In case I should happen to succeed in isolating the principle of ozone, as I hope I shall before long, I have a good mind to go to York with the view of performing my philosophical miracle before the British association. What do you think of that plan? Its execution would perhaps give some zest to the proceedings of the chemical section there. From having lately worked a little too much I am rather knocked up and want some relaxation. A trip to England would no doubt do me a great deal of good, but Mrs. Sch. will hear of no such thing and declares such a locomotion as downright wantonness.[1] But after all she would not throw any great obstacle in my way, if I insisted upon the visit. She charges me to present to you and Mrs Faraday her humble respects, in which I of course join

Your's

S.

Schœnbein to Faraday.[2]

MY DEAR FARADAY

Having of late made a series of experiments with the view of producing by chemical means that odoriferous principle which I have called "Ozone" and which is, as you are well aware, disengaged at the positive electrode during the electrolysis of water, as well as near the points, out of which common Electricity is passing into the atmospheric air, and believing that I have succeeded in the attempt, I think you will

[1] Schœnbein's frequent journeys, in fact, never seem to have been much to Mrs. Schœnbeins liking. Thus he wrote to his wife from London, Sept. 1. 1839: 'Du siehst also, meine liebe Frau, dass man den Mann doch in manchen Dingen machen lassen muss".

[2] Faraday on receipt of this letter sent it to Mr. Christie, by whom it was received on April 9th, to read at the Royal Society. With the exception of some slight alterations, it was read unchanged under the following title: "On the production of Ozone by chemical means. By Professor Schœnbein." The Phil. Mag. (vol. 24. 1844. p. 466) and the Proceedings of the Royal Society (vol. 5. 1844. p. 507) both contain short abstracts of it.

read with some interest a summary account of my proceedings and results. (*The details regarding these researches will be described in a paper which is to be published in one of the forthcoming numbers of the "Archives" and "Poggendorff's Annals".*)[1]

If at the common temperature, a piece of phosphorus be put into a bottle filled with ordinary air, an atmosphere is very rapidly formed in it, which possesses the property of polarizing positively a plate of Gold or Platinum *which is*[2] plunged into the said atmosphere for a few moments. In one instance, the needle of my galvanometer was deflected 90° by a gold plate, which had remained for twenty seconds within a bottle, whose air had previously been in contact with phosphorus for *only*[3] one minute.

The positively polarizing power of that atmosphere arrives at its maximum of intensity, sometimes, within the space of a few minutes, sometimes *in*[4] that of as many days, according to circumstances, into the description of which I cannot enter at this present moment.

That maximum being once reached the intensity of the polarizing power decreases, and within more or less time sinks down to zero, but not to remain in that state. The atmosphere, after having assumed a neutral, or inactive voltaic condition, passes into an opposite state i. e. acquires the power of polarizing negatively a plate of Gold or Platinum *which is*[5] put into it (the atmosphere) for a few seconds. This newly acquired power is, according to circumstances, either slowly or rapidly gaining in intensity until it reaches also its maximum. Having arrived at that point the atmosphere does not undergo any other change of state, if left to itself. *I am able*[6] to bring

[1] The italics are our own and signify that the passages in question were omitted at the reading of the paper, or otherwise amended; whereas the notes will in each case give the original reading of Schœnbeins letter.

[2] being. [3] but one. [4] within. [5] being. [6] I have got it under my control.

about the described variations of the voltaic condition of our atmosphere, either in a slow or sudden manner, and with respect to that point of my experiments I will only say that the rapidity of the changes alluded to, essentially depends upon the degree of temperature at which phosphorus is acting upon the atmospheric air. Supposing our atmosphere *to have*[1] assumed its neutral condition, remove the phosphorus from the bottle and put into the latter any readily oxidable metal, being in the *form*[2] of filings or powder, e. g. iron, tin, zinc etc. or any other substance being eager to unite with oxigen, for instance the protochloride of tin or of iron, or the common iron vitriol, shake the atmosphere with one of the bodies named and it (the atmosphere) will almost instantaneously be brought again to a positive condition of consi[de]rable intensity, which state does not seem to be liable to change any more. If on the other hand our atmosphere, after having acquired its highest degree of negatively polarizing power, be treated in the manner described, for instance with iron filings, this power is not only entirely and suddenly destroyed, but the atmosphere changes altogether its voltaic nature and assumes a highly electro-positive condition. It is a matter of course that by the quantity of oxidable matter put into the negative atmosphere, we may regulate at pleasure its voltaic condition. *(The intensity of its negatively polarizing power may be only diminished, or the atmosphere may be rendered neutral, or more or less positive from the slightest degree of that state to its maximum.)* Before farther proceeding in the account of my researches, I must not omit to mention the fact that by putting a solution of chloride of Gold into an atmosphere, whose positive condition has been restored by means of readily oxidable substances, that condition is suddenly and irrecoverably destroyed.

From the facts stated, it appears that by the slow action of phosphorus upon atmospheric air two gazeous princibles

[1] having. [2] shape.

are simultaneously produced, which are opposite to each other with regard to their voltaic properties; one of them is an eminently electropositive body, the other a still more powerfully electro-negative one. Under ordinary circumstances the generation of the first principle prevails at the beginning of the said action over that of the second one, but in the more advanced stages of that chemical process the contrary takes place. The production of the electro-negative principle becomes more copious than that of the positive one and hence it comes, that our atmosphere, whilst remaining in contact with phosphorus passes through different stages of voltaic condition until it arrives at the maximum of its negatively polarizing power.[1] But what is the nature of the two principles? As to the electropositive one, I am inclined to think it to be vaporous phosphorus mixt up with particles of what is called "phosphatic acid". *(Et voici mes raisons for making such a supposition. If you pass very slightly a piece of phosphorus over a plate of gold or platinum, the latter deflects very perceptibly the needle, if it be voltaically combined with a similar metallic stripe being in its ordinary state. The said deflection is such as to indicate a current passing from the phosphorated plate to the common one. I have also ascertained the fact that a plate of platinum or gold being surrounded with a solution of phosphorous acid, as well as of phosphatic acid, is positive to a similar plate being plunged either into acidulated (by muriatic acid for instance) or chemically pure water. That phosphorus is capable of assuming the vaporous state at the common temperature, no Chemist I think doubts of and that by the slow action of phosphorus upon atmospheric air phosphatic acid is produced*

[1] Grove writes in a letter to Schœnbein (Jan. 5. 1845): "Some of your results are very curious; particularly that of the two different sorts of polarisation by phosphorus. I have been making some expts. with Phosphorus but had not observed or indeed sought for such an effect."

belongs to the class of well-known facts.) It is *however* possible, and I think it even likely, that besides the two positive substances mentioned a third one of the same voltaic kind is generated in my experiment, but I do not think it reasonable yet to state the reasons for my holding such an opinion. I must however not omit to mention that the electropositive principle or principles, if shaken with a solution of chloride of gold throw down a perceptible quantity of that metal a fact that merits to be taken into consideration.

But what is the chemical nature of the electro-negative substance generated during the slow action of phosphorus upon the atmospheric air? Do not be startled at my telling you at once that it is my "Ozone", for I have got my good reasons for making such a bold assertion. The princible ones are as follows:

1. As long as our atmosphere exhibits a notably strong polarizing power of the positive kind, its smell is similar to that of garlick i. e. *to*[1] the smell which we ascribe to phosphorus; as soon however as that atmosphere is approaching to its neutral voltaic state, an easily perceptible change in its odor takes place also. It now begins to resemble that of Ozone. That smell grows stronger and stronger, the more exalted becomes the electro-negative condition of our atmosphere, and before having arrived at the maximum of its negative intensity, it is utterly impossible to the most delicate nose to perceive the slightest difference as to smell, between the odoriferous principle disengaged at the positive electrode during the electrolysis of water, and that being generated by the slow action of phosphorus upon the atmospheric air.

2. All the substances being possessed of the power to annihilate almost instantaneously the odor of ozone are without any exception capable also of destroying suddenly the same smell of our atmosphere.

[1] is.

3. All the substances having the property of destroying the negatively polarizing power of the odoriferous oxigen *being* eliminated at the positive electrode during the electrolysis of water, do also destroy the same power possessed by our ozone-like smelling atmosphere. To the facts mentioned I might yet add some others which you could hardly help considering as sufficient to prove, I think, beyond any shade of doubt the identity of the two principles in question.

The alleged matter-of-fact reasons are however, to my opinion at least, such as will fully bear out the correctness of my assertion, according to which "Ozone" is formed during the slow action of phosphorus upon the atmospheric air. The question "what is ozone itself?" I am not yet prepared to answer, I hope however to be able *to send*[1] you very soon some scientific news *upon*[2] that subject, being at *this* present *moment* very busy with isolating that curious principle. But whatever ozone may be, it appeares to me to be a most remarkable fact, a phenomenon highly worthy of all the attention of philosophers, that the odoriferous principle spoken of is generated under circumstances, being, apparently at least, so essentially different from each other. For I ask what similarity *exists*[3] between the passing of common electricity from a charged conductor into the atmosphere, the electrolysis of acidulated water and the slow action of phosphorus upon atmospheric air? Different as these circumstances appear to be, it will and must ultimately turn out that, with regard to the possibility of the generation of "Ozone" offered by them, they are alike. At any rate, you will agree with me in the opinion that a great number of accurate experiments must yet be made before we shall be enabled to clear up the mystery which still hangs about the subject.

I think however that the path is now opened which will lead us to the solution of our problem and it is not necessary

[1] of sending. [2] about. [3] does exist.

to assure you that I shall endeavour to the utmost of my powers to arrive at that end.

Should you think the contents of this letter interesting enough to be communicated to the Royal Society I have no objection to your doing so or to your making any use of them you think fit.

I remain

My dear Faraday

Your's

most faithfully

Bâle March 30 1844 C. F. SCHOENBEIN.

P. S. To obtain the results such as they are described in the preceding lines it is indispensably necessary to depolarize the electrodes after each experiment made with them and the galvanometer. Heating them red hot is the easiest method to effect that depolarization.

Faraday to Schœnbein.

Royal Institution 12. April 1844

MY DEAR FRIEND

I received your letter three or four days ago and was very greatly interested by it. I have given it to Mr Christie[1] to read at the Royal Society. — I do most earnestly hope that you will make out and establish this *Ozone;* it is a very fine thing to do and as you say, though the means of proving it seem to be anomalous and strange when composed together, yet most great discoveries in science have appeared equally strange and confused to us in the first instance. — I have not yet repeated the experiments for certain private troubles

[1] Samuel Hunter Christie, F.R.S. Professor of Mathematics at Woolwich, was born in 1784 in London and died 1865 at Twickenham.