him see the Royal Institution. He was once a pupil of mine and is in every respect a most excellent and amiable young man.

In offering to you and Mrs. Faraday my most hearty salutations

I am my dear Faraday

Yours

most truly

Bâle May 11. 1843. C. F. SCHOENBEIN.

Faraday to Schœnbein.

Royal Institution 16 May 1843

MY DEAR FRIEND

I must begin to write you a letter, though feeling, as I do, in the midst of one of my low, nervous attacks, with memory so treacherous, that I cannot remember the beginning of a sentence to the end — hand disobedient to the will, that I cannot form the letters, bent with a certain crampness, so I hardly know whether I shall bring it to a close with consistency or not. But that most valued thing, your kindness, moves me to write, when to another I would not reveal my weakness by a halting letter. As to your opinion and power of judgment etc. of a certain person[1] I have no doubt the advantages you possess which, I admit, have shown you blemishes as well as beauties; but I will not put your candour to the tets by asking for them. The glass of a kind heart through which you look has something to do with the matter.

Now as to the book in English I am afraid to say any thing on the matter, not because of my opinion of it, for how

[1] p. 277 of his 'Mitteilungen aus dem Reisetagebuch eines deutschen Naturforschers."

can that be anything but favourable; but because of the woeful mistakes which I have made in judgments of this kind before. I will tell you a case. A dear friend, a foreigner, now dead, sent me a M. S. on English scientific matters, which I thought good, and booksellers of character told me they thought good and attractive. In one way or another it led to the printing and publishing of the work. I paid for the printing and did not receive one farthing back from the sale. I could not tell my friend this; he never asked for or had an account, and the thought often comes back to my mind that up to the day of his death, he might perhaps imagine I had made a profit by his work and never rendered him an account. — So much for my judgment in these matters. In fact I find the Booksellers prospects are nothing but words, words, words. — I wish Murray would take your work in his own hands, for then I know he would use a sound discretion, but I do not know how to get him to do so.

As to the steam paper,[1] it is now printing and when you have it I hope you will think the reasoning satisfactory. The point that the water must be pure is a very strong one as a ground for conclusions — As to Grove I do not recollect that he says isolated oxygen and hydrogen can by combining produce a current of electricity[2] — but I have no confidence in my memory in such matters. — I have been reading with great pleasure some of your papers lately, but am so confused I cannot just now remember which; but I have not yet touched No. 7 of the "Archives" where I see your name — it now lies before me, but fear to read because of the giddiness.

[1] On the electricity evolved by the friction of water and steam against other bodies. Phil. Trans. vol. 6. 1843 p. 17.

[2] Phil. Mag. S. 3. vol. 14. 1839 p 130. Grove says at the conclusion of an account of an experiment in which a galvanometer was deflected when connected with two strips of platina covered by tubes containing oxygen and hydrogen: "I hope, by repeating this experiment in series, to effect decomposition of water by means of its composition".

De la Rive is here and I have seen his experiment on the increase of the decomposing power of a single pair of plates by adding in the inductive power brought into play at the moment of interrupting the current.[1] Grove brought the account over from Paris and tells me that he found all there, that he spoke to, apparently aware of the effect. I imagine this was only because they recognized in it an action due to the principle I had examined in Exp. Researcs series IX.[2], especially as illustrated at 1084. For myself I thank De la Rive for a very beautiful form of the application, though it is the same principle, and I do not see why a thermo-current should not be exalted in the same manner until it could effect chemical action and now indeed I have a faint recollection that Watkins or somebody has done that also.

I grieve to hear of Mrs. Schœnbein's illness and cares with the children, I wish there were nothing but happy pleasure in her way. But all these cares have their reward in a mothers bosom, and though we dislike them at the moment, it is better they should be than *not*. Nevertheless I am very glad to find that all are improving. The kindest thoughts from us both to you both.

Ever My dear Schœnbein

Your faithful friend

M. FARADAY.

Faraday to Schœnbein.

London Royal Institution 8 Aug 1843

MY DEAR FRIEND

I have the opportunity, though in haste, of sending you a copy of my last paper, probably the last. I know you

[1] Grove relates in a letter to Schœnbein (Nov. 14. 1843) that he has been working on what he calls voltaic reaction, a method of increasing the force of a voltaic combination by adding to it a reaction occasioned by itself. vide Phil. Mag. S. 3. vol. 13. 1843. p. 443.

[2] Phil. Trans. S. 3. vol. 18. 1835. p. 41.