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Dr. J.L. Prevost

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the good fortune to meet with Mr. Brown, during his journey through Germany (in 1833), will remember that he carried with him impregnated ovaria in spirits, and with his accustomed kindness showed the entrance of the pollen tubes into the ovulum to every one who took any interest in the subject. Notwithstanding this, Corda, a few lines lower down, affects an unpardonable ignorance of this fact, in order to arrogate to himself a discovery which Amici (already in 1830) and Mr. Brown had made long before him. . . . . . Inconsistencies are also evident in the figures. Fig. 14, for instance; the embryonal sac is termed *nucula,* (should be *nucleus,*) and the pollen tubes enter it in order to produce, by their emissions, heaven knows what kind of an imaginary figure. Fig. 22: here the embryonal sac is even called embryo (E), and the pollen tubes run around it. But it would be an herculean task to follow, step by step, this memoir. It will here suffice to observe that, with the exception of a few points of minor importance, everything almost surpasses the limits of possible error, and does not in the least represent nature. I refer every one, who has even but little practice in such examinations, to nature herself, as the observations are not of the most difficult kind."

ON THE DEVELOPMENT OF AN ELECTRIC CURRENT ACCOMPANYING THE CONTRACTION OF THE MUSCULAR FIBRE. BY DR. J. L. PREVOST.

About fourteen years ago we published, together with M. Dumas, a memoir on the muscular fibre, in which we determined that the contraction of the muscles was owing to the sinuous flection of the fibres; we attributed the flection to the attraction of the nervous reticula, which placed at short distances from one another and perpendicularly to the direction of the muscular fibres, approached each other when an electric current, emanating from the cerebro-spinal system, passed through them. Our observations having been made with a microscope inferior in goodness to those constructed by Amici, the true disposition of the motive apparatus escaped us, and our assertion was considered an ingenious hypothesis deficient in the investigations requisite for its confirmation. Last summer I again resumed this subject with better means, and the following is one of the results which I obtained. If we observe the muscles of a frog with a magnifying power of four hundred, we perceive that they are composed of small cylinders, the diameter of which varies from five to twenty hundredths of a millimetre: these cylinders are connected with each other by the cellular tissue, through which pass from one cylinder to the other the nerves and vessels.

The fibres arranged thus parallel to one another, fix themselves, without separating, either to the tendons or to the aponeuroses which correspond to their extremities, the latter becoming round and disposing themselves in a small cavity placed on the tendon to receive them.

The muscular cylinders, which we shall call fibres, are themselves composed of fibrillæ, the diameter of which amounts to about $\frac{1}{400}$

of a millimetre. They are placed in juxtaposition in the cylinder and united so closely that they appear to a common observer to form a homogeneous mass.

At the surface of the muscular fibres just described, we perceive some rings, which surround their entire circumference like small ribands; they are about $\frac{1}{300}$ of a millimetre apart from each other upon the fibre when it has lost all its irritability, closer on the living fibre: these rings belong to the enveloping membrane. If this latter becomes fissured longitudinally, which at times occurs, we observe the longitudinal fibrillæ which form its mass, project in the fissure. The torn portions of the rings enable us to observe the ends of the reticula of which they are composed, and which cannot be seen in the normal state.

On illuminating the muscular fibres by means of a mirror which reflects the light upon their upper surface, we observe that the nervous reticula which ramify on the muscle enter the linings of the fibres; they thus appear to surround them similar to a series of circularly curved handles (ansæ). The fibres in their quiescent state are not straight but slightly curved. When they act, every portion of the broken line which they present gravitates the one against the other, and the muscular contraction results from the shortening produced by this action. Such are the facts which every one may observe with a good microscope.

Now let us apply to this highly remarkable anatomical arrangement, the doctrine of electric currents along the nervous reticula. It is evident that in this case each fibre becomes a little magnet with a flexible joint, the various parts of which would tend reciprocally to attract each other, and would produce the effect which we observe in the muscular contraction. But how detect these currents? Hitherto they have been sought for only with the electrical multiplier; and we could not expect to find anything, as we had to do with closed circuits, and knowing at the same time that a divided nerve does not transmit any action. There was nothing left therefore but the magnet that could point them out to us. To employ the magnetic needle was rather a difficult affair: I had recourse to a different means.

If a needle is placed in contact with some finely divided filings, such as we obtain from a file and soft iron, be it ever so slightly magnetic, it is perceptible from the arrangement which the particles of iron take at its surface: they plant themselves as little needles, which arrangement is easily perceptible with a magnifier. We must not confound this action with the attraction by which minute bodies remain attached to a bar with which they are touched. I ran a very fine needle not magnetized into the thigh of a frog, following the direction of the fibres; the point projected and dipped into the filings. At the moment when I excited a violent contraction by wounding the spinal marrow, I saw the small particles of iron arrange themselves at the point of the needle as they do when it is magnetic; they disappeared with the irritation of the muscle.

By further investigating this phænomenon I hope to render it very evident, and I should have deferred the publication of it until then, had not Professor de la Rive advised me to join it to the preceding observation, [Matteucci's Researches on the Torpedo, vide last and present Numbers of Phil. Mag.] and to record its date in our Society.—*Bibliothèque Universelle*, November 1837.

ON THERMO-ELECTRIC PHÆNOMENA. BY CH. MATTEUCCI.

Every time that a copper wire attached to a galvanometer and well-brightened (*décapé*) is brought into contact with a second copper wire equally well cleaned, but heated by a lamp, we obtain an electric current, which passes from the hot end to the cold end. If we repeat this experiment with well-cleansed iron wires we obtain a contrary current which proceeds from the cold end to the hot one; the same takes place also with zinc and antimony. This difference is observed whatever be the temperature to which one of the wires is heated. Now if instead of touching the wires we immerse them in pure mercury contained in two capsules united by a siphon full of mercury, one of which is hot, the other at the common temperature, we have still a current, but which proceeds in the same direction with copper, iron, zinc, and antimony. The mercury has here no influence by the thermo-electric currents which it might develop, for the same results are obtained, if the two wires, always well-brightened, are immersed one after the other in the same heated capsule. It is therefore the action of the heat and of the air which produces an alteration at the surface of the metal: and in fact, if a copper wire is heated exposed to the air in the flame of an alcohol lamp, and afterwards immersed in the mercury where the other wire is, the same difference is still observed as when the unequally heated wires were placed one on the other.

I endeavoured to obtain thermo-electric currents with mercury by employing three capsules united by means of two siphons. In the outer capsules were plunged the wires of the galvanometer. I take away one of the siphons, heat the middle capsule and replace the siphon. I thus bring the hot mercury into contact with the cold. In this manner, however, I obtained but very feeble and doubtful deviations. Although the wire of the galvanometer was rather long, yet I doubt of there having been any development of thermo-electric currents on the mercury.

An amalgam of bismuth (1 of bismuth, $\frac{1}{4}$ of mercury) which is very crystalline, is endowed with a very considerable thermo-electrical power.

If a heated plate of bismuth is touched with the two extremities of a galvanometer, very powerful currents are obtained. If the bismuth be melted and we still retain the two ends immersed in the fluid metal, the currents cease; at times there are still some, but we at once easily perceive that either some of the bismuth has solidified, or that the two ends of the wire are unequally heated. With a larger melted mass in any sort of a vessel these currents entirely cease, If we then discontinue the heating, and allow the mass to become cold, at the instant when it solidifies the needle indicates