XXII. *Hints on the Subject of animal Secretions.* By Everard Home, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read June 22, 1809.

THE brilliant discoveries of Mr. DAVY on the powers of electricity in producing chemical changes, suggested to me the

Dr. WOLLASTON's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the Society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. WILLIAM BRANDE to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir JOSEPH BANKS, and stated that I should bring them forward in my lectures; at that time Dr. YOUNG's Syllabus was not published, and Dr. WOLLASTON's opinions were unknown to me.

Dr. BERZELIUS, Professor of Chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

"Trace all the nerves leading to any secretory organ in a living animal, and divide "them, being careful to injure the blood-vessels and the structure of the organ "itself, as little as may be: notwithstanding the continued circulation of the blood, "the organ will as little secrete its usual fluid, as an eye deprived of its nerve can "see, or a muscle whose nerve has been divided can move. We may therefore easily "conceive, that any trifling alteration in the nerves of a gland, may materially affect "its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. BERZELIUS's work was shown to me by Mr. DAVY while this paper was in the press.

idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this Society, the object of which is the improvement of Animal Chemistry, their intimate acquaintance with these branches of science, renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. DAVY's discoveries can be applied, and he has given it the consideration it deserves.

The VOLTAIC battery is met with in the torpedo and electrical eel, and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts, one, that a VOLTAIC battery can be formed in a living animal, the other, that nerves are essentially necessary for its management; for in these fish, the nerves connected with the electrical organs, exceed those that go to all the other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a VOLTAIC battery of high power; but their structure appears to Mr. DAVY to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a VOLTAIC battery, have a power of accumulating and commu-

nicating electricity, is proved by the well known experiment of taking the two hind legs of a vivaceous frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

Experiment 1. Middle of January, 1809.

The conductors from twenty four four inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire, was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment 2. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of

an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed in the following experiments.

Experiment 3. 10th of March, 1809.

The conductors from one hundred and twenty four inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment 4. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power; a battery was employed, consisting of twelve four inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in

length was seen floating in the fluid, and adhering to the same wire.

Experiment 5. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates *very weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form, with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed.

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from

the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose, and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. HATCHETT's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the Society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.