THE LIFE AND TEACHING OF WILLIAM SHARPEY (1802-1880) 'FATHER OF MODERN PHYSIOLOGY' IN BRITAIN

by

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PART II

VI. SHARPEY AND THE ROYAL SOCIETY

For forty years, William Sharpey exercised great power and influence in the London medical and scientific world.[153] As early as 1840 he was elected to a term of office as Dean of the Medical Faculty in University College. He had become F.R.S. in 1839, and in 1844–45 was elected to the Council of the Royal Society. In 1853 he was appointed a Secretary, and for almost all his long tenure of that post (until 1872) he was in fact the senior Secretary, Stokes being appointed in 1854. As Secretary, he became ex-officio a member of the Royal Society Club, 'one of the most genial and attractive members the club ever possessed', and in 1862 he became its Treasurer.[154] He was an original member of the Philosophical Club, which was formed in April 1847, of forty-seven members of the Royal Society, as a dining club with the aims of keeping alive the enthusiasm behind the Society's reforms of that year, and of increasing the influence of science in Britain.[155] Later in his career he served on the General Medical Council and on the Royal Commission on Scientific Education, and played an important part at the time of the anti-vivisection legislation in 1876.

I have been able to examine nearly four hundred letters that pertain to Sharpey's period as Secretary of the Royal Society. About half of these are in the possession of University College London, and are almost all uncatalogued. Most of the others are to be found either in the Royal Society Library or in Arbroath Public Library; a few are in the British Museum, in the Library of the Royal College of Surgeons, and amongst the Huxley papers in Imperial College. The Royal Society possesses, in addition, just under sixty referees' reports written by Sharpey between 1843–1872, and a small number of miscellaneous documents, unsigned, but clearly in his hand. Virtually all the letters have to do with the day-to-day business of the Royal Society. About eighty of them, including all those in Arbroath, are written by Sharpey, as

[153] Sprigge wrote of 'Sharpey, the profound physiologist and the autocrat of the elections of the Royal Society.' (S. Squire Sprigge, *The Life and Times of Thomas Wakley*, London, New York and Bombay, Longmans Green, 1897, p. 444). T. H. Huxley, in a letter to his sister (21 November 1850) about the publication by the Royal Society of his 'Rattlesnake' drawings and notes, said that 'Owen, Forbes, Bell and Sharpey (the doctor will tell you of what weight these names are) are all members of the Committee which disposes of the money and are all strongly in favour of my "valuable researches" '. (*Life and Letters of Thomas Henry Huxley*, by his son Leonard Huxley, 2 vols., London, Macmillan, 1900, Vol. I, p. 62).

[154] Sir Archibald Geikie, *Annals of the Royal Society Club*, London, Macmillan, 1917, pp. 364–65.

[155] Sir Henry Lyons, *The Royal Society 1660–1940. A History of its Administration under its Charters*, Cambridge University Press, 1944, p. 264.

copies or drafts. The majority are letters to him, written by Edward Sabine[156] (117), Sir Benjamin Brodie[157] (38) and G. G. Stokes[158] (27). They show abundantly the extent to which these men relied on Sharpey's judgment—in elections to the Fellowship and to the Council of the Society, for example—and the high esteem in which he was held. Sabine, writing about the Royal Society Treasurership says: 'Paget and Bowman are both Physiologists and you are yourself a *host* in that department'.[159] Sir B. C. Brodie, *Jr.*, after his father's death, sent a silver medal of commemoration to Sharpey as having been an intimate friend, with the words: 'He had a very great regard for you, and your kind attention to him especially during the last two calamitous years of his life will ever be remembered by me'.[160] And John Tyndall, hearing that he was to be proposed for a Royal Medal, asked Sharpey to squash the proposal 'in some nice sagacious way peculiar to yourself'.[161] Indeed sagacity and tact must have been frequently at a premium in his dealings with disappointed candidates for the fellowship, authors and others. An interesting example of their application is provided by T. H. Huxley, who wrote to Sharpey in the greatest indignation[162]—Sabine in a letter to Sharpey alludes to a 'very painful' letter from T.H.H.[163]—protesting about re-election to the Council of the Royal Society of Richard Owen, with whom he was at the time in a dispute of long standing on the presence or absence of the third lobe, the posterior horn of the lateral ventricle, and the hippocampus minor, in animals other than man. Sharpey replied on the same day in emollient terms, but firmly standing his ground on behalf of the Royal Society and taking the trouble to append to his letter a considerable list of persons with their dates of election and re-election to the Council.[164] Huxley, while maintaining that Owen's election would be construed to his own disadvantage, admitted to having 'as I hope you know, the most entire confidence in your sense of fairness and justice—and, as in such a case as this, indignation at what seems to me to be the grossest piece of scientific knavery ever perpetrated, may warp my judgement—I entirely defer to your opinion and shall act accordingly'.[165] Huxley, seems in fact to have been on terms of friendship with Sharpey over many years. Sharpey assisted him when, as a young man, he applied unsuccessfully for the Chair of Zoology in Toronto.[166] On more than one occasion he wrote to give Huxley the benefits of his wide reading,[167] and in his old age, his doings and movements were a matter of

[156] Sir Edward Sabine (1788–1883), soldier, explorer, and authority on terrestrial magnetism. Treasurer, Royal Society, President, Royal Society, 1850–61, 1861–71. See *Dictionary of National Biography*.

[157] Sir Benjamin Collins Brodie, the elder (1783–1862), surgeon. President, Royal Society, 1858–61. See *Dictionary of National Biography*.

[158] Sir George Gabriel Stokes (1819–1903), mathematician and physicist. Secretary, Royal Society, 1854–85. See *Dictionary of National Biography*.

[159] Letters (uncatalogued) in the Library of University College London dated 3 July 1861.

[160] Ibid., dated 27 February 1863.

[161] Ibid., dated 15 October 1863.

[162, 164] Ibid., dated 13 November 1862.

[163, 165] Ibid., dated 14 November 1862.

[166] Warren R. Dawson, *The Huxley Papers. A Descriptive Catalogue of the Correspondence Manuscripts and Miscellaneous Papers of the Rt. Hon. Thomas Henry Huxley, P.C., D.C.L., F.R.S., preserved in the Imperial College of Science and Technology London,* London, Macmillan, 1946. Scientific and General Correspondence, 26, 64–66.

[167] Imperial College of Science and Technology, University of London. Thomas Henry Huxley. A list of his scientific note books, drawings and other papers preserved in the College Archives, compiled by Jeanne Pingree, College Archivist, 1968. Box F. 48 vo.

interest both to T.H.H. and to Mrs. Huxley.[168]

Sharpey's reports as a referee of papers for the Royal Society make very interesting reading and in my view do much to render apparent to us those qualities from which a considerable part of his reputation stemmed. They are models of clarity and succinctness; they give first a penetrating analysis of the problem into its component parts, and are then constructively critical of the method by, and extent to which the author has dealt with these; and they show an impressive acquaintance with the relevant literature, demonstrably greater in many cases than that of the author himself—and this over the whole field of physiology and anatomy, histology, and embryology, a coverage that was maintained during a period of thirty years! Indeed, his vast knowledge of the literature may, to some extent, have inhibited production of original work on his part. The manuscript submitted by Augustus Waller, in which he described what has come to be known as Wallerian degeneration, was refereed by Sharpey.[169] The author, he pointed out, was evidently unaware of the work of Nasse and of Günther and Schoen who had also described degenerative changes in cut nerves. The introduction contained nothing outside elementary treatises and the aetiological discussion was superficial. These defects removed, the paper should be published. A paper by F. W. Pavy, 'On the metamorphosis of saccharine matter', which was not in fact published, provoked the comment 'I cannot but regret that the author should have without necessity connected his explanation of the fibrin with views as to the vitality of the blood which though at one time prevalent in British Schools of Physiology are at best totally without proof. Thus he speaks of liquid blood as living and coagulated blood as dead; in the same way he regards defibrinated blood as dead thus apparently denying vitality to the red discs while he assigns it to liquid fibrin; and his phraseology even in the statement of facts is tinged throughout with the same doctrine'.[170] As late as 1870, a report on a manuscript by Radcliffe on animal electricity shows that Sharpey's powers of penetrating but constructive criticism were not one whit abated.[171] Although he disclaimed any authority on the purely electrical aspects of the subject, for an opinion on which the committee were to refer to the remarks of the other referee, Clerk Maxwell, nevertheless this, the longest of the reports—for which he apologised to his colleagues with the reminder that the manuscript was over a hundred pages in length!—shows the same acuteness and grasp as might be expected from a man in his intellectual prime. Not infrequently, in order to be satisfied of the value or otherwise of a manuscript, he would repeat for himself the experiments and observations described. All this work of refereeing, so painstakingly performed, must have served to keep him fully abreast of new methods and additions to knowledge in the areas where his interest lay, this in turn to be incorporated into the foundations on which his teaching was built, as the notes taken from his lectures testify.

VII. INVOLVEMENT IN CONTEMPORARY QUARRELS

It is to be expected, in the nature of human affairs, that those who wield great

[168] *Huxley Papers* (see note 166). Scientific and General correspondence **4**, 94, 109, and **16**, 204, 209.

[169] Referees' Reports **2**, 260, undated. In the Library of the Royal Society, London. The paper was published in *Phil. Trans. R. Soc., Lond.*, 1850, **140**, 423–29.

[170] Ibid., **2**, 187, dated 12 June 1855. [171] Ibid., **7**, 50, dated 1870.

power, and who are so clearly members of the 'establishment' as Sharpey was, should attract unto themselves from time to time envy and obloquy, no matter how amiable they may be as individuals. Sharpey undoubtedly did so, despite the many affirmations that he had not an enemy in the world. There is evidence of it in his correspondence as Secretary of the Royal Society, and in one instance the acrimony overflowed into the correspondence columns of the *Lancet*.[172] More serious examples are afforded by the furore resulting from the award of the Royal Medals at the Royal Society in 1845, and by the 'Cooper-Syme affair' at University College in 1848, in both of which cases the *Lancet* itself saw fit to take a considerable hand. All are of some interest for the light they shed on the methods of controversy used in the academic wrangles of the period.

The *Lancet* on 21 March 1846 fired a few warning shots at the Royal Society and particularly at its secretary, P. M. Roget.[173] 'At the present time there is a very general feeling that the Royal Society or rather the medical section of it, has degenerated into a mere clique; . . . anyone ambitious of its honours had better, in fact, fawn and cringe to individuals than devote himself to the advance of science'. Had the office of secretary, it asked, become perpetual? The influence of the secretary, while great, might not necessarily be always exercised in the most satisfactory manner, and 'rumours were afloat about the award of the last Royal medal'. Further, 'the present secretary, has, we believe, received the not insignificant sum of one thousand guineas for his Bridgewater Treatise and one hundred guineas a year for fifteen or sixteen years. It may be asked, what new fact, or law, or principle in science has he discovered, for which these comparatively rich gatherings may be considered a just remuneration?' Hints of plagiarism, and discrimination against Dr. Robert Grant followed. The rumours about the Royal Medal were given a more substantial form in a letter from George Redford, Esq., surgeon, in which he charged that a paper by Dr. R. Lee 'On the nervous ganglia of the uterus' published in 1841, as the result of many years of work, and printed at the request of the Royal Society, admittedly accompanied by some doubts on the part of certain members of the physiological committee, had been held by the Society in 1845 to have been overturned by an unknown Mr. Beck, on the basis of one dissection, in a paper at once awarded the Royal Medal. On 4 April 1846, the *Lancet* returned to the attack. The awarding of Royal Medals since 1829 was analysed very critically. It was said that according to rumour, one recipient, Mr. Newport, received his medal, not so much for his work, as for correcting the proofs of Dr. Roget's Bridgewater treatise, after Grant had refused to do so on finding his own lectures plagiarised therein. Redford's letter was

[172] A Dr. J. Newton Heale wrote to the *Lancet* on 4 February 1860, to say that a MS. of his, on the distribution of blood-vessels in the lung, which had been refused publication by the Royal Society in 1853, was apparently no longer to be found in the Society's archives. He maintained that since a second paper on the same lines had been rejected not because it was false, but because it was unoriginal, his first paper established his priority. One of the referees on that occasion had been Sharpey, whose own work Heale maintained that he had disproved. Heale, in fact, virtually accused Sharpey of deliberately mislaying his paper of 1853. The details of the quarrel are recorded in the *Lancet*, 1860, **i**, and in uncatalogued correspondence in the libraries of the Royal Society and of University College London. The *Lancet* itself did not take a hand on this occasion.

[173] Peter Mark Roget (1779–1869), physician. Author of *A Bridgewater Treatise* (1834), *Animal and Vegetable Physiology considered with Reference to Natural Theology*, and of Roget's *Thesaurus*. Secretary, Royal Society, 1827–49. See *Dictionary of National Biography*. The details of the furore are to be found scattered through many numbers of the *Lancet*, 1846, **i** and **ii**.

then taken up, and irregularities were alleged in the conduct of the referees (who happened to be Drs. Todd and Sharpey), of the physiological committee, of which Sharpey was a member, and of the Council of the Society. The leading article of 11 April 1846, was even more uncompromising. 'Honest straightforward men are aghast at the trickeries exposed in our lifting the veil from the proceedings of the medical section of the Royal Society'. Why the neglect of Grant, 'the most eloquent, the most accomplished, the most self-sacrificing, and the most unrewarded man in the profession?' Why the rejection of a paper by Marshall Hall?—personal piques, the influence of a secretary who never advanced physiology by a single step! A committee 'formed out of order by a mean trick' had ruled in favour of Beck and against Lee. The Physiological Committee 'has caused the Society to stink in the nostrils of all decent people'. The same number of the *Lancet* contained letters from Newport and Roget, denying all the charges that had been made against them, but Grant now entered the battle saying that Roget's letter was misleading, and repeating the charges of plagiarism. Matters were further complicated by the raking up of the nine-year-old quarrel between the Royal Society and Marshall Hall, complete with correspondence, to which was appended comment on the letters of Newport, Grant, and Roget. The latter was the real target—'can we wonder after this, at any proceedings, however discreditable, that have occurred in the Royal Society during his secretaryship?' On 2 May 1846, Sharpey was accused much more directly of nefarious conduct.

> It now appears that Mr. Beck's two dissections were made at the instigation of Dr. Sharpey and that the expenses attending them were defrayed, through Dr. Sharpey's influence, by University College! The dissections were partly executed in Dr. Sharpey's own dissecting room, and three years ago he had confidently predicted, that at about this time, Dr. Lee's dissections would be overthrown. The profession already knows the manner in which Dr. Sharpey endeavoured to fulfil his own vaticination; that he and Dr. Todd were the referees of Mr. Beck's paper; and that it was on their report that the Council and Physiological Committee grounded the award. As far as Dr. Sharpey was concerned, seeing the part he had taken in promoting Mr. Beck's labours, it was little better than passing a medal from his right hand to his left. It is indeed most unaccountable that a man of Dr. Sharpey's high character and reputation should ever have mixed himself up with such proceedings. . . . The press is all-powerful and WE at least *will* listen to the complaints of any sincere labourer in our own profession. . . . We will hold up such things to the scorn of the profession—to a scorn which no man shall be able to endure.

Newport meanwhile wrote deploring the raking-up of the old controversy with Hall, and begged to withdraw from the whole affair. Much ink, however, had still to flow. The *Lancet* printed correspondence between Lee and Roget and between Lee and the Council of the Royal Society, a letter from Beck disclaiming collusion with Sharpey, and a long letter from Sharpey, to which further editorial comment was appended.

The *Lancet* now maintained that the strife was really the result of the existence of two sects—the book-physiologists, and the true physiologists. Viewed in this light it was nothing extraordinary that Roget and Todd, and even Sharpey and Bowman, the physiological luminaries of the Royal Society, should be in mortal antagonism to such men as Marshall Hall and Robert Lee. The charges against individuals were repeated and the inadequacies of their explanations declaimed against. A further

leader on 30 May 1846 took up again the distribution of Royal medals in general, and ended:

> Is it to consult the true interests of the Royal Society to allow the same hands to remain at the helm after they have piloted the noble vessels so carelessly amongst shallows and quick sands? We . . . recognise one individual who has witnessed all these things. . . . Presidents have died and changed, junior secretaries have given place to new men. . . . Dr. Roget the senior secretary, the principal acting officer of the Royal Society has alone remained, and to him, chiefly, will his contemporaries and posterity look as the responsible individual during the medical cabals of his too long secretaryship.

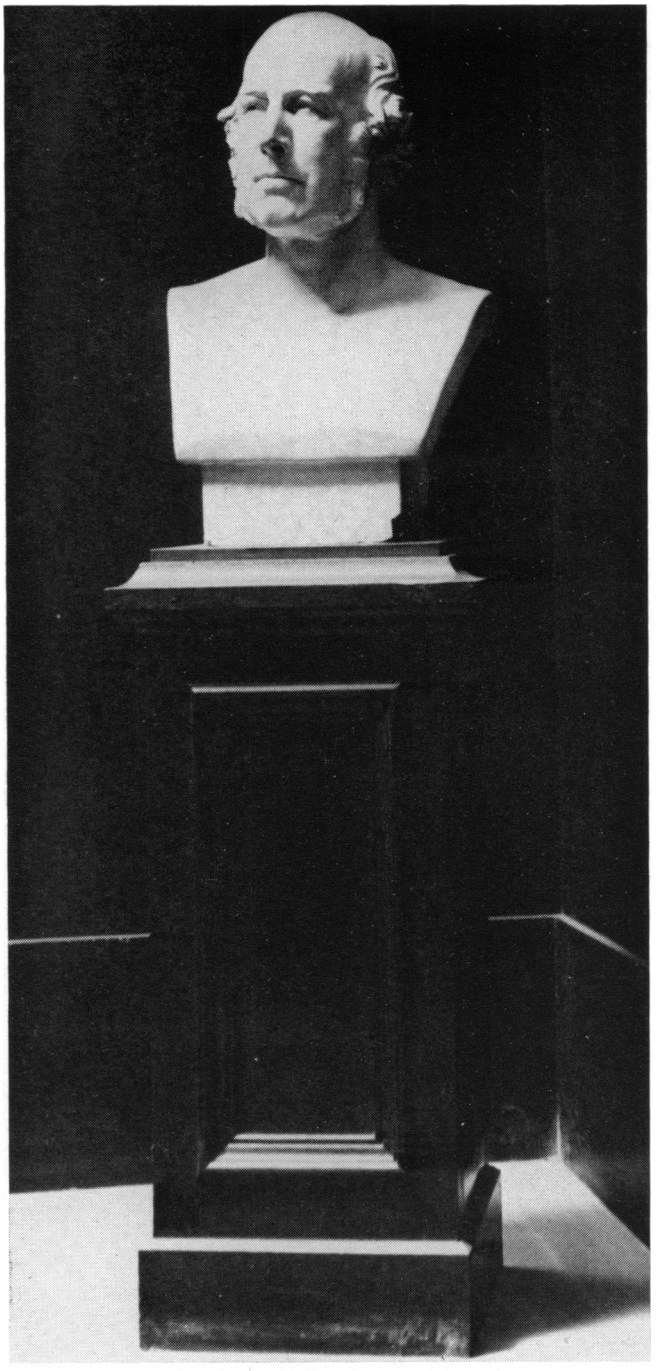
There is little doubt that the main object of the *Lancet* in writing as it did at this time was to see the end of Roget as secretary of the Royal Society, but in doing so, it generously bespattered with mud the Royal Society as such and its medical members in particular. On 6 June 1846 the tirade continued. Of the medical members of the Council for 1846, namely Roget, Sir W. Burnett and Drs. Bostock, Royle and Sharpey, 'we may ask what have they separately or collectively done to advance the theory or practice or medicine, or the sciences of anatomy and physiology. . . . Are they a flattering or even fair reflection of the British medical mind? Are their names well known in the scientific circles of Paris and Berlin?' The expected answer was undoubtedly no! Why had neither Hall nor Grant ever been on the Council or the Physiological Committee? The answer was the enmity of 'this alien secretary, who has so long been called the Mephistopheles of the Royal Society'. Roget then, ought to resign, and why should he not be replaced forthwith by Grant? The repeated mention of Grant's name throughout the whole campaign is interesting, and reminiscent of similar advocacy at the time of Sharpey's appointment, ten years earlier.

The methods chosen by the *Lancet* to achieve its aims had the effect, amongst others, of bringing Lee and Sharpey into direct conflict. Lee wrote to the *Lancet* on 21 November 1846, stating at length his case against Sharpey, and enclosed in support, a series of letters written between 1840–45 and including one from Robert Knox. Lee's list of grievances in fact contains nothing but what had already been alluded to in the editorial columns of the *Lancet*. Sharpey now felt himself forced to reply to 'this scandalous accusation', namely that having publicly expressed disbelief in Lee's work, he then employed Beck to make a dissection to provide a basis for this disbelief, and rewarded him with a Royal Medal. This, and other letters, show that Sharpey was well able to look after himself in any battle of words, and that, as so often in so many cases, his knowledge of the literature of the subject was greater than that of Lee, who professed to be an expert thereon. The argument continued into the following year, and the *Lancet* was able to devote yet another leader to a violent attack on the probity of Roget, 'the old worker of jobs and irregularities'.

It is difficult to see what anyone gained out of such a fracas, often conducted in terms which today would certainly provide grounds for libel.

The 'Cooper-Syme affair' is of interest not only because Sharpey was involved in it, but because it was one of the noisier domestic quarrels, to which, according to Bellot, was due part at least of the decline of the Medical School at University College between about 1840 and the 1860s.[174] Mr. Samuel Cooper, Professor of Surgery in

[174] Bellot, op. cit., note 18, pp. 270–1.



William Sharpey (1802–1880)
From a bust in Arbroath Public Library, Scotland.
(*By courtesy, Arbroath Public Library.*)

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the College, ended his course of lectures on 6 April 1848 with an epilogue, in which amongst other matters, he announced that he had determined to resign his Chair. He said:

> This determination was forced upon me nearly three months since—not entirely by considerations of my health nor by any inability to continue these lectures had only the same degree of assistance been conceded to me which was allowed in Mr. Liston's lifetime. . . . Suffice it to say, that my resolution was founded upon the impossibility of any agreement between me and two of my colleagues (the two who almost rule the medical end of this institution) on certain points affecting the claims of gentlemen brought up at this school, not to be forgotten in the distribution of its patronage; and also involving, as it appears to me and a large body of the profession, the character of the College itself.[175]

His resignation was duly offered and accepted. The *Lancet* found it 'truly a painful and an alarming statement' that the resignation, 'was solely owing to the feeling that one or two individuals exercised an undue influence over the Council and the Senate—an influence which permitted them to assume a dictatorial and an unjust power in all the proceedings of the College.' It ended by 'thus early inform[ing] the Council that their steps in making a new appointment will be closely watched by the profession. Another serious error . . . and the ruin of the medical department of the College will become inevitable.

'On this occasion it will not be tolerated that the gentlemen who have been educated at University College should be excluded from the vacant chair'.[176] It emphasized the point a week later by again demanding fair play for those educated at University College and Hospital, who 'hitherto . . . have been scandalously treated by persons who ought to have been their protectors'.[177] These various statements produced as might be expected, a crop of partisan letters in the correspondence columns of the *Medical Gazette* and *Lancet*, written mostly over noms-de-plume.[178] Some were rather vague, calling for the abolition of 'nepotism, favouritism, Scotticism', and denouncing as a 'gross piece of Scotch jobbing' the appointment of Syme a few months earlier to the Chair of Clinical Surgery; others hinted darkly at a repetition of the 'intrigue . . . practised some ten or twelve years ago in the case of the late professor of anatomy [Dr. Jones Quain]'. One writer, in particular, had no hesitation in openly accusing Sharpey and Richard Quain, and in slinging much mud in the process. Sharpey, it was said, had wished to oblige his friend Syme, and Quain had acquiesced through jealousy of Morton, Cooper's son-in-law, who might have expected to succeed Liston; 'the black doings of the Royal Society' were recalled, 'which has put so many of the scientific names of this generation under a cloud'. The Council was said to be unjustly partial towards Sharpey since 'he is insured £600 per annum, not trusting like the rest of the professors, to the ordinary remuneration', in contrast to Grant than whom 'there is no man who has more adorned or raised the reputation of University College by a European fame'. The writer then turned to Richard Quain, maintaining that 'it is the settled belief of the profession that Dr. Jones Quain retired in disgust, moved by annoyances similar to those which have driven away Mr.

[175] *Lancet*, 1848, i, p. 459.
[176] Ibid., p. 425.
[177] Ibid., p. 456.
[178] Ibid., pp. 482–84.

Cooper. His own brother reaped the benefit of his ejection. At the present time, Mr. Quain's position is peculiar and suspicious'. Another correspondent, this time for the accused, maintained that the Council 'in consideration of Dr. Sharpey declining a very advantageous offer made to him elsewhere, agreed to guarantee that gentleman receipts to the amount of £600 a year. In doing so the Council acted wisely for the interests of the college'. The writer is perhaps naive however in saying that 'Mr. Quain was not a candidate for his brother's chair, and would not have benefited by his brother's retirement had not the council determined to make him sole teacher of anatomy', the courses having been altered 'not to suit Mr. Quain, but, as we always understood in conformity with the wish of Dr. Sharpey'. One can hardly avoid recalling at this stage, the promulgation of the Senate's 'plan', before Sharpey's appointment and with the drawing up of which there is no reason to connect him. By this time, Sharpey and Quain, having been named publicly, were moved to reply, as did Cooper in his turn, and subsequently all three had pamphlets printed for general distribution, setting out what each deemed to be the truth of the affair.[179] Cooper was bitter and abusive—'Dr. Sharpey—with all the advantage of being a more frequent visitor in the college office it is said than in the dissecting room notwithstanding the augmentation of his salary by £150 more than he earns'—being clearly unable to forgive Sharpey for the part he played in persuading Syme, his friend and fellow-countryman, to come to London, and convinced that there was a plot to compel him to resign, 'and thus the road to the surgical chair might have been forcibly cleared for Professor Quain's triumphal installation in it—an event which, I am aware, he has long been earnestly looking for'. Both Sharpey and Quain on the other hand did not hesitate to express the opinion that Cooper's attitude arose from his desire to secure advantages for his son-in-law, Morton.

The whole business was unedifying and could have done no good to the College. From the evidence of the College records,[180] it appears plain that Cooper was at fault and that Sharpey was not in any way guilty of improper conduct. For many years, Cooper, on account of his health, had been assisted in his lecture course by Liston. Through illness Liston was forced to retire and on 18 November 1847 the Senate received from Cooper a letter stating that his health required aid such as Liston had provided, and requesting that Mr. Thomas Morton, his son-in-law, be allowed to provide this. It was moved by Sharpey and seconded by Potter that Cooper be authorised to come to an arrangement with Morton 'for the present session, with the usual intimation to Mr. Morton that such employment will not constitute any claim to preference on future occasions'. The Council replied on 20 November that they had adopted this recommendation and would consider fully, in March, the duties of the Surgical Chair. On 7 December Liston died. At the Senate meeting on 17 December, at which Cooper was present, there was passed a motion proposed by Quain and seconded by Graham:

> Resolved unanimously that in order to fill up in the manner most conducive to the interests of the College and Hospital, the vacancy in the Chair of Clinical Surgery caused by the lamented death of Mr. Liston, it would be highly expedient to secure, if possible, the services of Mr.

[179] Tracts. A 380, in Library of University College, London; and *Lancet*, 1848, **i**, pp. 508–11 and p. 531.

[180] Council and Senate Minutes (see note 41). For dates, see text.

> Syme, Regius Professor of Clinical Surgery in the University of Edinburgh . . . the Senate feel assured that no one so well fitted for the office as Mr. Syme can be brought forward by the ordinary plan of advertisement.

It was recommended that the Council issue an invitation to Syme. At the Council meeting on the next day, there was 'Read letter dated 4th December addressed by Professor Syme to Dr. Sharpey stating that he would accept the office should his services be requested'. The Senate Minute of the 17th was then read, and it was agreed to invite Syme, it being 'the desire of Council to place him in the same circumstances as Mr. Liston with respect to other lectures'. Final arrangements, however, were to be left until March. Syme was duly appointed on 8 January 1848, and so far, so good. Sharpey was entirely at liberty to make a private sounding of Syme, and the Council were legally entitled to dispense with advertisement. Nevertheless, the *Lancet* on 8 January devoted a long leading article to the matter, and to what it regarded as the misgovernment of the College and Hospital.[181] It deplored the death of Liston. The article continued:

> It is not grief that will be the effect of the new calamity which has befallen the College. . . . Disgust and indignation will more probably arise from the conduct of those bodies, than feelings of sorrow or regret, on discovering that they have advertised to all England and Europe,—nay, to the entire world, as we mentioned last week—*that there is not one English surgeon qualified to be a professor of clinical surgery in the College and a practitioner of surgery in the Hospital*, consequently, in the absence of all qualified men in London and the rest of England, they have sent to Edinburgh, and imported from *Scotland* a gentleman named SYME, whom be it observed, we do not censure for what has occurred, and whose merits it is not our intention in the remotest degree to disparage.

Since Syme was then Regius Professor of Clinical Surgery in Edinburgh, and, at the age of forty-nine at the height of his reputation as one of the greatest of British, indeed of European, surgeons, the writer was being parochial, silly and impertinent. The language throughout the article is reminiscent of that used to describe Sharpey's appointment twelve years earlier. There had been 'a post of profit as well as honour to bestow; and where has it gone? To a gentleman north of the Tweed'. The College, it was argued, could not consistently claim excellence for its products and then proceed to Scotland to find men worthy of its highest posts. 'When *Scotchmen* have any good offices to give away in Scotland, do *they* send to London . . .? No indeed! Journeys of such a character are only made from South to North, and not from North to South. . . . The Scottish influence which prevails in the Council and Medical Faculty appears to have resolved that the chief offices in the College and Hospital shall *never* be held by gentlemen who have been educated in these institutions'. It was to be feared therefore that English students would neglect such a college. Finally there was expressed concern for Mr. Morton 'most unjustly, and, in our opinion, impertinently thrust aside, to make room for a stranger from Edinburgh'. This diatribe did not escape censure in the correspondence columns for its illogicality and illiberality, and this in turn stimulated others to spring to the defence of the editor, while airing their own grievances and prejudices, similar to those evolved in the second and greater explosion triggered off by Cooper's resignation three months later.[182]

The upshot of the whole business was that Syme, too, resigned his chair after only

[181] *Lancet*, 1848, **i**, pp. 48–9.
[182] Ibid., pp. 107–9 and 164–65.

five months, and returned to Edinburgh. He stated that he had come to London, happy to assist Cooper as Liston had done, and had been embarrassed first by finding that Cooper had been allowed to select his own assistance for the course in progress, then by Cooper's resignation, and finally by the Council's request on 15 April that he undertake the whole of Cooper's duties in addition to his own. This would have interfered with the claims of practice. His mind was finally made up when on 7 May, at the distribution of prizes he

> witnessed a most painful scene in the contumelious treatment of two gentlemen standing to me in the relation of colleagues. One of these was a very old friend [namely, Sharpey] for whom I entertained the greatest respect and most sincere regard, who has devoted no ordinary talents, with no ordinary energy, during the best years of his life, to the services of a school, in his zeal for which he declined a chair of anatomy, yielding more than double the emolument of that which he now occupies besides being in other respects more advantageous.[183]

Quain wrote to the Council demanding an investigation of the charges that had been brought against him, but that body felt that Cooper had been under such misapprehension as not in any way to justify this course, recording that 'the talent, character and positions of Professors Quain and Sharpey must naturally and deservedly secure considerable weight to their wishes and opinions among their colleagues; while those very circumstances preclude the idea that they could be guilty of caballing for any unworthy purpose'.[184]

In fact, on 25 July 1848, Quain was appointed Special Professor of Clinical Surgery at a salary of £150 per annum, while Sharpey was to resign his endowment of £150 and receive that amount from fees for lectures on Descriptive Anatomy, given with Quain's consent.[185] Cooper died later in the same year, his death, according to the *Lancet*, hastened by the distress which the disturbance had caused. In this scrimmage, as in that over the award of the Royal Medals a few years earlier, it is difficult to avoid the feeling that the *Lancet*, in its zeal to expose corruption, was unduly prone to identify the possession of power with an automatic tendency to misuse that power, and to avoid also the feeling that despite protestations to the contrary, some degree of enmity was harboured against Sharpey simply because of his being the man that he was. In the case of Cooper's resignation for instance, much was made of the alleged fact that the London hospitals were, in general, shops closed to all but their own alumni, and that University College men, if not appointed to their own Medical School and Hospital would be unable to find such posts elsewhere in the capital. This certainly appears to have been the case in the earlier years of the nineteenth century, but according to Singer and Holloway was much less so by the middle of the century.[186] The *Medical Gazette* maintained that 'no such illiberal feeling [between hospitals] now prevails to any extent', and pointed out that anyhow 'the Council of University College does not overlook the claims of its alumni', ten out of fifteen appointments over the previous twelve years having been filled by former pupils.[187] This journal took the side of Sharpey and Quain. Of Sharpey's original appointment

[183] Ibid., pp. 588–9. See also Shepherd, op. cit., note 26, pp. 71–4 for another account of Syme's part in the affair, including a quotation of Christison's description of the scene at the graduation.
[184–5] Council Minutes (note 41).
[186] C. Singer, and S. W. F. Holloway, 'Early medical education in England in relation to the pre-history of London University', *Med. Hist.*, 1960, **4**, 1–17.
[187] *Med. Gaz.*, 1848, n.s. vi, 767–71.

it wrote 'we think it would have been difficult for the Council to have made a better selection', and it did not hesitate to state its opinion that Cooper's pique arose out of concern to promote his son-in-law. It printed only Cooper's original statement to his class and Sharpey's reply, refusing to handle anonymous letters, and set out to avoid 'endless controversy'.

All these episodes are interesting examples of the *mores* of the period, and illustrate Godlee's statement that 'Students of the medical history of the first half of the 19th century cannot fail to be struck by the acrimony with which discussions were carried on, the amount of jealousy which they excited, and the personal element which was constantly introduced'.[188] The same point is made abundantly clear in Shepherd's recent book on *Simpson and Syme of Edinburgh*.[189]

VIII. LAST YEARS: THE ANTI-VIVISECTION MOVEMENT

It will have become plain to the reader that Sharpey was not a man who gave much of himself away in his correspondence—at least as far as we can judge from extant letters. Even when writing to his friends, he stuck strictly to the business in hand. In a letter to Dr. Stenhouse he wrote: 'Among my many faults, I fear I have to reckon the want of a due share of enthusiasm . . . but your project could I think recommend itself to ever so cool a critic'.[190] There are in the Sharpey-Schäfer papers however, twenty-three letters written between 1873 and 1880 by Sharpey to Schäfer, his pupil and close friend, in which the mood is more informal. He talks of his increasing blindness and of his operation, is interested in Schäfer's visits to various continental laboratories, refers to his nephew William Colville, a medical officer stationed in Baghdad, who appears to have had much affection for the old man ('to tell me about Uncle Sharpey was above all things what I desired, for though he writes me very regularly, he tells me little of himself. Uncle Sharpey as perhaps you have grasped, is all in all to me, all I live for, all I care for.')[191] and makes occasional mention of his boyhood in Arbroath. These letters, and sundry remarks passing between Michael Foster and the Huxleys, between Burdon Sanderson and Schäfer, and between Foster and Schäfer, aid us a little in imagining Sharpey at least in his last decade. They show that he retained his intellectual interests, remained vigorous in mind until the very end (the last letter to Schäfer was written six weeks before his death), and held the affection of his younger colleagues.

By about 1870, however, Sharpey was beginning to show some physical signs of ageing; in particular he became increasingly blind from bilateral cataract. And whereas, four years earlier, in the interval between Harley's resignation and the appointment of Michael Foster to teach experimental physiology, Sharpey had conducted this class himself, in addition to his lectures, Burdon Sanderson, who succeeded Foster on the latter's translation to Cambridge in 1870, agreed also to give two-fifths of Sharpey's lectures.[192] In May 1873, an operation for cataract was performed and letters written later in that year refer to an improvement in the vision

[188] Godlee, op. cit., note 71, p. 31.
[189] Shepherd, op. cit., note 26.
[190] Uncatalogued letter 8 April 1861 in the Library of the Royal Society, London.
[191] Wm. Colville to E. A. S. dated 23 January 1878, from Baghdad. In the Sharpey-Schäfer papers.
[192] J. Burdon Sanderson to E. A. S. dated 1870. In the Sharpey-Schäfer papers.

of the operated eye, while that of the other grew more and more dim. In 1874, Sharpey resigned and was succeeded by Sanderson, who, following a redefinition of the duties of the chair, became first Jodrell Professor of Physiology.

Nevertheless, despite age and some degree of infirmity, Sharpey did not lose touch with the scientific world. He was on terms of intimacy with Michael Foster and Burdon Sanderson, and not infrequently stayed at their homes. Both men often sought and received his advice. The Royal Commission on Scientific Instruction and the Advancement of Science, of which he was a member, issued seven long reports and one supplement between 1871–75. He continued, also, to be a member of the Medical Council.

At this time, too, Sharpey was deeply disturbed by the increasing agitation against experiments on living animals, and by the various attempts at legislation to control the practice. A great deal has been written on the subject by those committed to one side or the other, but the deeper origins of the anti-vivisection movement in Britain seem to have received little dispassionate attention from historians. The idea that men should be kind to animals did not, of course, originate in the nineteenth century, although until the reign of George IV there was no legislation for their protection. The eighteenth-century 'Revival' was certainly important in bringing about a change in the attitude of the general public to the cruel treatment of animals. The rate of change of opinion, however, was accelerated in the intellectual climate that followed the reaction engendered by the Napoleonic wars. G. M. Young's 'boy born in 1810' entered manhood 'at every turn controlled, and animated, by the imponderable pressure of the Evangelical discipline and the almost universal faith in progress'.[193] Evangelical piety and faith in progress fused and found characteristic expression in a humanitarianism which included the humane treatment of animals, and which was activist in that it sought and accomplished reforms of very great benefit to society, such as the abolition of slavery, and the various Factory Acts. This sentiment of humanity, although focussed in the Evangelical movement, transcended party and creed to become a political force of great magnitude, whose adherents used the methods of agitating and moulding public opinion which had been so successfully employed in the anti-slavery campaign, and which were, as Trevelyan says, 'imitated by the myriad leagues and societies—political, religious, philanthropical and cultural—which have ever since been the arteries of English life'.[194] Evidence of a general sympathy for, and a desire to mitigate the sufferings of dumb animals seems however hardly sufficient to account for the growth of the anti-vivisection movement in Britain. After all, it could very reasonably be held that much cruelty was implied in addiction to field sports and in many agricultural procedures, but these were scarcely referred to in the attacks which were directed specifically at the laboratories and at the practice of experimental physiology, and they were exempted from interference in the various pieces of proposed legislation which culminated in the Act of 1876. The arguments used at the intellectual level on which much of the discussion was conducted, reflect in the minds of the medical and scientific protagonists of animal experiments as well

[193] G. M. Young, *Victorian England: Portrait of an Age*, London, Oxford University Press, 1964, p. 1.

[194] G. M. Trevelyan, *English Social History*, London, The Reprint Society, 1948, p. 501.

as of their opponents,[195] anxieties and conflicts the tangled roots of which run back through the eighteenth century—to the confusions, theological and other, generated by the Cartesian representation of animals as automata, to the diatribes against human pride, such as are to be found in Pope's *Essay on Man,* for example, or in Swift, and to the teachings of Wesley and others on animal soul and immortality, their cumulative effects further reinforced by the apparent implications of the growth of science, especially biological science, throughout the nineteenth century. The subject is a large one, which requires extended treatment in its own right.[196]

Public concern mounted steadily through the 1860s, and in 1871 the British Association at its annual meeting issued a number of recommendations by which it felt that experimentation on animals ought to be guided. Anaesthetics should be used whenever possible. Painful experiments should not be performed merely for teaching purposes, and, if necessary at all, should be done only by qualified persons in suitable places. Experiments on living animals should not be done merely to acquire surgical dexterity.

Two events, it would seem, made the prospect of legislation of some kind inevitable. These were the publication of Sanderson's *Handbook for the Physiological Laboratory*,[197] and the distorted reporting in the lay press of David Ferrier's experiments on the brain, carried out at the West Riding Asylum. In 1875, two bills were introduced into Parliament, one into the Lords, by Lord Henniker, on 4 May,[198] and the other into the Commons on 12 May, by Lyon Playfair.[199]

Sharpey's reactions to these proposals are to be found in letters to Sanderson and to Schäfer.[200]

> Dear Sanderson, dated 19/5/75 at 50 Torrington Square.
>
> You know I have grave doubts of the wisdom of attempting to check the abuse of Vivisectoni by means of special legislation—I should trust to the salutary operation of other influences. But, if the attempt is to be made, I should look upon Dr. Playfair's Bill as worthy of approval in its general tenour. It appears to me however that the terms in which it is drawn (as it now

[195] This is clear from the minutes of evidence presented to the Royal Commission of 1875. It is difficult, reading the Report of the Commission and the relevant columns of Hansard to agree with Lady Longford (in *Victoria R.I.*, p. 406) that, but for the Queen, Lord Carnarvon's bill would not have become law—Disraeli's dismissal of the arguments against vivisection notwithstanding!

[196] I have found most helpful the paper by L. G. Stevenson, 'Religious elements in the background of the British Anti-vivisection Movement', *Yale J. Biol. Med.*, 1956, **29**, 125–57.

[197] E. Klein, J. Burdon Sanderson, Michael Foster, and T. Lauder Brunton, *Handbook for the Physiological Laboratory*, 2 vols., ed. by J. Burdon Sanderson, London, J. & A. Churchill, 1873. The work was dedicated to Sharpey as follows:

> 'Dear Dr. Sharpey,
>
> To you, who have been these many years the friend of physiologists throughout the world, and who, by your original work, by your teaching, by your generous aid and judicious counsel, have been the mainstay of physiology in England, we desire to dedicate this attempt to promote the study of our science.
>
> Acceptit as a token of our personal regard, as well as of the high value we set on your life-long labours.
>
> Your devoted Friends,
>
> MICHAEL FOSTER
> J. BURDON SANDERSON,
> T. LAUDER BRUNTON,
> E. KLEIN.'

[198] A Bill intituled An Act for regulating the Practice of Vivisection. H. L. 85, 1875.

[199] A Bill to Prevent Abuse and Cruelty in Experiments on Animals, made for the purpose of Scientific Discovery. HC. 163. 1875.

[200] Imperial College of Science and Technology, University of London. List of the papers and correspondence of Lyon Playfair, First Baron of Playfair of St. Andrews, preserved in the Imperial College Archives, 1967, 623; and W. S. to E. A. S. dated 21 May 1875 in the Sharpey-Schäfer papers. Schäfer was in Germany at the time.

appears in the newspapers) require some amendment.
First: the Bill authorizes the performance of experiments intrinsically of a painful nature provided the animal is rendered insensible, but even then only for the purpose of 'new scientific discovery'. By this restriction it is no doubt intended to prevent the exhibition of such experiments for the purpose of scientific instruction, and also the private repetition, for the sake of verification, of experiments already known. Now to make such a prohibition absolute and enforce it in all cases, notwithstanding the condition of insensibility appears to me to be an unreasonable interference, and calculated seriously to affect the advancement of science in this country. I think it would be wiser to use the words 'for a scientific purpose.'
Then, secondly; it is proposed to authorise persons acting under a license to conduct painful experiments even on creatures still capable of feeling pain, but then, again, solely for the purpose of 'new scientific discovery.' Now I apprehend that an enactment in the terms proposed would be found extremely difficult of application—You know that there is no more fruitful source of dispute than the question of scientific discovery, and yet, in a given case, this question of 'novelty' would have to be settled by a Court of Law, and doubtless on the most conflicting, although at the same time bona fide evidence. A penal enactment so difficult of judicial application might operate unjustly but more probably would not operate at all. Instead of the words used in the Bill I would substitute 'for the purpose of scientific investigation, and for that purpose only.'

Yours sincerely,
W. Sharpey

Sanderson duly informed Playfair of these views.[201] To Schäfer, Sharpey wrote as follows:

dated 21.5.75 at 50 Torrington Square

There is still much clamour and agitation about vivisection. Two bills have been introduced into Parliament in order to prevent the abuse of vivisection—one by Ld. Henniker in the Lords, the other by Playfair in the Commons. The first would enforce very restrictive and vexatious regulations: Playfair's is less meddlesome but unreasonably restrictive, and I am satisfied its restrictions would be quite ineffective for their purpose—indeed it would be impracticable to obtain a conviction under its provisions. So much the better perhaps you will say—but the Lawyers in the House of Commons will never allow such a measure to become Law. I don't believe that either Bill will go on. A year's more reflection would be very salutary—and then if there must be legislation it should be dealt with by the Government in consultation with men of science.

Sharpey's belief that neither bill would become law was in fact proved correct and both were withdrawn on the appointment, a little later that summer, of a Royal Commission under the chairmanship of Viscount Cardwell. Incidentally, there seems to have been some misunderstanding amongst the interested parties in that Playfair maintained in the House of Commons a year later that his bill 'was in reality prepared by very eminent physiologists among whom I may mention Mr. Darwin, Mr. Huxley, and Dr. Burdon Sanderson'.[202] Huxley, however, wrote to Darwin on 5 June 1875, that 'Playfair seems rather disgusted at our pronunciamento against the bill, and he declares that both Sanderson and Sharpey assented to it. What they were dreaming about I cannot imagine. To say that no man shall experiment except for purpose of original discovery is about as reasonable as to ordain that no man shall swim unless he means to go from Dover to Calais'.[203] Sharpey's opinion of the bill has already been quoted, and the evidence given to the Commission by Sanderson and by Darwin suggests that their opinions were sought only at a preliminary stage of its preparation. Darwin said that 'the Bill itself did not exactly express the conclusions at which after

[201] Lyon Playfair papers (see note 200). 123.
[202] Hansard, vol. 231, col. 923–4.
[203] *Life and Letters of T. H. Huxley*, op. cit., note 153, Vol. I, p. 438.

consultation with several physiologists, we arrived. I apprehend that it was accidentally altered'.[204] Huxley was thus being rather quick in jumping to a conclusion.

Sharpey gave evidence at length before the Royal Commission on 6 July, and the commissioners were clearly impressed by what he had to say.[205] He was in no doubt where he stood. Experiments on living animals were 'absolutely necessary for the progress of the science of physiology'. This he illustrated by a long historical disquisition in which he quoted the work of Harvey, Stephen Hales and Charles Bell. Physiology in turn was 'one of the great foundations of all rational medicine'. It was the plough rather than the reaping machine, 'operating impalpably on the mind of the practical physician'. On the other hand, all experiments made without a clear perception of what it was desired to learn and without some experience in methods, were of little or no value. Anaesthetics had made a great difference, but there were such experiments where great and protracted pain might be necessary. He felt that there was no security that pain was abolished if only curare was used, but that the question was not yet settled, despite the views of Claude Bernard. Students, he thought, must see experiments demonstrated, and he saw no need for restriction, provided anaesthesia were used. He spoke frankly about Magendie's experimental demonstrations, one of which he had witnessed on his first visit to the Continent.

> I may mention to the Commission that when I was a very young man studying in Paris, I went to the first of a series of lectures which Magendie gave upon experimental physiology, and I was so utterly repelled by what I witnessed that I never went back again. My objection in these experiments was two-fold. In the first place they were painful (in those days there were no anaesthetics) and sometimes they were very severe, and then they were without any sufficient object. As an example I may tell the Commission that Magendie made incisions into the skin of rabbits and on other creatures to show that the skin is sensitive. Now surely all the world knows the skin is sensitive, no experiment painful or without pain is needed to prove that. Then several of the rest of the experiments, which he made were of a similar character and he put the animals to death finally in a very painful way. The consequence was that I never went back to that course of demonstrations.

A little later, he said that although Magendie did aid the advance of physiology

> some of his experiments excited a very strong feeling of abhorrence, not in the public merely, but amongst physiologists. There was that, I was going to say famous, it might rather have been called infamous experiment of his upon vomiting. . . he substituted a pig's bladder for the stomach of a dog he had cut out and then filled the bladder with water, and induced vomiting by injecting an emetic into the veins; and the object of that was to show that the stomach, although it has muscular coats, was passive in vomiting, and that it was emptied merely by the pressure of the muscular walls of the abdomen, and diaphragm, and the experiment, besides its atrocity, was really purposeless because it merely proved that if a bladder filled with water were compressed when the orifice of it was left free, water would come out.

These opinions are interesting. It may be that they explain Sharpey's barbed comments on Magendie made from time to time in the course of his lectures. We should remember, too, that more than half of Sharpey's scientific life was spent in the pre-anaesthetic era, and possibly—although of course we have no proof of this—his emotional response to animal experiments under such conditions closed off, for

[204-5] 'Report of the Royal Commission on the Practice of subjecting live animals to experiments for scientific purposes; with minutes of evidence and appendix.' London, H.M.S.O., 1876. Sharpey's evidence is given on pp. 318–30, paras. 386–596. For Darwin's statement, see p. 234, para 4665. See also p. 123 para 2314, for Burdon Sanderson's opinion.

him, a whole field of physiological investigation.

All in all, Sharpey thought that most necessary experiments could be done painlessly, and that there were no abuses of vivisection in England. He regarded the whole business as simply 'one of these excitements in the public mind, and among a certain class of people particularly, that from time to time occur', and made sarcastic reference to 'the unreasonable zeal of some, who are commonly called humanitarians, for interference'. He was prepared, reluctantly, to agree to some restriction on painful experiments, namely that they be performed only by competent persons, under licence.

The giving of evidence to the Royal Commission was followed by a visit to Michael Foster, when the two men did some experiments on curarized frogs.[206]

> Foster had tried the question as to curare thus; He passed a ligature under the sciatic plexus in two frogs A and B and tied off the lower half of the body in both. Both jumped about freely. He then curarised the forepart of the body in B. After a time he irritated the post. limbs and drew them out in both. A. retracted the leg suddenly. B. rather slowly—but we had to give up as the nerves had evidently suffered. Of course B could not move its forelegs, but on pinching the skin either of *fore* or *hind*legs it moved the latter. We tried strong vinegar and dilute Sulph. acid. Foster maintained that the succeeding movement ensued much more slowly than in an unpoisoned frog. He then removed the brain of B down to the optic lobes and the result of stimulation was the same as before. The experiment did not satisfy me. All I could say was that the curarised frog reacted as much after as before the removal of its cerebral hemispheres. When before the Vivisection Committee I felt bound to say that in the present state of our knowledge I did not think that curare could be accepted as an anaesthetic.

Following the Report of the Royal Commission two further bills were introduced into Parliament. One of these, Holt's bill,[207] was totally prohibitory and failed to get a second reading. The other introduced by Lord Carnarvon on 15 May 1876,[208] and into the Commons on 18 July received the Royal Assent on 15 August after some modification in the committee stages.

Sharpey remained in close touch with these developments. Sanderson wrote to Playfair on 22 May to say that he had read Carnarvon's bill with some surprise, that he had consulted Sharpey and Michael Foster, and that all were agreed it would completely arrest physiological science because the conditions imposed were impossible.[209] He enclosed a list of objections (in Sharpey's hand) to Clause 5 (prohibiting *in toto* the use of cats and dogs) in which particular stress was laid on the unsuitability of herbivores for many investigations especially in relation to digestion. Sharpey commented to Schäfer on 25 May:[210]

> dated 25/5/76 at 50 Torrington Square
>
> Lord Carnarvon's Bill is in many respects very objectionable—but it is not so bad in essence as in form. If you study it attentively you will see that it takes the shape of a strictly prohibiting bill—but then by means of provisos, one may 'certify' himself or at least get certified out of its most oppressive enactments. The subject comes before the Medical Council today in a motion by Lister and I doubt not a Committee will at once be set at work towards its amendment. Sanderson has been here; he tells me that the Presidts of the R.S., of the Coll. of Physicians and Coll. of Surgeons are to wait on the Duke of Richmond for a like purpose. The clause requiring that the experiment must be 'absolutely necessary for the advancement of medicine and the

[206] W. S. to E. A. S. dated 29 July 1875. In the Sharpey-Schäfer papers.
[207] A Bill to make more effectual provision for the Prevention of Cruelty to Animals. H.C. 168, 1876.
[208] A Bill intituled An Act to amend the Law relating to Cruelty to Animals, H.L. 85, 1876.
[209] Lyon Playfair papers, op. cit., note 200, 124.
[210] W. S. to E. A. S. dated 25 May 1876. In the Sharpey-Schäfer papers.

alleviation of Human Suffering' would interfere with schools such as Cambridge and Oxford—where the studies are not directly medical. Again the proposed bill would allow any restless associate or individual to harrass physiologists by vexatious prosecutions—who for all we know would not be slow to engage in such. There must be some protection against this, such as requiring the concurrence of the Attorney General in instituting any prosecution etc. The Parliamentary Committee of the Brit. Med. Assocn. is to meet tomorrow on the subject and have invited me to attend, as I certainly shall do if I am able.

In fact Carnarvon himself gave way, to some extent, to pressure from the physiologists, over Clause 5, by having inserted in the Committee stage in the Lords an additional clause permitting the use of dogs and cats on special certification.[211] Similarly in the Commons, on the motion of Mr. Assheton Cross (the Home Secretary), a clause was inserted whereby prosecutions under the Act might be instituted only with the written assent of the Secretary of State.[212] In both of these amendments it is perhaps permissible to see Sharpey's influence. On the other hand, a further new clause moved by Cross, which sought to exempt 'cold-blooded' animals from the provisions of the Act, failed, inasmuch as W. E. Forster's amendment to substitute 'invertebrate' for 'cold-blooded' was accepted, thus bringing experiments on the frog under surveillance. Lyon Playfair bluntly termed this a breach of the understanding between the Home Secretary and the medical men.[213]

It is well known that the threat of legislation following the Report of the Royal Commission was an important factor in the foundation of the Physiological Society. Sanderson called a meeting at his house on 31 March 1876, to which eighteen gentlemen came, Sharpey among them, 'the number including nearly everyone who is actually engaged in physiological research in England'.[214] The meeting was adjourned until 26 April, when a draft constitution was submitted and after emendation, adopted as the Rules of the Society. At the next meeting, on 5 May, it was resolved that the Rules be printed and sent with invitations to founder-membership to thirty-six physiologists of whom two were to be Honorary Members, Charles Darwin, F.R.S. and William Sharpey, F.R.S. Sharpey subsequently attended a number of meetings of the Society, the last apparently on 10 January 1878.[215]

Apart from his blindness, Sharpey remained in fairly good health until almost the end of his life. He spent his last two winters in Hastings, returning to 50 Torrington Square in March. His niece and housekeeper, Miss Colville, had died in the early summer of 1878, but his half-sister, Miss Arrot, remained alive in Arbroath and with her he seems to have spent his summer holiday. His mind stayed alert. In a letter to Schäfer, dated 4 March 1879, he discussed the custody of his own library, and that of Robert Grant, and a year later (15 February 1880) he referred to the forthcoming edition of Quain—edited now by Thomson, Shäfer and Thane—with the suggestion that Thomson could '*with great advantage* retrench', it being best not to increase the size of the book! In this letter there is noticeable for the first time some deterioration in the handwriting. The last of the extant letters to Schäfer is dated 26 February 1880, and here Sharpey does permit himself a reference to his own health. 'For

[211] Hansard, vol. **230,** col. 125–6.
[212] Ibid., vol. **231,** col. 1151.
[213] Ibid., col. 1152.
[214] Op. cit., note 93, p. 104.
[215] For details in this paragraph, I am indebted to Sharpey-Schäfer's *History of the Physiological Society* (see note 6).

myself', he writes, 'my condition varies a good deal. Some days I feel stronger and can take a *slow* walk for an hour and a half—on other days this is fatiguing. My hearing is now *very* dull—my head and gait sometimes unsteady'.[216] He returned to London at the end of March, convalescent from bronchitis, and collapsing on the morning of 11 April, died that same evening attended by Allen Thomson, Sanderson, Marshall and Ringer.[217] His body was taken to Arbroath and lies buried in the shadow of the great Abbey, magnificent even in its ruin.

It has for a long time been unfashionable to award much credit in universities to those who merely teach and do not at the same time actively pursue research. It is interesting that Michael Foster, himself a teacher rather than a research worker, should write to Schäfer: 'For heaven's sake—don't do too much lecturing—it *destroys a man* as I know—*I* have been *driven* to lecturing from my youth upward—you are not *obliged* to—Don't do it—give all your energy to research'; and again: 'Take warning by me who have been writing and teaching until all the juice has gone out of me and *I am worth nothing* more'.[218] Foster might be thought, writing thus, to contradict by implication what he had said elsewhere in appreciation of Sharpey's teaching, and to be tempering the affection he clearly bore his old teacher. Billroth, in his penetrating book,[219] divides teachers into two classes, those who excel in the formal presentation of their subject, in pedagogy in the best sense, and those who, possessed of creative minds, draw men to themselves and found schools. It is clear that his admiration was largely reserved for this second group. William Sharpey was not a great scientist; it is equally clear that he was a great and inspiring teacher. Billroth, however, defines within his first class, a subgroup, and it is here, perhaps, that Sharpey finds his place, amongst 'teachers who, without being themselves productive, possess marked receptive and reproductive ability, and who with all their tendency to external formalization, are inwardly alive. Training and spirit are in them united with a great power of assimilating their daily acquisitions and with a sort of eager and active enthusiasm for their work, as scholars and teachers, qualities that are normally possessed only by creative minds'.

Even more appropriate, as an epitaph, might be the remark made of Rutherford by Kapitza: 'The history of science tells us that an outstanding scientist is not necessarily a great man, but a great teacher must be a great man'.[220]

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[216] The letters mentioned are in the Sharpey-Schäfer papers.
[217] *The Arbroath Guide*, Saturday 17 April, 1880.
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[219] T. Billroth, *The Medical Sciences in the German Universities*, trans. from the German by W. H. Welch, New York, Macmillan, 1924, pp. 246–47.
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