

Notes and Comments

The Swedenborg Scientific Association is on the eve of undertaking studies of Swedenborg's scientific and philosophical works to prepare material for new editions.

In this context, the following letter¹ from Ernest Farrington to Alfred Stroth (who at the time was doing Swedenborgiana research in Sweden) is significant for many reasons, among them that it was written at the time in church history when some thought that a New Church science should develop on the basis of Swedenborg's ideas; there is reference to it in paragraph two. This involved the effort to interpret the chemistry of the day (1905) in terms of Swedenborg's theory of "finites" as presented in *Principia*. Much of the latter part of the letter is devoted to this topic.

We would welcome comments on both the subject of early efforts to frame a New Church science and on the attempt to correlate the finites with early 20th century chemical theory. In addition, we would welcome comments on other aspects of the letter's contents.

Letter from Earnest Farrington to Alfred Stroth

1626 Walnut St., Phila., Pa. August 25th, 1905

Dear Alfred:

If you have been as busy as I during the last six or seven weeks, letter writing must be a lost art with you/—it is with me. Cooper has been in the south since the first of July, and this necessitated three trips a week to Bryn Athyn for me, in addition to all the town work. As you may imagine, I have had little time for study, letter writing or even for reading the periodicals. George is back in B A again now, and this gives me a little time to breathe, but he expects to leave for Pocono at the end of the week, to be gone ten days, so I shall not get any work done, other than the routine of

¹We are grateful to Cynthia Walker for finding the letter in the Swedenborg Library Academy Archive.

practice, until October. I suppose you will be home again by that time. By Jove, old man, we will have some good times next winter. I have taken a little flat on the first floor of the Warwick, at 19th and Sansom, and between that and the office and laboratory, we ought to have opportunity for “improving our minds by many curious experiments and interesting dissertations,” not to mention pleasant social times. My work at Hahnemann will be particularly enjoyable this winter, as I have asked for a place in the physiological lab instead of the purely chemical department. The course will be a straight physiological course, with any frills of a chemical nature that we may see fit to throw in, and will give me an opportunity to learn a lot from direct animal experiment. Charlie Mohr has told me that I could have the run of all the labs,—a chance too good to miss. George and I both expect to do some work in the Academy this term, but I am quite in the dark as to what it will be, other than that it will deal with anatomico-physiologico-hygienic topics.

Novecclesian science (pardon the expression, but I have an innate feeling of its appropriateness) is dead—or at least very sick—at the present time. There is a growing query on all sides as to the cause of it, which you and I of course know, and which the laymen are fast finding out. Something may come of it.

Mr. Doering is again agitating the subject of a “circle scientific” to meet at the homes of the elect. I should certainly go into it with much pleasure, and I hope—more profit. I wonder how it will “take” in the Bryn Athyn Society as a whole. I had planned writing one or two papers for the Swedenborg Philosophy Club of Chicago along the line of that little paper about Swedenborg’s chemistry that appeared in the *N. P.* There is much that might be done with this subject, and perhaps it would be well to open a discussion of it here in *B. A.* The point that impresses me most strongly is the relation of water to chemical processes. This is striking in the inorganic field, but in the organic field it is no less than astounding. It stands out prominently in the old theories of types formulated by Dumas and Gerhardt, and even in the radical hypothesis, and it may be traced, although less clearly, in the present-day fusion of those two ideas. The part that water plays in body metabolism is nowhere more plain than in the researches of Odling. The other day I had the good fortune to pick up his little work on animal chemistry for the remarkable sum of five cents and I

have read it with great interest. He errs, as do the majority of chemists, in drawing unwarrantable conclusions as to the chemistry of vital phenomena from laboratory syntheses, forgetting that the same agents are not at work in both cases, and that tabulative and artificial relations are by no means necessarily genetic or actual. But his treatment of the subject brings out the undeniable relationship of the water particle to all or at least a majority of the chemical changes taking place in the body, both anabolic and katabolic. I have often wondered why some modern student did not follow out this line of enquiry. It seems to me rich in suggestive material. I have a feeling that water bears some basic relation to the whole of the chemistry of the carbon compounds. Why among all the seventy-odd so-called elements, should H and O, either as water or as hydroxyl, be associated so constantly with carbon and nitrogen to form the substances capable of undergoing vital transformations? Why, furthermore, does C, O, H, N present the sum and substance of organic chemistry? And yet broadly speaking, it does. Why is it that salicin is merely saligenin and glucose plus a molecule of water? Why is it that hippuric acid needs but two molecules of water to break up into benzoic acid, oxyacetic acid and ammonia? Why do all the organic acids contain hydroxyl, and why does hydroxyl enter into the composition of the alcohols, phenols, etc? Questions like these might be multiplied to the number of several thousand. I confess the subject is not very well arranged in my mind, and presents a rather confused picture, but there must be a reason, and if we could find it, I am sure it would throw remarkable light on the Principia theory. These "organic" elements, C, O, H, N, (and we should doubtless add S and P), appear to be primitive types, simple in atomic structure, and of plain geometrical figure. This figure probably accounts for the constitution of the typical or "group" compounds as given by the older chemists. Thus the water type H-O-H, $\begin{array}{c} \text{H-N-H} \\ | \\ \text{H} \end{array}$ or ammonia type and the methane type $\begin{array}{c} \text{H} \\ | \\ \text{H-C-H} \\ | \\ \text{H} \end{array}$ must owe their figure to the geometrical configuration of the central atoms respectively. If we accept the Beekman interpretation of the identity of hydrogen, a serious difficulty arises, for our graphic formula of water should then represent HH in the centre and O about the circumference, and by the same token, HHH in the centre of N and HHHH in the centre of C. But this seems very improbable, for C is certainly a crystalline

body and must have a figure other than spherical. We cannot conceive of four volumes of H crowded into a tetrahedron or other similar body. Does H then have more than one function and figure? Hardly, for if so it could certainly be detected by variation in behavior. Moreover, how can we explain the action of unsaturated radicals such as -OH on the 3rd finite theory, or account for the substitution for H of metals like Na and K on the one hand and of basic elements like Br and Cl on the other? Surely we must try again on the hydrogen proposition. Why do you suppose it is that the elements do not exist in an atomic state when free? One would think that 3rd finites if liberated from all encumbrance by grosser particles, would spontaneously assume their natural axial and orbital motions, and yet we find the atoms of hydrogen travelling in couples, and this even when reduction of pressure gives them the fullest freedom of motion. It is only when electrified that they break up, and then the decomposition is a veritable shattering of the atom to bits. If modern data are accurate, all elements occur in groups of two except Hg, Cd, Zn, and Ba, which are monatomic, Se and O (ozone) triatomic, and As and P tetratomic (also S below 550 degrees, hexatomic). Can we make use of the old electrochemical theory of Berzelius to account for this? Swedenborg certainly had no such conception, for he even explained chemical affinity on the principle of pressure from without. Still another point to consider is the matter of allotropism. Swedenborg, for the most part at least, assumed that a gaseous element was composed of spherical units, but this is not of necessity the case, for many elements which, even according to his method of reasoning, are obviously angular in structure, may be volatilized. It seems reasonable to suppose that all the elements which present allotropic forms are angular and that the variation in appearance depends on the way in which the atoms face each other. Of course in case of volatilization these angular particles may be gathered about a spherical globule. If we examine the Periodic table we will find that the 1st series (excluding hydrogen) includes carbon, nitrogen and oxygen, which, taken with hydrogen, form our organic basis. We might be justified in setting these down as parent substances, from which the remaining congeners of each group were derived by involution. In series 2 we find Na, Mg, F, S and Cl, all of lesser importance in physiological processes. Series 3 gives us K, Ca, and Fe, which if we except I, includes all the elements of any importance in body

metabolism. The problem then is to trace the relationship of the members of series 1 to the lower members of the Principia theory. The close chemical relation of the elements C N O of this series might suggest a common origin, accounting for the evident complementary structure of the atoms. Viewed from the standpoint of organic chemistry alone, carbon appears to be the central member of the trio, but when regarded in relation to general chemistry and physics, oxygen seems most prominent, nitrogen being rather in the nature of a residue perhaps, as suggested by its inert properties. And here we stick. What can you suggest? What are we going to do with ozone and hydrogen peroxide? And with the numerous other anomalies that persist in obstructing the way? Well, anyway it is an absorbing topic. I wish that several of us could approach the subject from different directions, and compare results from time to time. Take for instance the viewpoint of inorganic evolution as worked out along the lines of spectrum analysis. Here again oxygen takes a central position. But we find that hydrogen, the cleivite gases and the proto-forms of Mg and Ca are prior, while N and C are parallel. The stellar spectra (of varying temperature stars) show the following sequence: H, Mg, Ca, O. Their atomic wts. are 1, 24, 40, 16; hence according to the periodic law we should have H, O, Mg, Ca with Na(23) and K(39) inserted. This does not occur in the star-spectra, but the difficulty may be avoided if we suppose that Mg and Ca are broken up at high temperatures. If we assume two decompositions we have,—dividing the at. wts by 4—6 for proto-Mg and 10 for proto-Ca, which thus fall naturally into the series. If oxygen is 5th finites, we must account for the occurrence of (Lockyer) proto-H, asterium, helium, proto-Mg, hydrogen—all before the appearance of O. This rather embarrasses our conceptions of the difference between metals and non-metals, and we are led to ask what after all is the real distinction between them? Are we justified in assuming that there were other forms generated prior to formation of water in Swedenborg's theory? Is it possible that a part only of the earth's crust was produced from water by pressure? I wish you could find time to give me some assistance on these problems.

The more I think about it the more am I impressed with the truth of the claim we made some time ago that the whole subject of N C science must be approached from the historical side. Not long ago I bought a copy of the chemistry of Joseph Black in 5 vols, edited by Robison. It will be of use to

us, but no doubt you will have an opportunity to pick up material of much greater value. While you are in Stockholm don't forget Bergman, Scheele and Berzelius, especially the latter. If you can get hold of some of his memoirs, do so by all means, for much of his work is not readily available here. Would it be possible, when you are in London again, to scour the book-stalls for early physiological and chemical literature? Look into Hewson. He had the misfortune to die young, but he certainly left his mark upon the anatomical and physiological thought of his day. His connection with Wm. and Jno. Hunter might serve as a clue to your finding something interesting. I should think the periodical literature of the late 18th and early 19th centuries would be a rich mine. There are several men of the French school of 1790 to 1830 that are worth looking up. For instance there is the work on the blood of Provost and Dumas. Hunt up the *Annals des Sciences Naturelles* in this connection. Dumas also did much valuable work in physiological chemistry, and although most of it was done in Paris, I should think you might possibly strike something valuable in London. Then there is Milne-Edwards, the physiologist, and, among the chemists who might be of use to us, Laurent, Gerhardt, good old Wöhler and of course Leibig. And say, don't forget to keep a weather eye for a photo of the blood in circulation.

Well, I hope you come home loaded for bear. If you do, I will guarantee to produce the bear, and he will not be polar but multipolar. Next winter will, I feel sure from the way things are shaping themselves, be a notable one in the history of N C science. Let us be well stocked with ammunition.

If you can spare a moment's time I wish you would write me what you are doing and what are your plans. I am anxious to hear about them.

As ever,

Ernest,
Fannington