BRITISH MEDICAL JOURNAL

LONDON SATURDAY APRIL 20 1957

SIR VICTOR HORSLEY, 1857-1916 CENTENARY LECTURE*

BY

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A hundred years ago was born a man who would bear one of the most famous names in surgery. What may have been the ambitions of John Callcott and Rosamund Horsley for this third son we cannot know. It is most improbable that they could have seen him as a colossus of scientific medicine. Very likely they thought of him as a future artist of some kind—a painter like his father, already an Associate Royal Academician. Or perhaps he would be like his grandmother's brother, the Sir Augustus Wall Callcott whose landscapes were valued only after those of Constable and Turner and are still known to collectors. Or perhaps, again, he might be a musician like his paternal grandfather, William Horsley, one of England's most famous composers of glees and rounds, the friend of Mendelssohn, who thought highly of these compositions. Victor's father later became a close friend of Felix Mendelssohn. A relation, Lady (Maria) Callcott, was the authoress of *Little Arthur's History of England* (1835), a book so perfectly attuned to the spirit of the times, so edifying, that it was a best seller for more than one generation. It was into this family with its strong artistic associations that the young Victor came.

Antecedents and Social Background

His father was known to the Prince Consort through his designs, one a winning one, for the new Houses of Parliament. Also Miss Scarratt, the Queen's companion, was a close friend of the Horsleys, and this strengthened their slender Palace association. Miss Scarratt had only to come across the announcement in *The Times* of the birth of a son to that family when reading the news to Her Majesty (confined on that same April 14, 1857, the year of the Indian Mutiny, by the

birth of her daughter Beatrice) for there to ensue a discussion from which the names Victor Alexander emerged as those the Queen would like the Horsley boy to have. One year later Horsley senior was commissioned to paint the Princess Beatrice, and Victor was brought to the Palace for the Queen's inspection. Victor was inspected, as it were, by a host of people afterwards and impressed them by his rather deep-set eyes, wide forehead, his pronounced features, by the energy and alertness of his manner, quickness of decision, and the indefatigability of his frame.

His father before him had been exceptionally gifted and good-looking. The snapshot portrait of John Callcott Horsley which graces his *Recollections of a Royal Academician* (1903) shows a figure at ease in an outsize silk hat, braided cut-away coat and trousers—a figure elegant: one recognizes at once a man economically and socially secure. According to Stephen Paget, Victor's father was friendly, impulsive, hot-tempered, and restless. Like his son after him, he too had a crusade. This was the result of his once-famous address to the Church Congress at

Victor Horsley

[Swaine, London]

Portsmouth in October, 1885, protesting against the use of the nude model, especially when female, in art schools. His address was called "Art Schools and Art Practice in Their Relation to Moral and Religious Life." "Where," he said, "is the justification in God's sight for those who induce women so to ignore their natural modesty and quench their sense of shame as to expose their nakedness to men and thus destroy all that is pure and lovely in their womanhood?" He himself, though greatly dependent on models, had never required them to remove their clothes. This address, *The Times* said, caused a painful sensation. No doubt that was the speaker's intention, but it brought on his head the same sort of criticism, badinage, and hostility

*Delivered at B.M.A. House on April 12.

that would come his son's way in due time. There were those who held that John Horsley would have been a better painter if he had been a better anatomist. Amongst his critics was Whistler, as can be read in *The Gentle Art of Making Enemies*. But make no mistake about it, Horsley was a sincere and highly competent painter. It was deep Christian reverence and concern for the welfare of the model that moved the father to protest against the nude.

Religion, by contrast, was not the basis for Victor's crusades, for he was an agnostic, a Huxleyite, but, like his father, a reformer. The motivations of Victor's anti-alcoholic crusade were physiological: alcohol was a toxic substance, a dangerous drug that was too freely available. It was an unpopular thesis, but not for that the less true, especially at that time.

This much has been said about Victor Horsley's parents and forebears because it is necessary, if his work is to be recalled and reappraised, that we should know not only his antecedents but the social background against which he grew to manhood. Social security he evidently had. What he had inherited from his Northumberland artist ancestors might well have been creativeness, inventiveness, and energy. As to this last, we may well be astonished at the great amount of scientific work of quality which Victor Horsley so soon achieved. By the time he was 30 he was well known in Berlin; in another ten years he was a world figure.

Personal Qualities

It seems that Victor Horsley's teachers, and later his fellow students, quickly realized that they were in the company of a person quite exceptionally talented. He learned without effort, he had an exceptional memory, he was skilful with his ambidextrous hands, he had daring, he had energy, he had integrity—in short, he had from early days the qualities of leadership. He shone even in such intelligent company as that group who entered University College Hospital as students with him in 1875—Gotch, Mott, Beevor, Dawson Williams, C. J. Bond. He qualified M.R.C.S. in 1880, M.B., B.S. in 1881, when he was 24, winning the gold medal in surgery with a London University Scholarship. He was elected surgical registrar next year, and took rooms in Gower Street—as Stephen Paget reminds us, in those days the registrar was permitted private practice, though history does not say with what success. However, as Paget charmingly remarked, "Gower Street had been helpful to many young physicians and surgeons of the hospital by making them comfortable till they could make themselves eminent." He was appointed assistant professor of pathology in 1882; in 1883 he was admitted to the F.R.C.S., and the following year was appointed Director of the Brown Institution, where so much of his experimental work was to be done. Horsley became assistant surgeon to University College Hospital in 1885; five years later he was full surgeon, remaining so until he retired from the staff in 1906.

He continued his hospital work at the National Hospital, Queen Square, while he pursued his researches still at University College. He must also conduct his very considerable private practice, operating at what I am told was a singularly comfortable and efficient nursing-home, 9, Mandeville Place. This, in short, is the official story of his life in the barest of outlines, yet the synopsis does mention some things that we still wish to inquire into. What, for instance, was this Brown Institution, and what can have been the pressures or possibly the ambitions that made a man already internationally famous retire from one of London's premier teaching hospitals when he was only 49? He had been elected a Fellow of the Royal Society at the age of 29. By what means had he won the prestige that was so liberally allowed him in the world of science?

It is this story on which I particularly wish to dwell. His researches gradually came to show that the anatomy on which a surgeon's training is traditionally based must have life breathed into it by physiology. No clinician has ever conducted experimental laboratory research with such energy and without serious interruption over so long a period of years. We shall most admire in him not only his tenacity but his unwillingness to be halted by the technical difficulties that almost all neurophysiological problems present.

The Brown Institution

An excellent short account of the history of this Institution has been given in Stephen Paget's *Life* (1919). The Institution no longer exists, having been destroyed in an air-raid in 1944 at a time when Dr. Twort, famous, like d'Herelle, for the discovery of bacteriophage, was its director—its last. The Institution was built by the University of London, which had accepted a bequest from a Mr. Thomas Brown, of Dublin. The objects were to study and attempt to cure maladies, distempers, and injuries of quadrupeds and birds. The University founded it with these ends in view, but, at the same time, with great wisdom and foresight made it a centre for advanced research in physiology and pathology. The names of its successive superintendents are a sufficient guarantee of the quality of the work done there—Burdon-Sanderson, Greenfield, Roy, Horsley, Sherrington, Rose Bradford, Brodie. Sherrington's own appointment came, as he once told me, like manna from heaven, because the small salary attached as well as the living quarters there allowed him to marry. But there was something more—this Institution was the only place in those days at which recovery experiments could be done on animals of any size, and this had widened Horsley's scope greatly.

It seemed odd to many—it drove anti-vivisectionists to fury—that this institution for the welfare of animals should be used for experiments upon them.

Victor Horsley's tenure of the professor-superintendentship (1884-1900) was signalized with his first experiments on the nervous system, on the thyroid and pituitary glands, on myxoedema and infections in animals, on rabies and hydrophobia. During this period he performed and reported his first group of brain operations at Queen Square (1887) and had recorded with Gowers a world-famous case—the first successful recognition and removal of a spinal-cord tumour (1888).

Early Researches—the Pituitary

The account of Horsley's thyroid researches given by Stephen Paget, which were links in the chain of the cachexia strumipriva and myxoedema stories, is so complete that we may usefully pay attention to something else—the other ductless gland that attracted him, the pituitary. In the paper which he wrote with Handelsmann in 1911, "A preliminary note on experimental investigations of the pituitary body," the following sentence appears concerning partial or complete removal of the gland: "The first to perform such experiments was Horsley in 1886, after whom a series of authors (about 20) repeated the experiments with results which were not uniform." In some investigations the animals succumbed soon after the operation, in others they survived for long periods. Reference was made to the work of Cushing and his collaborators leading to the establishment by them of the experimental condition known as cachexia hypophyseopriva. This clumsy title served a useful purpose by drawing a parallel between the effects of total loss of the pituitary and of the thyroid. Cushing concluded from his own experiments that the pituitary was absolutely and urgently necessary to life, animals dying in a state of lethargy and drowsiness, slow respiration, lowered temperature after three to five days in adult dogs, 10 to 30 in puppies. Horsley criticized this statement and brought in proof three animals—a cat, a dog, and a monkey—which "survived a long time" in spite of proved complete pituitary extirpations.

In all, Horsley and Handelsmann reported experiments on 54 animals (20 cats, 21 dogs, and 13 monkeys). Some of the survivors were shown at a meeting of the Physiological Society in March, 1911. The gland had been removed by either the temporal or palatal route, the latter only in the cat. Incidental damage to other intracranial structures was shown in about half the animals by hemiparesis, hemianopia, and, in monkeys, by temporary ipsilateral ophthalmoplegia. This last is a possible disadvantage of the temporal route: third-nerve palsy occurred, as we shall see, sometimes in his human pituitary operations. In 15 animals (2 cats, 9 dogs, and 4 monkeys) the gland was totally removed. In a few others some fragments remained, but, unlike Cushing, Horsley did not include those cases, and thereby missed making some useful observations on chronic hypopituitarism. One of their animals was killed on the 115th day, though in good health; one monkey survived to the 39th day. No important changes were observed in the other organs of the animals that died naturally. They "died of asthenia as animals in captivity do." He did not know that the cause of death might have been adrenal or polyglandular failure. Glycosuria occurred in two out of seven examples where it was specially looked for.

Horsley remarked that he had cut the pituitary stalk in some experiments without causing death. This important paper is sadly lacking in the details that we should like to know, and the figures quoted in one place do not always tally with others elsewhere. Nor is it clear how much of this publication was based on the old work of 1886 or the horizon enlarged by recent experiments. However, the promise was made that Handelsmann would publish all the data in detail, but, so far as can be discovered, he never did. Turning back to the 1886 papers, we find the abstracts of the Brown Institution lectures in the *Lancet* are very bare. Whilst he says that "the cause of myxoedema and cretinism had been shown by Horsley to be due to the loss of the thyroid gland," there is little else exciting except the statement that he had twice successfully removed the pituitary and that the animals had lived five and six months respectively. During life no nervous or other symptoms were noticed. The excitability of the cortex was found to be raised in hypophysectomized animals; and that reminds us that the monograph of the brothers Wenzel (1811) on the association between epilepsy and the pituitary gland was almost the only known "fact" about it 75 years ago.

It was not until 1895 that Oliver and Schäfer discovered the unexpected properties of posterior lobe extracts, and not until 1900 that Benda demonstrated the acidophil adenomatous basis of Marie's acromegaly (1886). What this amounts to, in brief, is that when Horsley was in 1886 experimenting on the pituitary gland he was assisting at the birth of endocrinology, but the endocrines and steroid chemistry lay too far ahead in time for him to be able to see much more clearly what the uses of pituitary were than Claude Bernard had done. Horsley established the fact that the gland could be removed and that rapid death was not necessarily the result. And in this conclusion Horsley has been fully supported by large-scale experimenters. Animals deprived of their pituitaries do not die in a few days; their expectation of life is reduced by about a half. No urgent disaster faces them; Cushing was wrong about that. It will be recalled that P. Bailey and Bremer eventually showed that many of the supposed sequels of the hypophysectomies of Cushing and his school were due to damage to the floor of the third ventricle.

Pituitary Tumours

This may be a convenient place to introduce the subject of Victor Horsley's operations upon pituitary adenomas. It has often been complained that he left no full account (which is true), though he is known to have given lectures about the pituitary and its surgery; for example, Sheffield, 1910. What he may have said is not to be discovered. However, it is now possible to get a sufficiently clear

picture. Four of his cases are given briefly by H. H. Tooth in his report on brain tumours to the 17th International Medical Congress (1913) in London. In 1906, at Toronto, Horsley mentioned 10 pituitaries operated on by him—the cream, however, is contained in Verga's monograph (1911), and I am most grateful to my old friend Dr. Ernest Sachs for directing my attention in this direction. This book from Pavia on the pathology of pituitary tumours contains copies of the notes either at the National Hospital, Queen Square, or from Horsley's private files. Here are to be found the records of 10 patients and references to two more; of these eight recovered and four died. One private case came from South Africa, one from Chicago. With the assistance of Mr. Julien Taylor and Mr. John Hankinson, registrars at Queen Square, who helped in searching through the old notes, some of these cases were found. It is possible, therefore, to reproduce a description of the operative steps from one of these. I referred to this particular case in a former publication (Jefferson, 1955), but at that time it could not be identified.

William Knapp, aged 45, admitted to Queen Square, under Dr. Risien Russell, notes by house-physician, Gordon Holmes, and by clinical clerk, Grainger Stewart. The patient complained of severe pain in the left face and of loss of sight in the left eye, which he was unable to open. Temporal hemianopia was present in the other eye. He had been in hospital before, but was sent home to return if the pain got worse. The first symptom two and a half years previously had been drooping of the left upper lid, which progressed during the next 15 months to closure of that eye. In 1903 he began to suffer from "giddy turns" in which his wife said he had a vacant look—probably minor epilepsy. He began to have pain in the whole left trigeminal area, severe and somewhat paroxysmal, and now upsetting the patient badly. No sensory changes were noted in the left trigeminal distribution. The third nerve was paralysed but the sixth was not. His sight had deteriorated so that he could no longer read. V.A.R. 6/36, V.A.L. nil; bilateral optic atrophy was present.

December 2, 1904.—Operation first stage—Sir Victor Horsley. A large semicircular scalp flap was made from slightly behind the left external orbital process to the base of the mastoid, convex upwards. There was not much bleeding. An area of bone was then removed from the left lateral surface of the skull by trephine, saw, and bone forceps. The dura was found to be tense, but was not incised. The patient stood the operation very well.

December 6, 1904.—Operation, second stage. The flap was turned down and the dura incised and finally cut away where it was exposed. The brain did not bulge to any great extent nor did the convolutions seem flattened. The brain was then slowly lifted up by retractors placed under the temporal lobe, pressure being only slowly exerted so that the brain could mould itself to its altered position. In this way it was soon possible to see into the region of the sella turcica. There was not much haemorrhage, but what there was was controlled by hot irrigation.

An attempt was then made to cut the sensory root of the fifth nerve, but in doing so the superior petrosal sinus was injured and had to be compressed. Attention was next directed to the pituitary body; the dura in the lateral surface of the sella turcica appeared bulged out, and it was then incised by a knife; some tumour mass at once protruded and the greater part of it was then scraped out with a sharp spoon; it came away in small pieces.

A small rhinoscopic mirror was then inserted into the sella turcica so that it might be seen if all the tumour was removed. The small amount of bleeding which followed the operation was easily checked by hot irrigation, and a gauze plug was finally inserted, which reached down to the sella turcica.

(Horsley spoke in 1906 of inserting sometimes this small laryngological mirror into the capsule, and said what an excellent view he got of the second and third nerves, of the crus cerebri, and of the cavernous sinus. With a good headlight he could see the interior of the sella well.)

The patient stood the operation very well. An hour later he was quite bright, said he was quite comfortable, and moved both right and left limbs freely. There was, however, slight paraphasia, the use of wrong words, and occasionally some difficulty in finding words to express himself. He was discharged home on January 12, 1905.

The operation evidently failed to cure this patient's pain, and in March, 1905, we find him back in hospital to be operated upon again, this time for the removal of his left Gasserian ganglion. The notes are very brief.

March 28, 1905.—The previous trephine opening being utilized, Sir Victor Horsley removed the ganglion in the usual way by Krause's method. The fold of the dura covering the ganglion was raised up and incised. The sensory root was drawn from the pons varolii and the ganglion dissected forwards and removed after the nerves had been cut. There was not much haemorrhage. The lids were sewn together at the close of the operation. He lived for eight years.

Horsley's Gasserectomies

This case gives a chance for a brief comment on Horsley's operations for trigeminal neuralgia. Morriston Davies (1907) wrote an admirable paper on the sensory changes in the face as studied in Horsley's 48 cases. By the time of Horsley's death he had probably done more gasserectomies than anyone else, and was well known to share with Krause the reputation of being quite exceptionally expert at it. Observing with what ease apparently, in the case of the patient Knapp, Horsley reversed Krause's method and pulled the sensory root off the pons before he cut the divisions, the reader must wonder why he was not content with the root avulsion alone—why it was left to Spiller to suggest this as not only enough but sufficient. And yet did not Horsley (Horsley, Taylor, and Coleman, 1891) propose and carry this out when he went through the tentorium and cut the nerve in the posterior fossa? The patient, a feeble old lady, died post-operatively. What a pity it was that the case had that end, for otherwise he might have developed a good technique. It must be confessed that without the help of electrocoagulation the transtentorial operation was beyond the resources of Horsley's time as a routine procedure.

Any neurosurgeon reading through the accounts of Horsley's operations to-day will recognize that the operator must have been extremely dextrous. Why—we may repeat a question that has often been asked—why did Horsley use the temporal approach to the pituitary rather than the frontal that everyone used after him? The answer can be guessed fairly accurately. The most important influence must have been his experimental work at the Brown Institution. In the dog and monkey the temporal route is the natural and easiest way in. Familiar with this approach in the animal, it would be natural for him to follow a path that he knew so well. Then there is something else. He had evidently had a nasty experience with the frontal route. It is mentioned in his Toronto Address in Surgery (1906). He said that in 1889 he had been asked to operate on a tumour pressing on the front of the optic chiasma. It proved to be a "cystic adenocarcinoma." In order to get access Horsley tied some of the frontal veins so that he could displace the hemisphere. After the death of the patient some years later it was found that considerable softening of the frontal lobe had occurred. I imagine that a good deal more softening than he knew followed Horsley's elevations of the temporal lobe, but he was careful not to ligate Labbé's vein.

The impression obtained from reading the operation notes reprinted by Verga and from Horsley's own comments on technique is that he was an extraordinarily gentle, skilful, and knowledgeable surgeon. Certainly the testimony of his surgical pupils is that he was unsurpassed. It was the speed of his operating that shocked Cushing, bred in Halsted's school.

Experimental Neurology

Horsley had two main collaborators in his neurological work—Charles Beevor and Richard H. Clarke. Important experimental work was carried out also with his sister's husband, Professor Francis Gotch, with Sir Felix Semon, and with A. E. Schäfer. Some he had done by himself. Eight papers were printed in the *Philosophical Transactions of the Royal Society* between 1886 and 1891. The last of these was the Croonian Lecture presented by himself and Gotch on "The Mammalian Nervous System, its functions, and their localization determined by an electrical method." This paper is 260 folio pages long, and has to do with the

galvanometric pick-up of impulses in the cord and nerves set going by either central or peripheral stimulation. Thus, to take one example, stimulation of the motor cortex gave rise to electrical effects chiefly in the opposite half of the spinal cord, a little on the same side. Would it not be a good critical test if the spinal cord was vertically split for two or three inches at its lower end into two halves so that the electrical changes could be more easily kept separate? Very well—it was done and the result confirmed (Fig. 1). Again the sciatic nerve was stimulated and electrical impulses were picked up in the spinal cord and traced by dividing different spinal tracts. The cord itself was stimulated and the resulting discharge followed along it. All this was pioneer work of the first order and a huge undertaking; the results were accurate within the mechanical error of the recording instruments then available. It remains to this day highly important work.

The experiments with Beevor were mainly refinements of the experiments which Ferrier had done on the motor cortex. There is no room for mention of more than three: (1) the exploration of the motor area on the medial surface of the brain; (2) the mapping of the excitable fibres in the crus cerebri and in the internal capsule both in the macaque monkey and in the orang outang; and (3) the plotting of the motor cortex on the latter animal (1890), the first time that a primate had been so investigated.

A description of the furrow formation in the orang is given and the motor points are recorded by a series of 27 photographs—an expensive method, but one leaving no topographical doubt in the reader's mind. Horsley was struck by the similarity of the results of stimulating the orang's brain with what he had already seen in the human being and with what W. W. Keen had reported in his case of cortical excision for epilepsy (1888). With Semon, Horsley sought the cortical motor innervation of the larynx. They went on to explore the floor of the fourth ventricle, a structure that had not been explored electrically previously to 1889, when Horsley had first done this. Sharp closure of the glottis invariably followed stimulation of the calamus scriptorius, but higher up abduction of the vocal cords was the rule (Fig. 2). In parenthesis, no observations on consciousness could be made because the animals were anaesthetized.

As Horsley grew older his experiments, as might be expected, grew in maturity of thought and presentation. We may single out the work he did with Colin Russel, Otto May, and Arthur MacNalty, and with his second great collaborator, Richard Clarke. This work had to do with the cerebellum and brain stem. Moruzzi (1950) has said that the discovery by Horsley and Lowenthal as well as by Sherrington, that stimulation of the anterior median cerebellar cortex inhibited decerebrate rigidity, equalled in its revelation of cerebellar function the discovery of the excitability of the motor cortex.

FIG. 1.—From *Philosophical Transactions of the Royal Society*, 1891.

We may pause a moment here to comment on the excellence of the illustrations in Horsley's papers. The photomicrographs are of admirable quality; they were made by him and his groups at U.C.H. or else at his home, 25, Cavendish Square, in the evenings. Also in his papers with May and MacNalty we may well admire the operative dexterity which allowed him to make small cuts into the mid-brain after sliding in above the tentorium or excursions elsewhere with so many survivals.

The cerebellar discovery came about as part of the investigation of the relative significance of the cerebellar cortex and its deep nuclei, and the inquiry whether any efferent

FIG. 2.—Sites on floor of fourth ventricle for movements of the vocal cords. Semon and Horsley. (From *Philosophical Transactions of the Royal Society*, 1890.)

tracts left the cerebellar cortex to enter directly into the spinal cord or brain. Lesions of the cerebellar cortex were traced by Marchi's method; no such fibres were seen. On the other hand, it was difficult to damage the deep cerebellar nuclei in a precise way without injuring the cortex as well. It was to solve this particular technical problem that R. H. Clarke set to work to produce what came to be known as the Horsley-Clarke stereotaxic machine.

Richard Henry Clarke

And here we should say a few words about this remarkable man. Clarke, who was born in 1850 and was therefore the older of the two, had been at Cambridge and taken his M.B. at St. George's Hospital—in his day and later a medical school which received a disproportionately large number of Cambridge men. Clarke must have had private means; he never practised, but was well known as an expert all-round athlete—cricketer, golfer, first-class shot, fox-hunter, and judge of a horse. He may have been a shade hypochondriacal, because he spent, Dr. Sachs tells me, some years in Egypt curing himself of some chest trouble. All who knew him agree about his popularity and say that his gifts as a sportsman were signs of his extraordinary skills married to a first-class intelligence—in short, that he could do anything—so that if anyone could have invented an accurate piece of machinery Clarke would be just that man. He had demonstrated anatomy in Glasgow and physiology at St. George's (1885-7), then worked for many years with Horsley

in the laboratories at University College. It was there that the first square apparatus was finally completed and later a second oval version made.

The stereotaxic instrument is one of the most beautiful tools ever introduced into a physiological laboratory. These instruments were admirably described and illustrated in long papers in 1908 and 1912, and again in the Baltimore (1920) monograph that Clarke finally published with E. E. Henderson from the Laboratory of Pathological Chemistry, University College.

As Clarke said in 1920, without such help it was "impossible to expose the deep ganglia without serious mutilation, haemorrhage, and shock, which not only hinder the attainment of our object but rob it of much of its value when secured." The principles of the instrument are as simple as they are ingenious. Clarke realized that it would be hopeless to base measurements on the shape and external surfaces of the brain and skull. They were too variable. But if one took the equator and meridians as zero planes and measured the brain from inside out much greater accuracy should follow. The sites of deep nuclei and tracts were calibrated by comparison with slices of the brain of cat, dog, and rhesus monkey 2 mm. thick cut in a frame most efficiently designed to ensure complete accuracy. The serial photographs were published in the papers named. Most unfortunately the real results of their electrolytic lesions and deep stimulations were promised but not published. Descriptions of this revolutionary instrument being necessarily very lengthy, they absorbed all the available space.

Something new has, however, recently come to light, for has not Professor Robin Pilcher found in the Department of Surgery at University College Hospital a bound thick folio volume that contains details, including many photographs of the brain, from Horsley and Clarke's cerebellar experiments during 1906? It covers some 70 of the early electrolytic lesions and stimulation of cerebellar cortex and deep nuclei. There is a gap in the dates from May 4 to October 29, 1906, when a rhesus monkey's cerebellum was stimulated. In the right hand at the top of the paper is written in Horsley's hand, "First time of using new instrument and mode of record." It is probable that the new plotting of small squares in the animal's head was under trial before this date. Just now no further comment can be made than to say that most of the stimulations produced movement of the eyes or pupillary changes if they produced anything. One thing might be briefly singled out—stimulations of the posterior longitudinal bundle in the floor of the fourth ventricle. In a cat, on March 29, 1906, they observed twitches of the face and not only conjugate movements of the eyes usually to the same side but, further, sometimes movement of one eye alone and to the same side. This was very new indeed, a refinement on the experiments made with Semon in 1889 and already mentioned.

In Clarke's monograph written after Horsley was dead he permitted himself to make some very unflattering observations on brain surgery. He thought it mutilating and a most unsatisfactory pursuit. If Ferrier's estimate of 7% of cures was the best one could hope for, he would be a strange fellow, Clarke thought, who could call this a triumph of surgery. This acerbity is unhappily a sign of the estrangement that had grown finally between these lifelong friends, naturally grieving the friends of both. Clarke had long been the adored friend of the Horsley children. His bearded face, his twinkling blue eyes, his humour, and his ghost stories captivated them. He had for years been the constant companion of the family.

Clarke claimed that really elegant results could be produced by his machine—he hoped that even brain tumours could be cured by electrical means or by the placement of radium. Besides this Clarke thought that it should be possible to relieve pain better than in any other way by coagulation of intracerebral tracts—all these things through a 5-mm. trephine hole. And he was sure that time would show that there were other ways in which the stereo-

taxic instrument could achieve results far in advance of anything then available. Time would show. Allowing for some very pardonable special pleading, Clarke was right and time has vindicated him. His was very real prevision.

Although it is only too clear that Horsley's part in the instrument's actual design was negligible, it was he who had conceived its necessity. It is not too much to say that Clarke would never have thought of stereotaxis had it not been for Victor Horsley's curiosity about the roof nuclei of the cerebellum and his dissatisfaction with the method which he and Clarke had had to use in 1905 and earlier to explore the intrinsic fibres of the cerebellum.

In his flattering but no doubt well-deserved obituary notice on Clarke, Sir G. R. Turner (1926) said, what many must have thought, that this modest man did not receive the credit for his many researches that he deserved. He had left behind a manuscript on the divisions of the cerebellum and their relation to function. It is unpublished still. Time, that proved him right in his foresight of the uses of stereotaxis, has done him much but perhaps not yet enough honour.

FIG. 3.—Photograph of a traumatic epileptic with the coronal sutures and the cerebral fissures drawn on the skull.

FIG. 4.—Sketch from the operation notes of same case as Fig. 3. Queen Square, 1886.

good marginal sketch from the operation notes shows the state of the traumatic epileptic (Figs. 3 and 4). The tumour from another case in the first series operated upon immediately after the Brighton Meeting, although grossly it looks benign, was proved to have been malignant when the patient succumbed four or five months later.

Victor Horsley as a Neurosurgeon

It should be very plain that no neurological surgeon has ever turned his hand to the practical therapeutics of nervous diseases so well prepared by laboratory experimental work. And none has, as yet, continued, as Horsley did all through his life, to spend so much time in devising and conducting new experiments.

The pleasure felt by Victor Horsley can well be imagined when in August, 1886, he took down to Brighton for the 54th Annual Meeting of the British Medical Association three patients recently operated upon by him. All were examples of Jacksonian epilepsy (two traumatic and one a tumour) and all thus far successful. A strikingly

Charcot, who was present at Brighton, congratulated Horsley, adding how clearly clinical experience was illuminating the doctrine of cerebral localization as arrived at by animal experiments. Hughlings Jackson, in a characteristically brilliant commentary, ventured that all epilepsies were due to small lesions could they but be found. He said further and most wisely that he felt sure that it was important to cut out more than the lesion "which produced fits by altering the portion of the brain in which it was seated." Horsley had in fact done exactly that. Fortified by his recently acquired familiarity with operations on the animal's brain, he did not hesitate from the first to excise portions of the human cortex. Many surgeons—and in those days remember that all were general surgeons—were encouraged by Horsley's novel human experiments to operate for epilepsy and to report successes after removing trephine disks and bone splinters from old cranial injuries or supposed injuries; they did not often open the dura. Few except W. W. Keen did what Horsley did.

Unfortunately Horsley, except in his famous Linacre Lecture, never reported the later results of his cortical excisions for epilepsy or athetosis. That he made a considerable number is certain. The Surgical Reports of University College Hospital lent me by Professor Pilcher show that in the years 1900-6 30 cases were operated upon—more would be done at Queen Square—yet we do not know how they fared. Horsley published his first 10 brain operations in 1887, and in 1888, with Gowers, the account of the first correct localization and removal of a spinal tumour, the patient recovering from paraplegia. Naturally this case caused a stir. Two major events in combined neurology and surgery had now occurred—the localization and display at operation of both a brain tumour (Hughes Bennett and Rickman Godlee, 1884) and now a spinal.

Brain Surgery

No time shall be lost in discussing the real priority of the first case.

I have already dealt with that matter in a discussion of Macewen's neurological surgery (Jefferson, 1950). It is only fair to allow Macewen to speak again now, taking his words from the discussion on brain surgery at the International Medical Congress (1913). Macewen said: "Mr. President, I believe it was somewhere in the 'seventies when you visited my clinic in the Glasgow Royal Infirmary and asked me three questions—whether I had ever diagnosed a lesion of the brain from the localization of symptoms alone, and, if so, if I had operated upon such a case and had found a lesion in the part where it was expected to be, and, thirdly, if any of those cases so operated upon had recovered, and I was able to answer each of those questions in the affirmative." The logic of this sequence is perfect. It is the story of brain surgery. What was better was that Macewen was perfectly correct in what he said. He proceeded to say that if he were asked what most needed development, it was the physiology of the brain apart from the motor area. How right he was! But in fact it was just these subjects which had been engrossing Victor Horsley in his laboratory for some years, and, latterly, increasingly so. Perhaps it was these preoccupations that prevented Horsley from giving what I imagine was expected of him—a full account of his own experience of brain tumours. Disappointingly, whatever he may have said, the published report is maddeningly brief.

Percy Sargent gave an account of his first 100 cases, with 14.5% recovery in non-comatose patients. Byrom Bramwell spoke of his 225 cases, 42 operated upon, Krause his 154 tumour operations; and von Eiselsberg analysed his 166 cases, including 16 pituitary tumours. He concluded by saying: "Sir Victor Horsley geprägt hat: 'Bei Hirntumoren nicht zu operieren ist inhuman.'" All that Horsley had to say in effect was that it was unnecessary to talk about technique to surgeons—haemorrhage was unimportant, for it was easily controllable, "if it were capillary hot water stopped it, if it were venous then oxygen

stopped it, and if it were arterial the judicious use of chloroform would suffice to lower the blood pressure and so put an end to the haemorrhage." Too simple? Maybe. But it is a physiologist speaking. And, interestingly enough, the modern tendency in neurosurgery is, by means of deliberate hypotension, to return again to the advantage of chloroform anaesthesia without most of its dangers. Horsley, like MacEwen, made a demand on the future; what he thought was most needed was a proper understanding of the structure and histological types of brain tumours. That was to be Cushing's main and lifelong preoccupation for which he needed much help, notably, predominantly, from Percival Bailey.

Possibly Victor Horsley at that 1913 Congress thought that Howard H. Tooth's very complete analysis of 500 cases that had come under observation at Queen Square in the years 1902-11 inclusive made it unnecessary for him to make any large communication. Of the cases operated upon—and only half the number were—most had been attended to by Horsley, and the immediate mortality and the survival periods of the patients were recorded. Tooth's was a major work of the first class, and nothing comparable appeared again until W. B. Van Wagenen (1934) and then Hugh Cairns (1935) each analysed the results of the cases of a different single year in Harvey Cushing's service.

The two things which most impress the modern reader when reading through the Queen Square material are: first, the oversimplicity of the pathological diagnosis, and indeed the now patent error of much of it; and, second, the frequency of death from shock after what should have been a highly successful tumour removal. If Horsley had had blood transfusion and blood banks the cases would have had happier endings. Then there was the sad frequency of infection that was a killer. Its incidence was multiplied and became more agonizing still during the first world war. It was 30 years after the date of Tooth's paper that intracranial infection began to come under control. Another failure was the incorrect diagnosis, the mistakes in localization, chiefly in the case of frontal and cerebellar tumours. This difficulty that had serious results was not resolved for many years, and then only by mechanical diagnostic means, not by neurological refinements. Horsley (1906) pointed out the risks of an incorrectly placed attack. Of 79 cases with a correct diagnosis, the post-operative mortality was a little over 8%, but of 16 where the localization was wrong the mortality was 37%. Notwithstanding, many of Horsley's patients were alive and well seven, and eight years afterwards.

Horsley had one bee in his bonnet—that papilloedema (Parsons's new name for optic neuritis) always first appeared on the same side as a tumour, and hence had great localizing value. Leslie Paton and Gordon Holmes failed to persuade him of his error. The rest of his work on the nature of papilloedema was first-rate; he was ahead of Hughlings Jackson and Gowers there, having at once accepted the mechanical theory of Manz and made excellent preparations of Schwalbe's sheath and its ampulla. He illustrated his papers with photographs, taken by himself, I suppose, with the German camera that Sir Arthur MacNalty tells us of. One other piece of surgical pathological work should not be overlooked. It was the experiments that he had carried out with Walter Spencer in 1891 on the mode of death in cerebral compression and its prevention. They for the first time used the mercury-filled bag to raise the pressure and noted the same effects on respiration and blood pressure that Harvey Cushing reported in his much more interestingly written paper. Cushing did not refer to the Spencer and Horsley paper in the *Philosophical Transactions of the Royal Society*, which is fully documented, but to a later popular version.

Mention has already been made of Victor Horsley's dexterity; it is impressively evident in the description of his survival experiments on animals. He had stimulated the fifth, seventh, ninth, eleventh, twelfth, and upper third cervical nerves in the monkey; working with Otto May,

it is mentioned that in three cases Horsley had cut the fibres of the hypoglossal nerve off its nucleus by slipping a knife under the tubercle of Rolando parallel with the floor of the fourth ventricle. The animals recovered. Little wonder that Sir William Osler (1919), reviewing the *Paget Life*, said that Horsley was the outstanding surgeon of his generation, that he had acquired a technique hitherto unequalled.

The Man

The work and writings of a man can be properly assessed by anyone whether he has been personally acquainted or not. But to speak of a personality from hearsay is dangerous and almost impertinent. That Victor Horsley was a giant amongst men I have no doubt at all. That he excited admiration and devotion in those who worked with him in their youth can be confirmed because several of them are still alive to testify. Outside his scientific work his reputation was in some directions less secure. Stephen Paget dealt with this with fine discrimination and honesty. Perhaps in his fifties his creativeness was at last on the wane. Stephen Paget quotes him as remarking without complaint that his mind did not "boil with ideas" the way it once did. But that was late. Until then his life had been a fury of research and practice, hospital and private; on top of all were his crusades, first and always the famous anti-alcohol affair, later against treating the suffragettes as criminals, and finally politics, medical and national. He was a Liberal, but nowadays would surely have been a Socialist.

It is not greatly to be wondered at that Harvey Cushing found little to admire and nothing to imitate in this great Englishman. Their lines of interest diverged too far. Horsley's preoccupation was in the intimate structure and physiology of the nervous system. Cushing's was not, it was more practical. Horsley grew up a young man in the company of the great founders of clinical neurology: Hughlings Jackson, David Ferrier, William Gowers, C. K. Mills, Spiller, and Weir Mitchell in America; Sherrington, Gotch, Schäfer were his contemporaries. Horsley could bring to bear operative skills that these men did not have, and so he came actively to explore functions—and that, be it remarked, when 99 out of 100 surgeons thought that anatomy was the only proper study for a practical man. Horsley's eye was on the future. The details of operative surgery Horsley thought important, but not important enough to deserve much publication. Cushing, on the other hand, was much more interested in the immediate present, in technical improvements in a field which was ripe, if mortality was to be reduced and the rate of cure raised, for a change in method. His great gifts were better canalized than Horsley's, but had he been more generous to the older man it would have been more seemly and more just.

Horsley never, it seems, worried overmuch about what people thought of him, nor about the collecting of honours. He had work to do, absorbing work. In 1906 he was in the full flood of his work with Clarke. It was because of this that he gave up his professorship and charge of beds at University College Hospital. He must have found the responsibility of the care of general surgical patients and the routine of student teaching too much an interference with what he really wished to do. Furthermore, the clinical work he really enjoyed was mainly at Queen Square, and he was in great demand as a consultant. In those days a successful London specialist had many long-distance calls. The country was rich, the number of people who could afford to summon the best opinion north, south, east, and west was considerable. Horsley's counsel was often sought on cases which turned out not to be surgical. This was regarded as quite dreadful by some of his medical neurological colleagues. It was, on the other hand, quite all right for them to see surgical cases; that was traditionally correct since Linacre's day, and put the surgeon in his proper place.

There is rarely unanimity as to the shape reforms should take even when their necessity is agreed. This showed up

in his work for the British Medical Association, into the affairs of which he threw himself with his accustomed vigour, and there his professional and scientific reputation gave great weight to his counsels. He was one of the founders of its new constitution, and Chairman of its Representative Body (1903-6). Muirhead Little in his *History of the British Medical Association* (1932) spoke of Horsley's radicalism tempered though it was by a strong belief in constitutional methods. But, he says, "whatever opinions he held, whether on experiments on animals, on total abstinence or medical reform, he held them very strongly, and it was difficult for him to realize that those who differed from him were actuated by as honourable motives as he was himself." Those who differed were ruffians, scoundrels; he did not mince words. The fact that Lloyd George turned to him for advice in drafting his Insurance Act did him great harm in the estimation of the more conservative members of our profession.

In his letters home after he had had time to look around as consulting surgeon to the Army in Mesopotamia, he wrote of the "drunken misdirection of our military affairs," of the "apathy and indifference of the whiskey drinkers." He was, he said, "trying to make the aged and incompetent realize that the British Soldier is a human being" (Paget, 1919, p. 305). Off he went to India to lay the state of medical affairs before the Viceroy, Lord Willingdon, and the Commander-in-Chief, who had already had disturbing reports and were now so impressed that inquiries and reforms were quickly set on foot. Nothing in Horsley's life does him greater credit than the tearing and characteristically unselfish energy of his work for the Army in the field in the last months of his life. It was superb. His disregard, his open contempt even, for bigwigs would have brought only derision had it not been for the force of his personality, his absolute integrity, and the passionate logic of his arguments.

There is a story, now traditional, that Horsley had an almost imbecile belief in the immunity of a total abstainer from heat or disease. It is time that that fable was destroyed. Dr. Anthony Feiling, who was a young medical officer in the hospital to which Horsley was admitted, there to die on July 16, 1916, has kindly given me his impressions of Horsley's last illness. It is true, he says, that he died of heatstroke, but the cause was an infection, probably paratyphoid, though that was never proved. The lack of proof was not very surprising, since their bacteriology was, Dr. Feiling says, by no means reliable. Nor did differentiation become so anywhere, I believe, until some years later. Horsley perhaps did not wear the regulation topee and spinal pad that was so wrongly supposed to prevent sunstroke. But Horsley did not die of sunstroke, but of heatstroke following an infection, the atmospheric temperature near Bagdad in July making body thermostasis difficult even for the young. His opposition to alcohol did not, by the way, prevent him, I am told by Sir Archibald Gray, from permitting his house-surgeons to give alcohol in emergencies to his patients at their discretion. It was a common constituent of the nutrient enema then in such use; Horsley's patients were not exempt.

It was perhaps not always easy to catch enough glimpses of nobility in this man's character. They were often obscured by the mantle of his "extracurricular" activities, in which he roused such passionate devotion or such dislike. Maybe he fished too much in troubled waters, but had he not followed his bent he would have denied his genetic inheritance. Time has shown, annoyingly for his opponents, that the causes to which he devoted his life were good causes—the creation of neurological surgery, the necessity for experiments on animals, temperance if not abstinence, universal women's suffrage, the liberalism of his political opinions, the need for some reforms in our profession, and Government provision of free treatment for the working-man. Such variety suggests a kaleidoscopically colourful personality and makes us feel pale and desiccated beside him. Violent in expression Victor Horsley might at times have been, but he was a fastidious man as well as generous and humorous too.

Well might he say now, "Praise me for virtues, excuse me for my faults, which were never mean."

I wish to acknowledge the help which has been generously given me by many people. My thanks are particularly due to Sir Victor's daughter Pamela (Mrs. Stanley Robinson), to Sir Arthur MacNalty, Dr. Ernest Sachs, Sir Archibald Gray, Professor Robin Pilcher, Mr. Morriston Davies, Dr. Anthony Feiling, Sir Gordon Holmes, Dr. Purdon Martin, Dr. Golla, Dr. Z. Mennell, and to Mr. Julien Taylor and Mr. John Hankinson, of Queen Square, who helped in the search for old case records. Lastly, my debt to Stephen Paget's biography of Sir Victor Horsley is immense.

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SIR VICTOR HORSLEY: HIS LIFE AND WORK*

BY

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Victor Alexander Haden Horsley was born at No. 1, High Row, Kensington, on April 14, 1857. His father was John Callcott Horsley, R.A., a famous artist of his day; his mother Miss Rosamund Haden, sister of Sir Francis Seymour Haden, surgeon and etcher. Horsley's first two names were given him by Queen Victoria, and we note how appropriate they were. Victor, "a conqueror," and he was to conquer in the battle against disease; Alexander, "a helper of men," and he was to be a benefactor of mankind.

Early Days and Education

Horsley was educated at Cranbrook Grammar School, and became a medical student at University College in 1874. In October, 1875, he entered University College Hospital. It was a vintage year, for several of his fellow students achieved distinction in later life. Of these may be mentioned: Francis Gotch, later professor of physiology at Oxford, who married Rosamund Horsley and became Victor's brother-in-law; Sir Frederick Mott, physician and neuropathologist; Sir Dawson Williams,

*A Holme Lecture delivered at University College Hospital Medical School on October 28, 1955.