SYPHILITIC AFFECTION OF CEREBRAL NERVES;

WITH OBSERVATIONS.

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CASE 1.—A gentleman came under my observation on July 17th last, who had had fever in India, followed by loss of power in the right leg, in 1881. From this he completely recovered. In October, 1883, he contracted syphilis, and secondary symptoms appeared in November following. When I first saw him his hearing was impaired, and he had constant singing in the head. The whole of the muscles supplied by the portio dura of the seventh nerve were paralysed on both sides. This had produced a very peculiar expression of countenance: the lower lip had fallen; the food lodged between the cheek and the teeth; he was unable to close his eyes; he never smiled; and when talking he artificially supported his lower lip with his finger, but even then his articulation was very peculiar; sensation of the skin of the face was perfect. He commenced a course of calomel baths on July 17th, and was directed to take a pint of the simple decoction of sarsaparilla (L.P.) daily. On Aug. 12th his hearing had improved, and he could bring his lips together and nearly close his eyes. On the 25th he had regained the motion on the left side of his face, some singing in the ears still remained, and, as he said, "the food still sticks a little in the jaw." During my absence from town this gentleman wrote to me, saying that, at the urgent solicitations of friends, he had been induced to place himself under the care of a "specialist for the nerves," so that he might be treated in the country.

The simultaneous affection of the auditory nerves and nerves of motion clearly point to the origin of the seventh pair as the seat of the diseased action in this case. The pathological condition is as clearly demonstrated as in Sir Charles Bell's experiments. In fact, the conclusions are, if anything, more satisfactory, as the patient could describe his symptoms. In Sir Charles Bell's first experiments he was, no doubt, led to a wrong conclusion. On dividing the branches of the fifth nerve in a donkey he observed that the lower lip dropped, and he then concluded that the fifth nerve was a nerve of motion. This error was subsequently corrected, and it was satisfactorily shown that upon the division of the branches of the fifth nerve in an animal the lower lip fell and the food escaped out of its mouth, because the regulating power, the power of feeling, was gone. In Sir Charles Bell's first essay, published in the *Philosophical Transactions* for 1821, he represents the seventh nerve as controlling the instinctive actions, but not the voluntary actions, of the face. In support of this theory Sir Charles Bell observed that after dividing the seventh nerve on one side the animal still continued using its lips in taking food. The motor nervous influence was withdrawn from one side only of the orbicularis oris, but not from the other. When the seventh nerve was divided on both sides the muscles of the cheeks, the muscles of the lips, as well as of the nostrils, were completely paralysed.¹

CASE 2.—A married gentleman had syphilis some years ago, for which he appears to have been imperfectly treated. After the secondary symptoms had subsided, the parietal bone on the left side became enlarged to a considerable extent. There was some loss of power in the right leg; this was occasionally better and occasionally worse. After the lapse of many months he had a fit and became unconscious, and there was some loss of power in the right arm. His memory at the same time suffered. The point of interest in this case was that for many months the power of adjusting the focus of the *left* eye was impaired. This was tested on several occasions and at long intervals, always with the same result. He saw well at a distance with the right eye, but was ordinarily short-sighted with the left. By an effort of

the will and after a few seconds he could focus the left eye to the same distance as the right, and see as well with one as with the other. On looking along a narrow ruler with both eyes the extremities of the ruler appeared to diverge, "so as to make a very closely formed X." There was nothing the matter with the eye itself, nor was there the least indication of any branches of the sympathetic being affected. The change of focus was produced entirely by a voluntary effort. The ordinary motions of the eye were perfect.

The very interesting question then arises, Upon what condition did the imperfect adaptation of the eye to vision at different distances in this case depend? Since Sir Wm. Bowman made the discovery of the mode in which the eyes are adapted to vision at different distances by a mechanism within the eye itself, little attention appears to have been given to the influence of the external muscles of the eye in producing the same effect. This subject, however, occupied the attention of former physiologists. The fact that the eye is naturally flaccid, and liable to different degrees of tension, indicates that by slight pressure its diameter may be altered. Extreme tension, which prevents any sense of fluctuation, indicates very serious disease. Short-sighted persons are constantly in the habit of half-closing the eyelids so as to make slight pressure on the anterior surface of the eye in looking at distant objects. The antero-posterior diameter of the eye is thus slightly shortened. The action of the external muscles on the eyeball might be called the coarse adjustment of the eye in contradistinction to the fine adjustment by the mechanism within the eye itself. The antero-posterior diameter of the eye may be lengthened as well as shortened. Dr. Hosack² says that external pressure is capable of altering the focal length of the eye. With a speculum he made pressure upon his eye while directing attention to an object twenty yards distant, and saw it distinctly. Endeavouring to look beyond, everything appeared confused. Upon increasing the pressure considerably he was enabled to read a book distinctly held at a distance of two inches from the eye, although in the natural state of the eye he could distinguish neither lines nor letters. This experiment may easily be repeated in a very rough way. If a book be placed before the eyes so close that the type can no longer be distinguished, and pressure be made through the upper eyelid deep into the orbit, the type will become more distinct. In all animals, birds, and fish the muscles which in man are called oblique are really transverse. The superior in all has its special nerve, the fourth; while the external rectus in like manner is supplied almost exclusively by the sixth. Sir Charles Bell has shown that with regard to opposing muscles when one acts the other relaxes, or loses its tone, so that they act in concert although not supplied by the same nerve.

Now, we have in the eye the external and internal rectus, which taken together have a double supply of motive nervous power, the one for the ordinary motions of the eye; the other to regulate the degree of divergence from the usual axis of vision.³ In like manner the two oblique or transverse muscles taken together having a double nervous motor supply must have a double function. The first, that of rolling the eye so as to enable it to follow a revolving object sufficiently long to produce distinct vision; the other has not been generally acknowledged. Considering the disposition of the oblique or transverse muscles in all classes of animals, the action must be (when they act together) to compress the eye laterally and then slightly to alter its antero-posterior diameter. In dissecting the eyes of animals I have found a thick cushion of fat on the inner side of the eye. This, during the action of the oblique or transverse muscles, would afford a slight resistance and compress the inner surface of the eye.

Irritation of a nerve has a natural tendency to increase the susceptibility, and, if long continued, the development of the muscle which it supplies. A familiar instance of this is shown in the case of children who, from some reflex irritation, begin to squint. If the irritation be long continued, the strabismus becomes permanent. A very remarkable case occurred under my observation some years ago, which illustrates the same point. A young girl had her little finger drawn across the palm of her hand, so as to prevent her closing the other fingers. After a variety of ineffectual treatment, Sir William Fergusson removed the finger.

¹ Vide May's *Physiology*, p. 174, and Caesar Hawkins, *THE LANCET*, March 15th, 1856. A well-marked pathological case such as that above described might have given the same conclusions with regard to the functions of the seventh nerve as those arrived at by so much toil and so many experiments. It is curious that Sir Charles Bell's first experiments illustrate his own words—that we can seldom rely upon the answers that are extorted from living animals by experiments which go counter to the natural feeling of mankind.

² *Philosophical Transactions*, 1794, Part ii., p. 222.

³ The influence of the sixth pair of nerves must be of very especial service to animals, such as the hare, that look behind them in running.

Upon dissecting the parts, I found that the adductor minimi digiti was enormously developed. After a time the ring finger was drawn across the hand in the same way.

The conclusions to be drawn from the foregoing observations are—first, that the focal length of the eye may be altered by external pressure; secondly, that the mechanism by which this is usually effected is under the control of the fourth nerve; and, thirdly, that prolonged irritation of that nerve, direct or reflex, and of its corresponding muscle produced the comparatively and permanent short-sighted condition of the left eye in the last recorded case.

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ON THE NERVOUS OR MUSCULAR ORIGIN OF CERTAIN SPASTIC CONDITIONS OF THE VOLUNTARY MUSCLES.

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OF late years the method of pathology has been, and is still, becoming more and more experimental. The like is true of therapeutics, experimental pharmacology becoming daily more recognised as an important means of study. Experimental pharmacology is, however, able to do more than teach us in therapeutics, it is able also to render important services to pathology itself. The following paper is offered as a pharmacological contribution towards experimental pathology.

In experimenting with drugs a striking fact is everywhere apparent—viz., that we may reproduce the facsimiles of disease in the laboratory. We may illustrate this statement sufficiently by examples taken from two important genera of disease—e.g., if from that kind of morbid action which manifests itself in the shape of over-action of the muscular apparatus—i.e., spasm,—if from this we select tetanus as an example, we can at once put against it as pharmacological counterpart the effect on the organism of strychnine in toxic dose. Still better examples do we get from that kind of morbid action which manifests itself in the shape of under-action of the muscular apparatus, to which we apply the name paralysis. Pathology has taught us concerning this form of disease that it may be of cerebral, of spinal, of peripheral origin; and pharmacology enforces this teaching by presenting us, as the result of drug action, with each of the above varieties of palsy. Thus the group of the anæsthetics, including chloroform, ether, nitrous oxide, may probably be considered as of value chiefly in that its members abolish the functions of the cerebral cortex, motor and sensory (the sensory effect is in excess of the needful here). Then to illustrate by drug action the second group, that of the palsies of spinal origin, we may take physostigma, the action of which is paralytic by depressing the functions of the cord; lastly, to illustrate the third group we may take curare, this drug, as is well known, effecting paralysis by abolishing the functions of the motor nerve endings. It may be said that we have not pushed our examination very deeply and have not attempted to decide, in the case of physostigmine action, whether the paralysis is the result of action on the motor or on the sensory areas of the cord; but this was not the problem before us. We had simply to present, as the result of drug action, a paralysis of spinal origin; the further analysis is not called for.

These few examples will suffice to show how much pharmacology may aid us in the study of disease, since, enabling us as it does to actually produce disease, to multiply its causes, it thereby enables us, by a careful comparison of these causes, to separate the essential from the non-essential, and so to arrive more nearly at a just appreciation. This is the object we have in view in the present paper—viz., to compare certain effects, the result of drug action, with certain other effects (symptoms) the result of disease. We shall begin by first describing the real thing and then proceed to its simulacrum.

There has long been known as an occasional symptom in certain nervous disorders a condition of the muscles marked

by rigidity. Thus in cases of cerebral lesion—e.g., apoplexy,—we are familiar with the so-called early rigidity of the paralysed parts, probably an irritative effect, and also with the rigidity which supervenes more remotely, and is hence known as late rigidity; this latter is probably significant of cord implication, the result of a descending degeneration. Then in various primary diseased conditions of the spinal cord we may meet with this rigidity of the muscles; thus in lateral sclerosis of the cord, likewise, and probably in virtue of the same, in disseminated sclerosis. In the so-called spastic paraplegia of infants we again meet with the same symptom; in this case it may be observed that the pathology is still obscure, and that it is uncertain whether a cerebral or a spinal morbid state underlies the symptom.¹ Lastly, we have in what we term cramp, or cramps, tonic spasm of certain portions of the muscular system, the simple forms of which we are in the habit of regarding as local affections.

In all and each of these cases the question arises, What share are we to attribute to the muscles themselves—what to the nervous system? In how far is the pathology myopathic or neuropathic? This question is none the less pertinent that we have just seen the symptom we are considering in association with disease of the cerebrum and of the spinal cord, for there still remains to determine in how far cause, in how far coincidence is before us.²

A very valuable aid towards answering the above question on clinical grounds has been gained of late years in the appearance of a disease which has been named Thomsen's disease, though most accurately described before Thomsen by Leyden. The literature on the subject of this disease is chiefly German, though more recently a very valuable article on the same has been contributed by Messrs. Ballet and Marie.³ Amongst German literature the article by Bernhardt⁴ is very complete, and we shall follow chiefly in his lines in our description. To bring out the characteristic features of the affection it will be perhaps best to take an imaginary case and endow it with the several symptoms which have been observed in the cases hitherto recorded.

The patient we will suppose to be a man⁵ in early adult years. His complaint is simply and solely of muscular stiffness—nothing else. His appearance is that of health, and it will be found on questioning him on this point that there is nothing manifestly wrong. His past will likewise very possibly show a clean bill of health, with the exception of the affection of which he now complains. As to this his memory will probably carry him back to early childhood. This dating back of the disease to early childhood comes out very abundantly, and is an important feature in the complaint. In one of the three cases published by Seeligmüller this history did not obtain, the patient stating that the affection had appeared suddenly, some four or five years previously, as a stiffness affecting one leg, the left; and that subsequently the other limbs had become implicated. Seeligmüller⁶ rises to the occasion, and thinks that the patient had previously suffered from the complaint, but had not noticed it! Be this as it may, the recognition of the disease at a very early date remains a generally observed fact. Hence for most authors the affection is congenital.⁷ Passing to the patient's family history, we may very possibly find that the mother or the father has suffered in the same direction, or, in the absence of such, that a brother or sister is similarly troubled. So marked a family history as in the case of Dr. Thomsen, in which in three generations at least we trace the complaint, we shall scarcely look for. The evidence then obtainable from the family history will give prominence to heredity. We have thus traced back the complaint into the life of the patient himself and into that of his family. What, now, is this complaint more definitely? It is that in the performance of certain voluntary movements the patient finds himself hampered. Thus, should the patient flex vigorously his

¹ Leyden, discussing this question of muscular rigidity, states that it occurs not infrequently in cases of spinal cord disease, and, as he believes, "in association with various diffuse processes of the motor areas."—*Klinik der Rückenmarkskrankheiten*, 1874, Bd. i., pp. 128, 129, *et seq.*

² Under the heading "muscular rigidity" we have not mentioned the rigidity met with in the cataleptic state; yet this also must be included in a sufficient pathology.—*Op. cit.*, p. 128.

³ *Archives de Neurologie*, No. 13, Jan. 1883.

⁴ *Virchow's Archiv*, Bd. lxxv. (1879), p. 516.

⁵ Recorded cases are too few to yield statistics of any value on the subject of sex.

⁶ Gerhardt, *Handbuch der Kinderkrankheiten*, Bd. v., Erste Abth., 2te Hälfte, p. 175.

⁷ Thomsen has observed the affection in his own family, even in the cradle, *Archiv. f. Psychiatrie u. Nervenkrankheiten*, Bd. vi. (1876), p. 709.