

new growths and in certain of those with tumours of one or other hemisphere the reactions were sufficiently characteristic to be of supplementary value in localising the disease. In other cases with intra- or extra-cerebellar tumour, on the contrary, the results were often ambiguous and afforded no assistance in establishing a diagnosis. In many instances, indeed, the conclusions drawn from these results were actually at variance with the other physical findings, and had much reliance been placed on them would have led to error. Several factors are probably responsible for the occurrence of atypical reaction in patients with cerebellar tumours. In the author's opinion the two most important are, first, the greatly increased intracranial pressure due to internal hydrocephalus, and, second, the diffuse nature of many of the tumours.

Thomas Guthrie.

Randall, B. A., and Jones, I. H. (Philadelphia).—The Ear-tests of Bárány in Locating Cerebellar and other Encephalic Lesions.
"Amer. Journ. Med. Sci.," April, 1916.

This paper is the outcome of work done in the Department of Neuro-otology, recently created in the University of Pennsylvania. In the past eighteen months 125 pathological cases have been investigated in the Department, and of these 18 have been submitted to operation and 4 have been examined *post-mortem*. These sections, *ante-mortem* and *post-mortem*, have shown the deductions from the ear examinations to have been remarkably accurate. Most stress is laid in this paper on the pointing reactions, which have far surpassed the nystagmic in value and importance.

The writers have come to the conclusion that all pointing reactions are primarily cerebral, and not, as generally stated, cerebellar. Yet they are achieved through the synergising action of the cerebellum. With the cerebral mandate is associated the cerebellar influence, muscle sense, arthro-dial sense, tactile, auditory, and visual impressions, and memory. The pointing reactions are, in fact, dependent on so many things that a complicated arc must be assumed to explain them. The writers postulate and give proofs of a "subjective circuit" through the cerebellum to the higher centres. They also claim that each semicircular canal has an entirely separate tract of its own, and this they have proved in the case of the horizontal and superior canals in a series of 28 cases.

In conclusion, they affirm that by means of these ear tests "we can usually distinguish lesions of the labyrinth from those of the cerebellum; we can always tell when the eighth nerve is diseased; we can say positively whether or not the posterior longitudinal bundle is affected; and we can detect a lesion of the cerebellum, but cannot always locate it."

Thomas Guthrie.

OBITUARY.

SIR VICTOR HORSLEY, C.B., F.R.C.S., F.R.S.

THE premature death of Sir Victor Horsley is the greatest loss medical science has so far sustained in this horrible war. Though not slain in battle he fell like a soldier on the post of honour he had selected. With that utter disregard of himself which he had shown throughout life he had no sooner heard of the insanitary conditions prevailing in

Mesopotamia than he volunteered for the post of Consultant in that deadly climate. There he succumbed to heat-stroke on Sunday, July 16th, 1916, in the fifty-ninth year of his life.

Many have been since the sad event the voices of those extolling Horsley's merits. Nor is this to be wondered at. So great have been his achievements in the divers fields of anatomy, physiology, pathology, neurology, surgery, and social progress that each of them would suffice to secure undying fame to the departed friend. Indeed, not a few of those who most strenuously combated his politico-social views will, after the experiences made in this war, admit that his foresight was greater than their own. Nor do I doubt for a moment that the scientific repute of Victor Horsley, great as it is amongst his contemporaries, will continue to increase until he will be recognised as one of the rare geniuses of the medical profession.

Better qualified pens than my own have already paid tribute to Sir Victor's outstanding merits as a general physiologist and daring surgeon. To me it is only left to register what he has done for laryngology, and mournfully to lay down at his bier the thanks of our community. The Editor of the JOURNAL OF LARYNGOLOGY, RHINOLOGY, AND OTOTOLOGY has kindly desired me to deliver our "Vale," and adds that there was no one more fitted to do so than myself. In a certain sense that is true: for more than thirty years I have been united with Victor Horsley by intimate friendship, and with the exception of his "Note on some of the Motor Functions of certain Cranial Nerves, etc." (*Proc. Roy. Soc. of London*, vol. xlv, 1888), which he wrote in collaboration with our mutual friend, the late Dr. Charles Beevor, and in which the authors endeavoured to show that the soft palate, the uvula, the levator palati, and the pharyngeal constrictors were innervated by the spinal accessory nerve—all Horsley's laryngological work has in some way or another been linked with my own. This will explain and excuse, I trust, why my task is not undertaken in the shape of the cool and impassionate survey of an independent observer. It is conceived in the spirit of ardent love and admiration; and, furthermore, it has to be, to some extent, autobiographical, since our whole joint work dates from our close personal association. To characterise its nature, I cannot do better than quote the sentence from Horsley's obituary notice in the *Times*: "It was his generous custom to invite other workers to his laboratory and to place his services at their disposal; he would carry out all the delicate manipulations necessary at the request of any colleague, and would then resign to that colleague all the credit of the work accomplished." This description literally applies to our joint work.

Already, when publishing in 1881 my first paper on the greater vulnerability of the abductor fibres of the recurrent laryngeal nerve in progressive organic diseases of its centres or trunk I had arrived at the conclusion that clinical observation and pathological anatomy, though they established the existence of this phenomenon, were unable to explain its cause. Hoping that experimental research might advance such an explanation, and unacquainted with the technical difficulties of this course, I obtained permission to undertake the necessary studies in a London physiological laboratory. So long as the results of exclusion of the activity of certain nerves had to be investigated all went well. But when I came to study the effects of electric stimulation of intact laryngeal nerves I obtained most contradictory results. Repetition of the experiments undertaken in different species of animals only increased the difficulties. The authorities of the laboratory, whose aid I invoked,

were unable to shed light upon the irreconcilable results. Ultimately I was driven to the conclusion that the method I had been advised to employ (use of Ludwig's clamp-electrode) was faulty, and this conviction was confirmed by a physiologist who had himself worked a great deal in this line, and who explained to me that my results had been vitiated by spread of the electric current to neighbouring nerves. I had thus learned, by unpleasant experience, that in order to obtain reliable results for the performance of these delicate experiments not only zeal but technical experience were indispensable, and after several months thus simply lost I gave up my—mostly fruitless—endeavours in 1882.

In November of the following year, in a discussion on myxœdema, held in the Clinical Society of London, I suggested that myxœdema, Kocher's "cachexia strumipriva," and cretinism merely represented different phases of one and the same pathological process, and that they all were due to loss of the function of the thyroid gland, which at that time was commonly supposed to have no function at all. My proposition was simply derided. But when I returned to the charge at a meeting of the same Society in October, 1884, and showed that in a case of removal of a goitre in a young boy, the joint symptoms of myxœdema and cretinism had supervened, my statement was received in a very different spirit, and a committee was appointed to investigate the whole question of myxœdema and its allied morbid conditions. Amongst the members of that committee were the youthful Mr. Victor Horsley, who was at that time already known to be engaged in experimental study of this subject, and myself. It was at one of the meetings of this committee that I made Horsley's acquaintance.

I had looked forward with keen interest to this occasion. Although he had only recently graduated, and had only been appointed Assistant Surgeon to University College Hospital in 1885, his physiological and surgical work had already attracted general attention, and he was universally looked upon as "the coming man." My anticipations were not belied. From the first moment I felt irresistibly attracted by the handsome young man with the Apollo-head on a stalwart body, and with the frank expression of his intellectual, amiable face. Our liking seemed to be mutual. He invited me to come to the Brown Institution, to which he had been appointed Professor Superintendent in 1884, and to follow the progressive myxœdematous changes in monkeys whose thyroids he had extirpated. To-day it is a historical fact that by these experiments he definitely proved that myxœdema was caused by the loss of that organ, and that by his suggestion of grafting the thyroids of sheep into the abdominal cavities of patients suffering from that disease—though this method was subsequently superseded by feeding, on Murray's suggestion, the patients with thyroid extract—he laid the foundation of modern organotherapy.

During one of my visits to the Brown Institution Horsley began to talk to me about my neurological work, with which he showed himself surprisingly familiar. I told him of my experimental failures, and added that they were the more regrettable, as Krause lately had endeavoured to imitate the pressure of a growth upon the recurrent nerve by attaching a piece of cork to the recurrent laryngeal nerves of dogs, and that he had on the strength of his results advanced the surprising theory that the median position of the affected vocal cord in cases of progressive organic lesion of the corresponding motor nerve was not due to greater vulnerability of its abductor-fibres, but to a primary neuropathic contracture of all the muscles supplied by that nerve. These experiments, I felt sure,

imperiously demanded repetition in order to ascertain whether the original process and the imitation were identical processes or not.

Horsley replied that he had been particularly interested in a discussion which had taken place between Krause and myself on that topic at the International Medical Congress of Copenhagen in 1884, and to my equal surprise and delight offered me there and then, not merely to repeat Krause's experiments and to resume my own original experimental research, but to investigate with me the whole representation of the larynx in the central nervous system! Need I say that I most gratefully at once accepted this generous offer?

Thus begun our collaboration. It lasted uninterruptedly from 1886-1890, and was resumed for a short time in 1893 and again in 1897. I look back upon this association, in which Horsley undoubtedly took the lion's share, as one of the greatest scientific privileges of my life. The number of our experiments during that time exceeded one hundred, performed on different species of animals (monkeys, dogs, cats, rabbits), and the nature of the experiments undertaken extended to:

- (1) Repetition of Krause's experiments of compression of nerves.
- (2) Repetition of Frank Donaldson's experiments. (Greater excitability of abductor muscles by weak electric currents.)
- (3) Repetition of Franklin Hooper's experiments. (Peripheral and differentiating effect of ether upon the laryngeal muscles.)
- (4) Repetition of Jeanselme and Lermoyez' experiment. (Loss of excitability of laryngeal muscles after death.)
- (5) Investigations, so far as the larynx is concerned, of the functions of:

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|-------------------------|---|---------|
| (a) Spinal accessory | } | Nerves. |
| (b) Vagus | | |
| (c) Superior laryngeal | | |
| (d) Median laryngeal | | |
| (e) Recurrent laryngeal | | |

- (6) Investigation of action of crico-thyroid muscles.
- (7) Width of glottis after section of both vagi nerves below origin of recurrent laryngeal nerves.
- (8) Excitation of cortex cerebri.
- (9) Ablation of cortex cerebri.
- (10) Excitation of corona radiata.
- (11) Excitation of internal capsule.
- (12) Ablation of hemispheres.
- (13) Excitation of medulla oblongata.
- (14) Position of glottis after section of recurrent and of superior laryngeal nerves.

To work with Horsley along these partly just opened, partly entirely uncultivated, fields of science was a liberal education. To no one did the old saying "Familiarity breeds contempt" less apply than to him. With ever increasing admiration one watched the mental plan governing his whole line of research, his perfect surgery and marvellous ambidexterity, his scrupulous care and inexhaustible patience, his fertility of modifying, varying, and newly inventing methods of examination, his safeguards against falling into any of the fallacies in which experimental research abounds, the sobriety of his deductions, the clearness of his literary expressions. And what enchanted me more than all were those flashes of genius which, often quite suddenly and unexpectedly, gave an entirely new turn to our line of thoughts and allowed us to contemplate the subject under consideration in an entirely new light. Yet

all this was done without any affectation of superiority, done as simply and naturally as if it were self-understood. I learned to love Victor Horsley in these unforgettable years, the happiest ones, from a scientific point of view, I have ever spent.

Our work has been between twenty-five and thirty years before the profession. With few exceptions and additions its conclusions are still accepted as valid. It will therefore suffice if I briefly summarize its outcome.

In 1886 we published our first joint paper: "On an Apparently Peripheral and Differential Action of Ether upon the Laryngeal Muscles" (*Brit. Med. Journal*, September 4 and 11). In this it was shown—as previously discovered but left unexplained by Franklin Hooper—that ether exercises a peripheral, differentiating influence by means of the circulation upon the antagonistic groups of laryngeal muscles, and that, therefore, a difference in the metabolic processes of the abductor and adductor muscles appears to exist. Moreover, it was demonstrated that in all species of animals operated upon the abductor muscles, though individually the largest of all laryngeal muscles, lose after death their electric excitability long before the other laryngeal muscles.

In the same year I read my paper, "Abductor Paralyse, nicht Adductorencontractur," before the Deutsche Naturforscherversammlung at Berlin. In this I showed from very different points of view the fallacy of Krause's contention. The experiments which proved that Krause's cork-experiments were only superficially similar, but pathologically different, from the pathological process in man, were, with most ingenious variations, performed for me by Victor Horsley.

From 1886 to the end of 1889 we were busy with our experiments and did not publish anything concerning them. After we had published our results in a preliminary note, however, "On the Central Motor Innervation of the Larynx" (*Brit. Med. Journal*, December 21), Prof. Krause, who fancied that we had endeavoured to rob him of a priority which we had not only never contested but directly acknowledged previously, forced upon us a perfectly superfluous and absolutely unnecessary controversy (*Berliner Klin. Wochenschrift*, Nos. 4 and 7, 1890).

Shortly afterwards we had another much more scientific controversy ("Du centre cortical moteur laryngé et du trajet intra-cérébral des fibres, qui en émanent," *Annales des Maladies de l'Oreille et du Larynx*, Mai et Juin, 1890) with MM. Garel and Dor. These gentlemen maintained that a unilateral cortical lesion could determine a pure laryngeal hemiplegia on the opposite side. We opposed that notion on the strength of our own experiments and of clinical experience, and, although the statement has been spasmodically revived from time to time, I think that its untenability is nowadays generally admitted.

In the same year I published my paper, "On the Position of the Vocal Cords in Quiet Respiration of Man and on the Reflex-tonus of their Abductor Muscles" (*Proceedings of the Royal Society*, vol. xlviii). The paper is entirely my own, but it seemed desirable to ascertain by experiment whether respiratory impulses could be conducted rhythmically along the different fibres of the pneumogastric nerve to the respiratory centre, and there be changed into a tonic semi-innervation of the posterior crico-arytænoid muscles. The extremely difficult experiments—section of the vagi below the origins of the two recurrent laryngeal nerves—which were required to test my idea were performed by Victor Horsley, and went far to prove its correctness, though it was evident that the

impulses thus engendered cannot be the only ones reaching the respiratory centre in the medulla, and that this may also be influenced, so far as the larynx is concerned, through other afferent impulses.

Again, in 1900, a demonstration of the most important of our results concerning the representation of the larynx in the central nervous system was given by Horsley and myself before the combined Sections of Physiology, Neurology, and Laryngology of the International Medical Congress of Berlin. This demonstration resulted in a perfect ovation for Horsley, whose skill as an experimental physiologist was thus acknowledged by the most competent tribunal in the world. A short *exposé* of our results was given under the title "Of the Relations of the Larynx to the Motor Nervous System" in the *Deutsche Medicinische Wochenschrift*, No. 31, 1890.

Finally, the result of the greater part of our experiments during the preceding four years was published in the *Philosophical Transactions of the Royal Society*, vol. 181. The facts communicated in that paper did not, by any means, exhaust the material we had already then collected, and we intended to return to the subject. But when we resumed our studies in 1893, and when I found that their object was mainly to examine the results of descending degeneration following ablation of the higher parts of the central motor apparatus, I insisted, in spite of Horsley's friendly remonstrations, in withdrawing from an investigation which was above my ken, and in which I felt I had become more a hindrance than a help. That part of our task, therefore, was left unfinished.

Once more I had to have recourse to Victor's friendly services, in 1897. I had been denounced by Dr. Grossmann, of Vienna, for not having repeated Dr. Wagner's experiments on the position of the vocal cord after section of the recurrent laryngeal nerve. The charge was frivolous, for every experimenter, including Horsley and myself, who had performed that experiment and had published his work, had arrived at results totally different from Dr. Wagner's. To avoid the obvious retort, however, that we had neglected to test Dr. Wagner's statements, it became necessary to repeat these experiments. This Horsley did for me, and the outcome, a sample of which was fixed by photographic representation, utterly negated Dr. Wagner's results.

With this Horsley's work for laryngology ended. It represents an achievement for which our specialty can never be grateful enough to him. The foregoing small tribute does not nearly exhaust the amount of my personal indebtedness to him for thirty years' unbroken friendship, for unceasing encouragement, as well as for invaluable services rendered to my family. But it shows, I hope, that among the small number of general physicians, surgeons, and practitioners who supported British laryngology during its struggling period between 1860 and 1890, a place of conspicuous honour must be reserved for our friend, the late lamented Sir Victor Horsley.

Felix Semon.