected with an aspirator. Before connecting the bulbs containing the baryta water, air freed from carbon dioxide was drawn slowly\* through the apparatus. On now connecting with the baryta water bulbs no precipitate was formed. Even on allowing the air to remain in contact with the moist phosphorus for periods varying from one to twelve hours, no carbon dioxide could be detected. This experiment was tried over and over again at different temperatures, but always with the same result.

About one-third of the air in the ozone flask was now replaced by carbon monoxide, from which all dioxide had been scrupulously re-

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\* This operation requires care and constant attention. The phosphorus not unfrequently takes fire. In one case in our experience it took fire after having stood quietly and unmolested for several hours.

moved. The mixture was allowed to stand for some hours and then drawn through the baryta-water bulbs, when no precipitate was formed. This experiment was repeated a number of times with the same result. In some cases the air and carbon-monoxide were drawn together slowly for a long time over the moist phosphorus, but this made no difference in the result. So frequently was the experiment performed, so great were the precautions against error, and so undeniably-negative were the results, that, in spite of the explicit statements of Leeds and of Baumann, we do not hesitate to say that carbon-monoxide is not oxidized when exposed to the action of air and moist phosphorus. It follows, of course, from this, that the action of air and moist phosphorus on carbon-monoxide furnishes no evidence in favor of the view that there is an active condition of oxygen distinct from ozone."

The authors further say : "Leeds' error is easily explained. In the first place, he took no special precautions to protect the corks from the action of the ozone, and in the second place he passed the gases through a filter of moist cotton for the purpose of removing 'Oxides of phosphorus.' Now cotton, as it is usually met with, yields carbon-dioxide when subjected to the influence of ozone, so that the carbon-dioxide noticed by Leeds was probably the result of the action of ozone on the cotton or the oily substances which always adhere to it."

With regard to this explanation, it may be said that at a very early stage in experimenting with ozone, the observer is compelled to note the destructive action of ozone on corks and rubber connections, and either to eliminate them altogether or to boil the corks in paraffine and cover them with the same, both of which last precautions were taken in the present instance. The action of the very attenuated form of ozone obtained from phosphorus is excessively slight, as I knew when speaking of its employment in connecting ozone apparatus at an earlier date (*J. An. Chem. Soc.* 11, 150) "Paraffine does not perfectly withstand the action of ozone, being slowly decomposed with the formation of carbonic acid. In the course of long continued use, the paraffine joints employed by the author in connecting the ozonizing elements of his large electrical ozonizer (*ib.* 1, 440) were destroyed, and had to be renewed from time to time. The amount of action on the joints was progressive, the paraffine connection between the first and second ozonizing element requiring to be changed very seldom, while that between the eleventh and twelfth elements needed frequent renewal." \* \* \* \*



“ In experimenting with ozonized air, containing so small an amount of ozone as that employed in the investigation described in the present article” (ozonation by phosphorus), “ the connections may be made of paraffine without entailing serious error, but in researches of greater nicety, where not merely the relative but the absolute amounts of ozone have to be determined, paraffine connections cannot be employed.”

The cotton employed to remove the oxides of phosphorus, and I should have added in this as I have in other places, that cloud of vesiculated peroxide of hydrogen (the antozone of Schönbein and other authors) which by its passage through *water*, etc., without absorption, so puzzled earlier experimenters, this cotton, I repeat, was not the common cotton wool of commerce. It has been previously treated with alkali so as to remove all fatty matters, and then repeatedly exhausted with water and dilute acid, until its reaction was neutral, all soluble matters removed, and a white fibrous material as near pure cotton fiber as possible, had been obtained.

Still, in performing a crucial experiment, it was certainly important to eliminate every surface of contact except those of glass and water. Even mercury or any other possibly oxidizable metallic surface is objectionable, on account of a possible deozonation of the very minute amounts of ozonized air, which are formed by ozonation with phosphorus.

But before stating the details of this experiment, it will be well to narrate that performed by Baumann (*Zeitsch. f. physiologische Chem.* V, 250), who subsequently to my own labors effected the conversion of carbon monoxide to dioxide at ordinary temperatures by means of active oxygen.

He writes :—“ A slow stream of air free from carbonic acid was conducted through a flask containing moist phosphorus ; thence into a second flask containing a mixture of 3 vols. oxygen and 1 vol. carbonic oxide, and finally through baryta water. The last remained perfectly clear after the gas had been passed through it for six hours, and after every trace of carbonic acid had been previously removed from the apparatus. On the other hand, when the mixture of carbonic oxide and oxygen was conducted into the flask containing phosphorus, and in which there must have been, according to our proposition, active oxygen, an altogether different result followed :—the baryta water presently became turbid and in an hour threw down an abundant precipitate of barium carbonate.”

Commenting upon the above, our critics say :—“ Nothing is said

about stoppers or rubber connections, and it is safe to assume that, in the apparatus used by Baumann, ozone came in contact at some points with organic matter. Hence the formation of carbon dioxide is easily accounted for." \* \* \* \* "Whatever the sources of error in Baumann's experiment may have been, we are confident that a repetition with the precautions taken by us will show him that his conclusion is wrong."

It is not our duty, but that of the writer impugned, to inform us whether this confidence in his having neglected to take the most obvious and indispensable precautions in the conduct of his experiments, is well founded.

My own repetition of the original investigation was performed as follows :—

Seven six-inch sticks of clean phosphorus were placed in a ten-liter flat-bottomed bottle, with an accurately fitting glass stopper, the bottle filled with air, and carbonic-acid-free, distilled water, and inverted in a small pneumatic trough, likewise filled with distilled water. Carbon monoxide, which had been made several days previously, and allowed to stand over distilled water in a glass gas-holder, was purified by passing through a long train of purifiers similar to those employed in connection with carbon determinations in steel, and to which additional wash-bottles containing caustic potash and baryta water had been added. Whatever carbon dioxide was present originally, was so thoroughly removed that the baryta water in the last wash-bottle remained absolutely limpid on passing sufficient carbonic oxide into the ozonator bottle to fill it nearly one-half. An equal quantity of air, after passage through the same train of purifiers, and equally well freed from the dioxide, was then introduced into the ozonator. While the bottle was still inverted in the trough, the tight fitting stopper was introduced, and it was then reverted, sufficient water being allowed to remain in the jar half to cover the phosphorus. The bottom of the bottle being flat, on distributing the sticks by gentle shaking, they were partly and uniformly submerged and exposed the largest possible amount of deoxidizing surface.

The ozonator still standing in the water of the trough, was then brought to a temperature of 24° C. by heating the trough, and kept at this temperature for six days. The antozone cloud (i. e. suspended hydrogen peroxide) produced the first day gradually subsided and the atmosphere became perfectly clear. The very gradual disappearance of these clouds, in part due to the gradual inter-

action of ozone and hydrogen peroxide with the formation of water and ordinary oxygen, and in part due to the mechanical difficulty with which the peroxide when once brought into a condition of aeriform and probably vesicular suspension goes into solution in water, strongly countenances the supposition that they are mainly and probably entirely, suspended hydrogen peroxide. The energy with which the oxides of phosphorus unite with and are absorbed by water, makes it an untenable supposition that they could, even if formed, remain for any length of time suspended in the atmosphere, so that whilst in my earlier experiments it seemed easier to suppose the clouds to be oxides of phosphorus than a non-volatile body like hydrogen peroxide in a state of aeriform suspension, yet later on the latter explanation was proved experimentally to be the correct one.

At the expiration of six days, the atmosphere contained in the bottle was withdrawn by the following manipulation, all contact with organic surfaces, except for a few seconds when the atmosphere remaining in the bottle came in contact with paraffine, being impossible.

The glass stopper was removed, a cork saturated with paraffine through which the necessary connecting tubes passed, inserted, and instantly the mouth of the bottle plunged beneath the surface of a mercury trough. This inversion carried the phosphorus and water down, so that the water covered the paraffined cork. Then the cork was quickly loosened and the aspirator being connected with the series of connecting tubes, but no air being allowed to enter, mercury was drawn up into the flask sufficient to cover the paraffined surface. The cork being forced back again, remained covered with mercury, during the remainder of the experiment.

The glass tubes were so bent, that no breaks or connections in them occurred between the atmosphere contained in the baryta water of the last wash-bottle through which the air was aspirated to take the place of the atmosphere contained in the ozonator, and the baryta water contained in the wash-bottle, through which the mixture of carbon oxides and air remaining at the close of the experiment, was finally passed. No break, except in the ozonator itself. And here it occurred above the surface of the water, the displacing air entering near the bottom, the gaseous products passing out from the upper portion of the jar. To prevent the diffusion backwards into the last wash-bottle of the entering current, of any of the atmosphere in the ozonator, a bulb was blown on one of the

lower curves of the entrance tube so as to form a mercury valve. Such a precaution was not necessary on the escaping tube, its end dipping directly beneath the surface of baryta water. This baryta water was again protected from the action of the atmosphere, by being connected with another wash-bottle containing neutral solution of potassium iodide. On now aspirating a current of air freed from carbonic acid through the apparatus, an abundant white precipitate formed in the collecting flask, and eventually a white crust formed on the sides of the entering tube beneath the surface of baryta water. When the atmosphere of the ozonator had been completely changed and no further precipitation occurred, the contents of the wash-bottle were transferred to a carbonic acid apparatus and the percentage estimated in the ordinary manner. The air used in aspirating, was freed from carbonic acid likewise. The weighings were as follows :

Potash bulbs after decomposition and aspirating.....	43.8295 grms.
Potash bulbs before decomposition and aspirating.....	43.8140 grms.
Increase in weight (Carbon Dioxide).....	0.0155 grms.

There is no difficulty in obtaining a qualitative test for carbon dioxide from the white crust adherent to the entrance tube, and the amount of which should properly form a part of the 15.5 mgrms. actually weighed. On cutting off this tube with its white crust, dropping it into a test tube and allowing a drop of acid to run down the crust, it was energetically decomposed with the formation of bubbles of gas, and a drop of lime water properly supported on the under side of a glass cover stopping the exit of the test tube, became turbid.

Objections to the fact of the above experiment being conclusive as to the production of 15.5 mgrms. and upwards of carbon dioxide.

1st. That there was an interval during which at the close of the experiment the atmosphere of the ozonator was in contact with the paraffine cork. Admitting that the air in this experiment was sufficiently charged with ozone to decompose paraffine, yet as a matter of fact at the close of the experiment no ozone was present. The potassium iodide in the final wash-bottle, after acidulation, contained no free iodine. And as it should properly be objected that this negative result is inconclusive, on account of the combination of nitrogen and ozone in presence of alkaline liquid to a nitrate, it

should also be remembered that this reaction would not have been sufficiently energetic to detain every trace of residual ozone.

In my earlier experiment, in which 5 liters of air had been used and the amount of carbon dioxide formed was 14 mgrms., the amount of residual ozone was only 0.04 mgrm.

2nd. The water contained sufficient ozone in solution to decompose the paraffine submerged beneath in the instant of time before being covered with mercury. This is eminently improbable, since the establishment of the fact that ozone is soluble at all, was a very laborious matter, and was only successful after many long continued qualitative tests made with very highly ozonized oxygen. (J. Amer. Chem. Soc. I, 220.)

The same remark applies to the possibility of the dissolved ozone being converted into ordinary oxygen in contact with the mercury covering the paraffined cork.

3rd. It may be said that even if we grant that carbon dioxide was formed, it might have resulted from the combined action of ozone and hydrogen peroxide, both of which oxidants were certainly present. But inasmuch as neither of these bodies alone effects the oxidation of carbon monoxide, (and Prof. Remsen's own experiments are conclusive as to this point), and inasmuch as their mutual action is not of an oxidizing but of a reducing character, both bodies being reduced, the one to oxygen, the other to water, the above supposition is untenable.

I hold, therefore, that the fact of the oxidation of carbon monoxide to dioxide by air over moist phosphorus, has been established by a rigid quantitative and qualitative analysis. Moreover, that this oxidation is, as I have always held it to be, the proof of the existence of active oxygen, as a body antecedent to the formation of ozone.

The essential feature in my own interpretation of the phenomena studied, is that active oxygen is always formed when reduction of the oxygen molecule occurs at temperatures compatible with the stability of the ozone molecule. That nascent hydrogen is not essential to this reduction is shown by the reduction being effected by phosphorus, in which case the intervention of nascent hydrogen does not occur. Hydrogen and phosphorus are probably only the two most conspicuous of the bodies capable of reducing the oxygen molecule at ordinary temperatures, and whilst both these are perissads and this fact appears to be connected with the production of active oxygen, I am not able in the present state of experimental

knowledge to say that the perissad nature of the reducing element is an essential feature of the formation of active oxygen. As I have repeatedly stated, the essential part of my hypothesis is the reduction of the oxygen molecule at a temperature compatible with the stability of the ozone molecule. Even without chemical intervention, the silent electrical discharge will do this, and form primarily active oxygen and secondarily ozone. That such is the case is shown by the formation of carbonic dioxide when a mixture of carbon monoxide and oxygen are submitted to the silent discharge.

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## ABSTRACTS.

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Abstracts from the Journal of the *London Chemical Society*, by E. Waller, Ph. D.

**On the Constitution of some Bromine-derivatives of Naphthalene.** (Third notice.) R. Meldola (XLIII. p. 1.)

Description of the preparation of meta di brom naphthalene, Beta-di brom naphthalene, tri-brom naphthalene, and ortho-di brom naphthalene.

**On the Constitution of Lophine.** T. R. Japp. (XLIII. p. 9.) An argument against the view of Radziszewski, regarding the composition of the lophine molecule.

**Contributions to the Chemistry of Lignification.** C. F. F. Cross and E. J. Bevan. (XLIII. p. 18.)

The hypothesis of the authors previously adopted that *bastose* is a chemical whole in the sense of presenting a true combination rather than a mixture of cellulose with its own cellulosic constituent, has received further confirmation from their researches.

The close connection between these plant constituents and the trihydric phenols seems to be established.

A chlorinated derivative of *bastose* was obtained, which, though amorphous, was the same in composition when obtained from different sources, from jute, and from manilla. (*musa paradisiaca*.) The composition corresponded to the formula  $C_{32}H_{44}Cl_{11}O_{16}$ .