

prove the independent irritability of muscle, which is now generally an accepted fact among physiologists. M. Rosenthal asserts that these experiments (and those of Kuhne upon the sartorius muscle), do not prove this; which is equivalent to stating that it is not proved that curare paralyzes the motor nerve endings.

More direct evidence upon this point is that of Dr. Onimus, who, not long ago, "read a paper before the Academy of Medicine, Paris, upon electro-muscular contractility and the action of curare. Contrary to the opinion of M. Claude Bernard, Dr. Onimus believed that curare does not act on all parts of the motor nerves, but only on their trunks;—the nerve centres and terminal filaments being unaffected" (a).

In view of these authoritative opinions (and doubtless of others to which I have not access), it is evident that this objection falls to the ground and loses the weight which otherwise might attach to it.

But suppose it were established beyond doubt that the influence of the nerve were completely eliminated from the muscle in any case, and that the contractile protoplasmic masses of muscle were left wholly to themselves, and their life being not yet extinct, that they gave token of that still flickering life when comparatively rudely assailed by a shock of electricity or a corrosive or injurious agent,—what then? Such signs of irritability, elicited under such circumstances, would not militate against my thesis; for such would be the behaviour to be expected from still living protoplasm, wherever found, and would in no way disprove the contention that in the association of nerve and muscle in the organism the *role* of the nerve is to restrain or control the protoplasmic energy of the muscle so long as their mutual relations continue. For, after all, "the contraction of muscular tissue is, in fact, a limited and definite amoeboid movement, in which intensity and rapidity are gained at the expense of variety" (b).

Indeed, I think the rational view of the situation just depicted, turns the argument the other way; and tends to show that in the joint *role* of nerve and muscle the function of the nerve is *not* to goad or stimulate the muscle to contract. To suppose this is to assign to nerve energy the re-

lative value of the fifth wheel in the coach. Such enduring power of contractility as the muscle here exhibits evidently needs no supplementary aid from the nerve. What it really *does* need, however, is restraint, control and co-ordination for the purposes of the organization of which it is a part.

OTHER OBJECTIONS.

A further objection has been suggested, on the ground that on a nervous impulse reaching a muscle, an electric current is generated during the period immediately preceding the contraction of the muscle; but this is an objection which is only of any force on the assumption that electricity is a stimulant. There is nothing in the action taking place here to show that the electric current is a stimulant rather than a paralyzer. There is simply a "freeing of the forces in the muscle," just as the spark of electricity frees the forces bound up in gunpowder, and so fires the train (c).

As for the additional plea that nerve force and muscle force are too much alike for us to consider one a paralyzing and the other a contracting agent: that is merely begging the question. Nothing whatever is known regarding the nature of these forces; and the intimate structures of nerve and muscle are so widely different as to justify the idea that the product, so to speak, of each, is equally diverse.

This theory has been objected to as a proposed addition to the inhibitory system of the text-books. This is a mistake. If the views here enunciated were adopted, the huge incubus of the present inhibitory hypothesis could be in great part swept away, to the great advantage both of physiology and therapeutics.

If it be claimed that on the cutting of the spinal cord or of a nerve trunk, the "irritation" set up at the point of cutting, or the generation of electrical current as the result of chemical change in the transverse section, act as a stimulus, and the contraction of the corresponding muscle is thus produced, such a claim must be regarded as untenable for the following reason:—The acts just referred to cannot be stimulating acts, because they are attended by precisely similar effects as are produced in the muscle by death from any cause, in which condition, it is needless to say nervous activity is not increased. The proof of

(a) Dr. M. Foster, *Phys.*, p. 63.

(b) *N.Y. Med. Record*, 1880, p. 73.

(c) Rosenthal, p. 250.