

## Chapter 4

# The Prolific Years

In the life of every scientist who contributes something of importance to his field there must have been a long period of intense devotion to experimentation, reading, study, and thinking, taxing the patience of a dear wife who would find it difficult to understand the sudden shifts in the husband's moods from distant solitude and absent-mindedness to moments of obvious elation depending upon intellectual revelations difficult to share with others. In Sherrington's life 1895 to 1913, the years at Liverpool, were such a period of intense creation and study. So far only those aspects of his work at Liverpool have been presented that became normative for the future development of theoretical neurophysiology as we know it today, but his accomplishments included many other things which alone would have made a lesser man satisfied with his day of labour.

During those years Sherrington wrote *Integrative Action*, he was responsible for some of the best chapters in Schäfer's *Text-book of Physiology*, those on the spinal cord, the brain stem, cerebellum and medulla oblongata, on cutaneous sensations, and on the muscular sense, all of which are still highly readable, forward-looking products of well-digested experimental experience and vast reading; he taught, even physiological chemistry, sat on committees, and published reports as professors had to do then as now; he built and organized a new laboratory, and wrote much of the poetry in the *Assaying of Brabantius* not to mention the experimental papers, from which only some leading ideas can be extracted here. He wrote too much, said Cushing, but life was exciting and the cup was full.

What would Sherrington have retorted, had he known of Cushing's remark? Possibly what he said to me once when we

were talking about John Fulton's biography of Harvey Cushing: 'You know, the life of a great surgeon is not really exciting, except, of course, from the patient's point of view.' We discussed the subject at some length and Sherrington began to recall names of surgeons whom he would regard as exceptions. The two he mentioned were Lord Lister and Wilder Penfield, both also scientists and physiologists.

In those days professors did not have secretaries but Sherrington's wife took upon herself the secretarial duties needed in the laboratory and also for his international work, because at the time and for fifteen years Sherrington was Anglo-American secretary of the International Congresses of Physiology. Sherrington and Ethel Wright of Preston, Suffolk, had been married in 1891 when he was made Professor Superintendent of the Brown Institution.

There was not much of a laboratory when Sherrington arrived in Liverpool. He described it to me as 'almost a shed'. But the University found a benefactor in the Rev. S. A. Thompson Yates and in October 1898 the new laboratories for physiology and pathology, bearing Thompson's name and built by Messrs. A. Waterhouse & Son, were opened with a solemn act. Several eminent guests were present, among them Lord Lister and the pathologist, Professor Virchow, in whose laboratory in Berlin Sherrington had worked when he returned from Puglia. The donor gave an opening Address published in the *Thompson-Yates Laboratories Report*, volume I, edited by the two professors (later, Sir) Rubert Boyce (1863-1911) and Sherrington. In that volume of 238 pages octavo a large number of papers came from Sherrington's laboratory.

The architect, Sherrington told me, laughing heartily, discussed with the two professors what kind of an emblem should be placed on the wall of the new building. In the end they decided in favour of an idea symbolizing Pathologia and Physiologia by two attractive female figures in the manner shown in Plate 7 and decided to have them executed by a sculptor in the likeness of Mrs Boyce and Mrs Sherrington. This may have been a well-kept secret, as I began to suspect when I asked Professor R. A. Morton of Liverpool University to obtain a reproduction for this book and also mentioned the story to the present physiologist at Liverpool, Professor R. A. Gregory; neither of them knew it.

As a consequence the grime and soot of sixty-four years have now been removed from the relief and the young charming wives, attired *à la Grecque*, can be admired in their original beauty, possibly somewhat idealized.

In the opening address by the Rev. Thompson Yates there were some words which Sherrington at least would have listened to with appreciation: '. . . I would urge you all, whatever your profession, not to be satisfied with knowledge merely professional. You will make better doctors, better men of Science, better citizens, and be of greater use to your fellow-creatures if you know something of the side of human thought expressed in the words Philosophy, Poetry, History, Literature. You have not time? Then make it somehow; I believe we all can do so, and make ourselves better men and women by the self-denial involved in the manufacture.'

The *University of Liverpool Recorder* published in January 1960 an extract from the *Memories of Sherrington* by his son, C. E. R. Sherrington, which referred to the Liverpool years:

Writing to his mother in October, 1895, from Grove Park, Liverpool, he described the house as in many ways like his devoted home at Edgehill, Ipswich. . . . [In the city he found] the distances small, and the traffic countrified, and the shops too.

He was pleased with the climate which was bright and sunny compared with that of London; and he was well received both by his colleagues and the great Merseyside families who ruled in the shipping world and society and, on occasion, also proved willing to support his experiments.

One of Sherrington's early students, an otologist and surgeon, H. V. Forster, in a brief address to the laboratory at Liverpool (1961) has some interesting reminiscences of the young professor, worth putting on record:

I can see him now, coming through that door on my left in his black gown (1907). There was nothing of the showman about him, but much humility and respect. . . . We had heard with much curiosity that at Liverpool when working with Grünbaum, Sherrington had mapped out areas of function in the motor cortex of the chimpanzee and even the gorilla, and in due time after instruction about the reciprocal action of antagonistic muscles we were introduced to its reversal in the leg area here . . . both in strychnine and tetanus

toxaemia. . . . It is not surprising that we were especially attentive to his ideas on the predominance of the brain and the head as physiologically conceived.

Forster had some old lecture notes on this:

'The segments lying at the leading pole of the animal, armed as they are with the great "distance" sense organs, constitute what is termed the "head". [Then he goes on to enquire whether this dominance of the leading segments which is traceable in the receptors of the exteroceptive field applies in the field of reception which is termed the proprioceptive.] The labyrinth (for example) is the chief proprioceptor of the leading segment of the body which is the head. The labyrinth keeps the world right side up for the organism by keeping the organism right side up to the external world.'

Sherrington's ideas on the rôle of proprioceptors, the labyrinth and the cerebellum in muscular tone had already acquired their general form. Sherrington spoke of the cerebellum (in Dr Forster's notes) as the head ganglion of the proprioceptive system, as he was wont to do to the end. The cerebrum was the ganglion of the distance receptors. He liked to venerate its conception as the 'neo pallium', the pallium being 'that woollen covering, an emblem of archiepiscopal dignity' built up chiefly on impressions from the eyes with their capacity for binocular vision. His master's voice is also vividly there in the notes on the knock-out in the Prize Ring which excited the young student: 'The knock-out blow where the lower jaw conveys concussion to the otocyst, reduces in a moment a vigorous athlete to an unstrung bulk of flesh whose weight alone determines its attitude, if indeed a reactionless mass can be described as possessing attitude at all.'

A. S. F. Grünbaum (1869-1921) whom Forster mentioned is better known under the name of Leyton (after 1915, by deed poll). He was Sherrington's assistant at Liverpool and later became Professor of Pathology at Leeds. Like Sherrington he was a Gonville and Caius man who had also been at St Thomas's Hospital. In 1896 Leyton had worked in Vienna where he devised the agglutination test in typhoid fever but felt that he had too few cases and so could not publish it. Widal then, on two cases, preceded him by a few weeks (Sherrington). Leyton

and Sherrington carried out work, still quoted today, on the motor cortex of the chimpanzee, orang-utan, and gorilla. Other members of the group were T. Graham Brown, F. W. Mott, and E. Schuster. In 1901 Cushing joined them for a month. A young American, R. S. Woodworth, proved good and was tempted to stay on after having studied with Sherrington a 'pseudo-affective reflex' in the decerebrate preparation, recently taken up again by R. K. S. Lim, but finally he returned to the States to become Professor of Psychology at Columbia University and live to a great age as the revered Nestor of the American psychologists. In a letter to me (January 27, 1954) containing some reminiscences from Liverpool, Woodworth among other things pointed out that Sherrington began his day with research, and not, as most professors, with minor matters of administration and instruction. Prominent among foreign guest workers at Liverpool were Rudolf Magnus (see below), H. E. Hering, discoverer of the carotid baro-receptor reflexes, and the Austrian, Alfred Fröhlich, well known for his description of the so-called adipo-genital syndrome. In his Sherrington Lecture of 1952 Fulton has some extracts from Cushing's diary and concludes: 'Gay and busy days these were, and Sir Charles and Alfred Fröhlich still recall them with a lively sense of pleasure, as did Harvey Cushing while he lived.'

'Glancing back in perspective,' says C. E. R. Sherrington in the *Liverpool Recorder*, 'the years at Liverpool were, I believe, the happiest in his life, and my mother's too,' and small wonder. Peace prevailed in the world and work in the new laboratory was exciting. Guest workers came and stayed on to experiment under Sherrington's guidance; visitors from the United States and Canada in those days took the route by Liverpool, and international contacts with the European continent were developed, officially, by his work as Anglo-American secretary of the congresses, unofficially as well, by travel for pleasure during holidays. The young couple loved Paris. His wife, being partly of French origin, spoke the language fluently but may not have shared her husband's taste for browsing among the *bouquiniste* stalls on the Rive Gauche and hunting up incunabula and old books in libraries to lay the groundwork for his study of Jean Fernel and the ancient history of the Latin Quarter. For this

suspicion of mine Fulton is responsible because he mentions the following incident.

Sherrington in 1935 'summoned me to his room and with a slightly apologetic expression showed me some forty medical incunabula stored away in the back of a large drawer. With a smile and twinkle that all book collectors understand [Fulton, of course, was a passionate collector of old books], he said: "Dear Ethel, my devoted wife, doesn't fully share my interests in incunabula and I am passing them on to the British Museum."'

With some right, Wilder Penfield speaks of the 'celebrated' contributions by Leyton and Sherrington to our knowledge of the functional organization of the motor cortex in primates. He should know, having himself explored the motor areas in man more extensively than anybody else. The question naturally arises as to what Sherrington could add to the findings of Fritsch and Hitzig (1870) in Germany and of Ferrier (1873-81) in England. They were the pioneers. Sir Victor Horsley (around 1890) followed up Ferrier's work. The Liverpool group started in the early nineties but publication followed some twelve years after Beevor and Horsley. The full report came in 1917, after Horsley's death, possibly because Sherrington wanted to avoid a second polemic with the latter. In a letter (in Fulton's collection) from Sherrington to Dr Mond, who took care of the expense of buying monkeys, Sherrington pointed out that he could not confirm Horsley. Between Goltz on the one hand and Fritsch, Hitzig, and Ferrier on the other, surgery had improved a great deal. Animal care and surgery underwent further improvement with Leyton and Sherrington, the latter, as all his pupils know, being extremely careful in his surgical work. Of Leyton, Sherrington said: 'He combined in rare measure enthusiasm for research with a critical, and, indeed, highly self-critical attitude of mind.'

In those days difficulties were considerable. Ferrier, probably the most skilful of the early workers, had, for instance, misplaced the visual centres and as late as 1905 Horsley, in a well-known and important paper with Clarke, had found that electrical stimulation of the surface of the cerebellum did not produce any response in the animal. Yet it is well known that both movement and inhibition of movement can be elicited from the cerebellar cortex, as shown by Sherrington in 1896.

The extremely detailed map of localization in the motor cortex of primates which Sherrington and Leyton published became the accepted standard for many years, but equally important were the concepts they reached. They emphasized the functional instability and deviation of the response that became evident as soon as the explored point was sufficiently small and tested often enough over some time. A cortical point representing, say, extension of the index finger is connected to a number of other points (cells) which represent various combinations in which this movement is elicited. A contraction of a particular facial muscle may take part in chewing, in mimetic acts, in the production of sound. 'And in the motor cortex this discrete "representation" of small local items of movement, each highly co-ordinated with others yet separably elicitable, instead of becoming less evident with ascent to the higher types of hemisphere, becomes more so. Thus, it is more evident in cat and dog than in rabbit, more evident in the Macaque than in cat or dog, in baboon than in Macaque, in gibbon than in baboon, and in the chimpanzee, orang, and gorilla than in gibbon. . . . Phenomena such as "reversal of response" (from one type of movement to its opposite), "facilitation", and "deviation of response" prominent in cortical responses and accounting for the functional instability of cortical motor points, are indicative of the enormous wealth of mutual associations existing between the separable motor cortical points. Those associations must also be a characteristic part of the machinery by which the synthetic powers of that cortex are made possible.'

We have not since found it necessary to alter in any fundamental way the picture Leyton and Sherrington gave of the functional organization of the motor cortex even though the number of sites in the cortex from which motor effects can be elicited has since increased.

Localization of function has always been a field in which physiologists, anatomists, neurologists, and neurosurgeons take a common interest. It is hardly necessary to dwell upon the need for functional maps in clinical work. One example may suffice. Hughlings Jackson, from studies of focal epileptogenic foci, had predicted the localization of the motor area one year before Fritsch and Hitzig discovered it by direct electrical stimulation.

Sherrington followed this work through with great interest, and when Penfield discovered places in the brain of man which on stimulation during operations made the patients recall long-lost incidents in their lives, his old teacher looked at him with a quizzical smile and said: 'It must be nice to hear the preparation speak to you.'

To this time at Liverpool belongs a story that has often been told but which is so characteristic of him that I must re-tell it, in spite of my intention to include very little of the well-known anecdotal flora of Sherringtoniana. The professor was reported in *The Daily Mail* as having said in a lecture: 'One day after a visit I turned back, pondering what the chimpanzees might do when I left them. I stooped down and looked through the keyhole, and there the chimps' eye—I mean the chimpanzee's eye—met mine. The same thought had struck us both, but the chimpanzee, being a lady, had got there first.'

With some justification one might say that much of the work that Sherrington did on the supraspinal structures was of the kind that in itself would not have given him the unique position he now occupies in the history of neurophysiology and neurology. It was important enough in its day to place him in the front rank among those who influenced progress in these fields, and probably it was more reliable than that of many of his best-known contemporaries. Why then did he spend so much time experimenting on the brain when the rich harvest lay in the spinal cord with the conceptual advance that the better-known organization of that structure made possible, as explained above?

The answer has already been indicated in the extracts from Dr Forster's lecture notes. The spinal cord is a servant of that great master, the brain. It may be an exaggerated metaphor to compare the cord with a grand piano requiring its 'supraspinal' virtuoso, but the simile is useful in order to explain why Sherrington and most of his successors sooner or later have been irresistibly drawn towards the higher centres, mostly, I suppose, coming to the conclusion that in the end the relative precision of work in the lower portion of the neuraxis provides greater satisfaction. On the other hand, experience with the organ of supreme control is required by any one interested in reflexology

and the spinal cord. Without it, the integrative action of the central nervous system can never be fully apprehended.

Thus with Sherrington, who on Gaskell's advice had fled from the *clair-obscur*e of cortical physiology into the relative daylight that study of the spinal cord seemed to promise, was forced back to his original point of departure by observations and circumstances which should now be briefly reviewed in order to illuminate his development as an experimenter and thinker.

During his visits to Goltz's laboratory, Sherrington had become acquainted with the general symptoms exhibited by the spinal dog and so was familiar with the fact that removal of supraspinal control led to a state of shock characterized by great depression of response to stimulation in the musculature below the section. This was a striking demonstration of the importance of the higher centres. In many respects this condition was known to develop in different ways in dog and in man where it was familiar, as a result of accidents for instance. Sherrington had recourse to monkeys and soon found that both permanent damage and initial shock are disproportionately greater in monkeys than in cat and dog, and following out his case he separated 'shock' as such from the later effects of isolation-dystrophy. He found monkey and man more similar in their symptoms than man and dog. 'The symptoms of shock, in many monkeys, persist for days instead of hours and minutes, as in cat and dog' (Marshall Hall Prize Address 'On the spinal animal', 1899). This is in keeping with the law of increasing encephalization of control upwards along the phylogenetic scale (von Monakow).

Sherrington studied the spinal state with his customary attention to every detail of behaviour, sensory, motor, vegetative, and his work became normative both for the clinic and the laboratory. Several of his pupils have continued this work (Denny-Brown, Fulton, Liddell, McCouch, Rioch), and for thirty years the spinal or the spinalized, decerebrate animal has been a favourite preparation in which to study the spinal cord deprived of central control, especially in work on problems of synaptic transmission. For such problems it is often necessary to simplify the neural circuit. Thus, for instance, Lloyd in his analysis of monosynaptic transmission in the forties found this preparation a valuable tool.

In a similar way other supraspinal structures were drawn into Sherrington's sphere of interest. In 1896-98 he made the fundamental observation that decerebrate rigidity could be inhibited by stimulating the anterior portion of the cerebellum, unilaterally, if only one side of it was stimulated. This work was developed by one of his later pupils, Frédéric Bremer (1922), professor at Brussels, one of the best-known neurologists and neurophysiologists of the present day. Sherrington reported his finding in 1906 and published it in 1907. One month later the same result was reported by Loewenthal and Horsley. Sherrington went on to describe in considerable detail a number of other sites from which decerebrate rigidity could be inhibited.

In many other ways the problem of supraspinal control engaged Sherrington's attention, in connection with tone, standing, and postural reflexes in general. The matter seemed to him very important, and rightly so, because nearly all movements have a postural component. The importance of posture is sometimes emphasized by saying that it is the starting-point and end-point of every movement. But posture, as Ramsay Hunt remarked, accompanies 'movement like a shadow'. Stanley Cobb (another Sherrington disciple) writes that 'they coalesce and often cannot be separated'. As an example, Sherrington said, 'one might take the scratch reflex elicited by an "artificial flea" in the form of a faint faradic current tickling the skin behind the ear. The dog executes a most complex series of postural reflexes to balance itself while scratching. An array of muscles is co-ordinated or integrated in this act'.

If one were to describe the many fundamental observations Sherrington made on posture, they would fill this book. Alongside his experimental and conceptual advance towards the definition of properties of synaptic action at membranes of single neurones, his work on tone, or posture as he preferred to call it, stands as a monument *aere perennius* in a triad whose third component is the analysis of reciprocal innervation. I place them in this order but we have seen that Sherrington himself put reciprocal innervation first, probably because to him it must have been the great eye-opener for the general problem of inhibition. And so this particular conquest of his youth became surrounded by an aura of delight that never vanished

What we now call the Sherringtonian type of decerebrate

rigidity, discussed above, is a kind of extensor spasm or exaggerated tone dependent upon intact afferents from the rigid muscles. But it is not the only form of extensor rigidity, as Sherrington very well understood. Ablation of the cerebellum will also cause rigidity, and comparing this state with the one ensuing upon intercollicular section Sherrington with penetrating insight concluded: 'That the two conditions are identical I am not convinced' (1898). Later, but in his lifetime, Pollock and Davis contributed experiments which made it clear that cerebellar rigidities did not vanish upon de-afferentation. Sherrington himself showed that rigidity in the forelimbs increased when the spinal cord was sectioned at some lower level. This finding is known as the Schiff-Sherrington phenomenon because Schiff had made a similar observation on the frog (1859). In 1936 it was taken up again by T. C. Ruch. Today we know a great deal more about the various factors determining muscular tone (see later) and they have been well analysed by Moruzzi in his important monograph with Dow, the latter responsible for the clinical part (*Physiology and pathology of the cerebellum*, 1958).

The restricted space available for discussion of each subject should not lead to the rash conclusion that nothing was known about supraspinal factors influencing tone before Sherrington arrived on the stage. He had had great predecessors, especially in the field of cerebellar physiology, and at least two names must be mentioned, those of Flourens (1794-1867), in perspective one of the greatest of all French physiologists, and Luciani (1842-1919), practically a contemporary, who had published his famous book, *Il Cervelletto*, in 1891. Ewald, whom Sherrington knew well, had pointed out that destruction of the vestibular organs reduces the tone of the skeletal musculature on the side of the operation. Sherrington's standpoint that the cerebellum might be regarded as the central station for the control of proprioceptive reflexes therefore had much support in both old and contemporary experimentation.

Is this notion still valid? There are those who deny this, basing themselves upon what Moruzzi has called the 'unexpected discovery' that the cerebellum receives important projections from exteroceptive senses such as the eye, the ear, and, in particular, the skin. This was found almost simultaneously in

independent electrophysiological experiments by Adrian, and also by Dow and by Snider (1942-43) and co-workers. But why need such findings exclude the cerebellum from its rôle as 'proprioceptive head ganglion'? It seems reasonable that an organ for proprioceptive control would need all that information from exteroceptive senses. Posture, after all, is part of every movement and movement takes place in surroundings continuously measured by our exteroceptive senses aided by the vestibular organs of balance and the endings in muscles and joints. The cerebellum, we might say today, in expanding Sherrington's concept, computes sensory information for the purpose of postural control. Space does not permit us to discuss other tasks allotted to this organ (cf. the Dow and Moruzzi monograph, mentioned above).

Sherrington's work on muscular tone had led him to the final conclusion that, in mammals, tone was due to postural reflexes, just as Brondgeest at Utrecht (1860) had found it to be the case in frogs and rabbits. Brondgeest's work had immediately been repeated and confirmed by Dr Rosenthal in the laboratory of Du Bois-Reymond, who added a note to this effect in his *Archives* where the Dutch worker had published his findings. These problems occupied Sherrington very much at the time (in 1908) when Magnus arrived in Liverpool and possibly the latter had been impressed by the 'reflex figures' which the decerebrate animals assumed when stimulated in various ways (Fig. 7). Be that as it may, when Magnus with his co-worker de Kleijn demonstrated that turning of the head set up characteristic patterns of tonic reflexes (1912), Sherrington was quite content to hand this theme over to them even though, two years before, he had written in his extensive study of stepping and standing: 'Again, active or passive rotation of neck on its long axis in the decerebrate preparation inhibits extensor tonus in hind-limb on the side of the lowered pinna and causes active flexion of knee; and does so after severance of both cranial 5ths and both octavi.' Indeed, Magnus and de Kleijn were urged by Sherrington to pursue their work and in the end they made a monumental contribution to our knowledge of postural reflexes. In due course Magnus and de Kleijn became serious candidates for the Nobel prize (Liljestrand) but Magnus' untimely death (1927) put an end to such proposals.

The work of Magnus and de Kleijn is a model of pure integrative physiology. It describes a large number of automatic reflex adjustments of posture which are static 'figures' or else sequences of co-ordinated acts, the most interesting being the righting reflexes analysed in the thalamic rabbit and those arising from the labyrinthine organs and the neck muscles (or, rather, their ligaments, McCouch) which lead to characteristic postural effects from the head upon body and limbs. These involve the long spinal paths which Laslett and Sherrington had studied (1903). Magnus and de Kleijn were also inspired by their own findings to make a fundamental analysis of the natural stimuli to which the vestibular organs are sensitive, a field in which another pupil of Sherrington's, the late Mario Camis, professor at Parma, had much success. His excellent monograph *The Physiology of the Vestibular Apparatus*, was translated into English by R. S. Creed in 1930.

The integrative line and Sherrington's work on the cortex appealed particularly to pupils of certain temperaments and interests especially, of course, to those who wanted to devote themselves to clinical work; the spinal cord work, as the years went by, tended to become more theoretical and analytic and this line therefore held out greater promise for the physiologists and the theoretically minded. To the former group belonged John Fulton (1899-1960), professor at Yale University, who played a great rôle as an inspiring force in brain physiology. 'The establishment of the first laboratory of primate physiology in the U.S.A. was a signal triumph' for Fulton, wrote Earl Walker in his obituary and continued: 'experiments by the great workers of the past were repeated there under more ideal conditions and with anatomical controls so that they yielded more "meaningful" results.' In the thirties Fulton's laboratory attracted a large number of young neurologists and neurosurgeons from all over the world and many more have sought information about the intricate functions of the brain from his *Physiology of the Nervous System*. Throughout his life, Fulton looked upon the Oxford years and the guidance of Sherrington as his main source of inspiration.

The most important clinical schools which owe allegiance to Sherrington are the Montreal Neurological Institute, Wilder Penfield's creation, where he and Herbert Jasper have for years

played a leading rôle in world neurosurgery, and the department of Denny-Brown in Boston, combining experimental neurology with clinical work.

A large part of the influence of a teacher, and the most difficult part to account for, derives from the example he sets in trying to realize his ideals, scientific, personal, moral, aesthetic, and in his loyalty towards them. A modicum of idolatry on the part of pupils must perhaps be accepted as an expression of their attempts to formulate valid ideals of their own, needed to carry them through disappointment and monotony. In this wider sense Sherrington was a teacher of many young men who later in life remembered him with gratitude and paid homage to his influence. These are difficult to trace unless they were actual co-workers or have since made a name for themselves in the wider subject of medicine. It is impossible to mention more than a few, neurologists such as Russell (since, Lord) Brain, Stanley Cobb, W. C. Gibson, Gordon Holmes, Sir G. Jefferson, S. Obrador, Henry Viets, F. M. R. Walshe; physiologists, as H. C. Bazett, N. B. Dreyer, Lord Florey, the brothers H. E. and E. C. Hoff, Grayson McCouch, Ian Marcou, Karl Matthes, F. R. Miller, J. M. D. Olmsted, R. W. Reid, H. E. Roaf, T. C. Ruch, K. Sassa, S. C. M. Sowton, F. M. Tozer, and many others.

Graham Brown, representing an older generation of co-workers, is here chosen as their spokesman: 'It was to those who worked in his laboratory that Sherrington gave and showed most. The research student would enter it, timidly, with a mental picture of the author of the *Integrative Action* as a man who must dwell in a sphere of his own making, difficult to attain or to travel in—a man necessarily aloof and out of common reach, "academic", coldly intellectual. But the disarming spirit of equality with which the recruit was received at once destroyed a mental barrier which could not in any case have for long withstood the sympathetic friendship which was offered by Sherrington and earned in return. . . . The laboratory life was a continuing adventure with always widening horizons . . . he would be moved . . . by the beauty of an experiment or of an ingenious technical method. And always there was originality and insight.'