

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

by

D. W. TAYLOR

## PART I

### I. INTRODUCTION

THE TITLE-PHRASE was used by E. A. Schäfer,<sup>1</sup> one of Sharpey's latest and most distinguished pupils, who throughout his life acknowledged his debt to his old teacher and who eventually added the name of Sharpey to his own.<sup>2</sup> Such a phrase may be historically suspect but it represents an opinion subscribed to by many others, not only his pupils and colleagues. Burdon Sanderson, who succeeded him, wrote: 'If I, or any of the men I have mentioned, were asked to what circumstances the unquestionable productiveness of the University College School in scientific men is due, all would I think unhesitatingly attribute it to the influence of one man, Dr. Sharpey.'<sup>3</sup> Much later, A. V. Hill said of him: 'by Sharpey's example, other medical schools were encouraged to start proper laboratories and adequate instruction in Physiology . . . to his fine judgement of men and to his power of inspiring them to original work, the present high position of British Physiology is due . . .'.<sup>4</sup> The *British Medical Journal* in its obituary notice, stated that 'for years he was the greatest teacher of anatomy and physiology in the country, occupying a position here equal to that held by Johannes Müller in Germany.'<sup>5</sup> In his *History of the Physiological Society*, Schäfer (by then, Sir Edward Sharpey-Schäfer) wrote that

during a period when other experimental sciences were rapidly progressing, Physiology in this country could show no names worthy to be mentioned with those of Magendie, Bernard, Müller, Helmholtz or Ludwig. . . . In one place only—University College London—was a lamp kept burning; the lamp was that of William Sharpey. . . . He was a great teacher: all his pupils have testified to this. . . . He had a gentle nature, a genial disposition and sound judgment. He was devoted to the interests of the institutions with which he was connected, especially University College and the Royal Society. He spared no pains to promote the advancement of Physiology. . . . He knew and was known to everybody in the world of science and medicine. Wise in council, he exercised a very considerable influence on medical education, and was universally esteemed by members of the profession.<sup>6</sup>

<sup>1</sup> E. A. Schäfer, 'Reminiscences of Professors. William Sharpey', *Univ. Coll. Gaz.*, 1902, 3, 238–39.

<sup>2</sup> 'I did this partly on Jack's account [his son John Sharpey Schäfer, was killed in the 1st World War], partly because it was the name of my old teacher and master in Physiology—the best friend I ever had.' Sir Edward Sharpey-Schäfer, Private diaries (1918). MS in Sharpey-Schäfer papers in the Library of the Wellcome Institute of the History of Medicine.

<sup>3</sup> J. Burdon Sanderson, draft of address to Royal Medical Society of Edinburgh. MS in Burdon Sanderson papers in the Library of University College London.

<sup>4</sup> A. V. Hill, University College London Trust Funds Book, in the College Archives.

<sup>5</sup> *Brit. med. J.*, 1880, i, 606.

<sup>6</sup> Sir Edward Sharpey-Schäfer, F.R.S., *History of the Physiological Society during its First Fifty Years 1876–1926*, Cambridge University Press, 1927, pp. 1–2 and 17–18.

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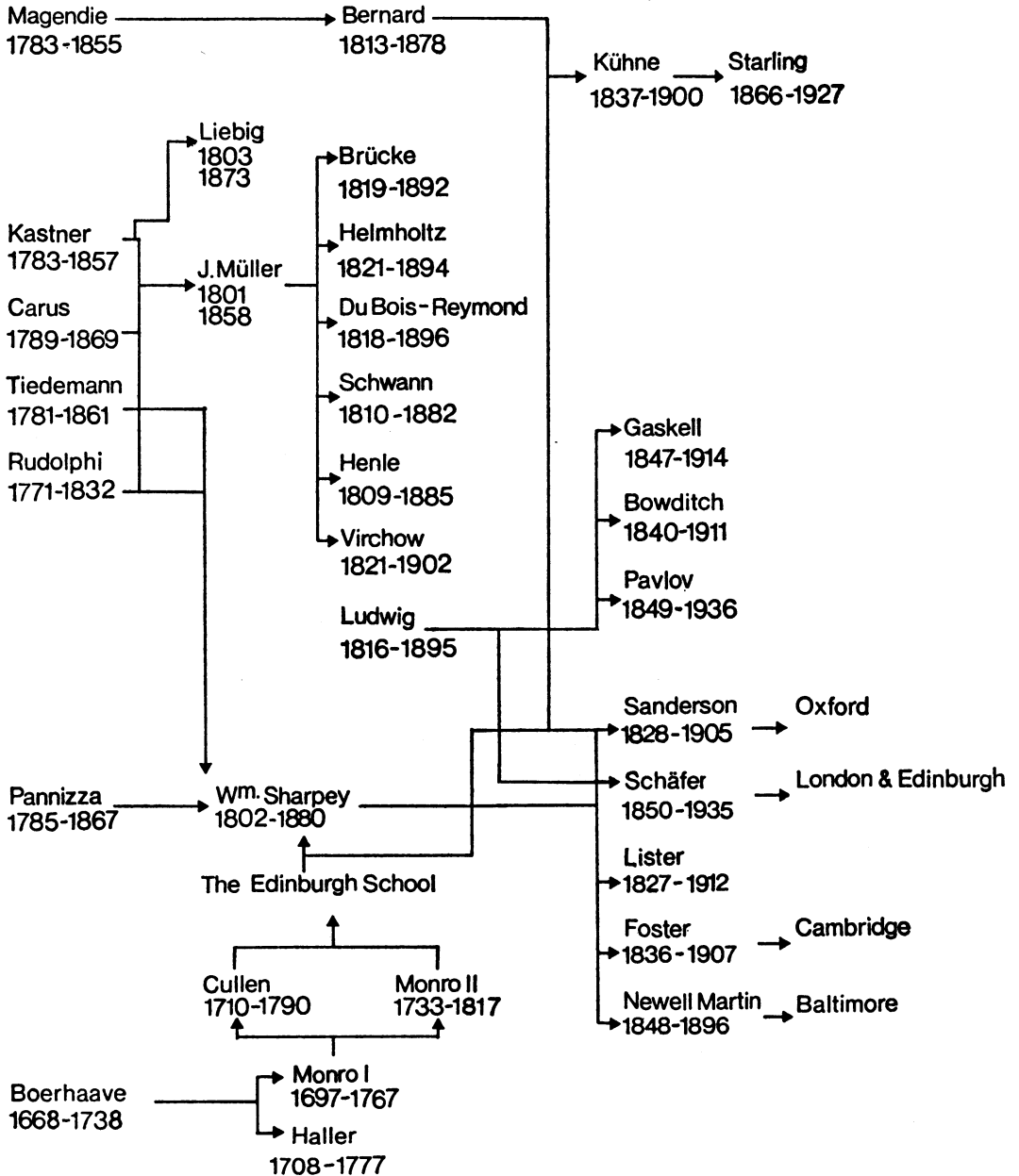


Diagram to show the more important 'master-pupil' relationships in nineteenth-century physiology, and Sharpey's position therein. (N.B. Burdon Sanderson was a close colleague, but never a pupil *stricto sensu* of Sharpey).

Nevertheless, despite such ample testimony to his many great qualities and immense influence, there appears to be no extended study of Sharpey.

The outlines are to be found in the obituary notice in the *Proceedings of the Royal Society*,<sup>7</sup> unsigned, but written in fact by his erstwhile colleague and continuing friend, Allen Thomson;<sup>8</sup> in the article in the *Dictionary of National Biography* written by D'Arcy Power, who in his youth knew Sharpey as an old man;<sup>9</sup> and in Sharpey-Schäfer's *History of the Physiological Society*.

William Sharpey was born, at Arbroath, on 1 April 1802, the posthumous son of Henry Sharpy [sic]<sup>10</sup> a shipowner from Folkestone, who had moved to Arbroath, and of his wife, Mary Balfour, who later married again to become the wife of Dr. William Arrot, of Arbroath.<sup>11</sup> Sharpey was educated at Arbroath and at the University of Edinburgh which he entered in 1817, spending one year in the Faculty of Arts studying Greek and Natural Philosophy, before transferring to Medicine. In 1821 he obtained the diploma of the Royal College of Surgeons in Edinburgh. In 1823 he graduated M.D.(Edin.) and in 1830, became F.R.C.S.E. From 1831–36 he taught Anatomy extramurally in Edinburgh. In 1836 he was appointed to the Chair of Anatomy and Physiology in University College London, from which he retired in 1874. Sharpey became a Fellow of the Royal Society of Edinburgh in 1834, and in

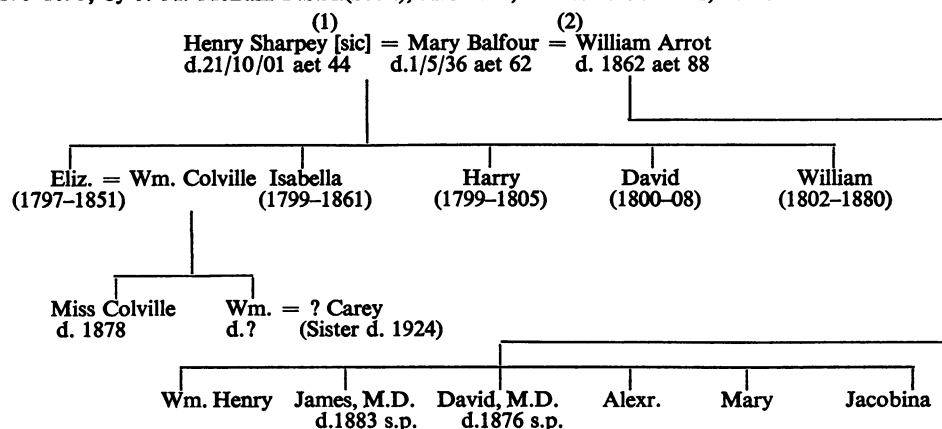
<sup>7</sup> Obituary notice, *Proc. Roy. Soc.*, 1880, 31, xi–xix.

<sup>8</sup> This is clear from a note in the hand of E. A. S. in the Sharpey-Schäfer papers. For Allen Thomson (1809–1884), biologist, see *Dictionary of National Biography*.

<sup>9</sup> Mr. C. R. Rudolf, the present Secretary of the Hunterian Society informs me that D'Arcy Power told him of how finding Sharpey 'half starved and living alone and forlorn in some attic' he was the means, with the assistance of a few friends, of enabling him to end his days in comfort. Sharpey was in fact awarded a pension of £150 p.a. by the Gladstone administration in 1874. (*Brit. med. J.*, 1874, i, 354). He left his library to University College, and by his will, dated 12 August 1878 he left £800 Great Indian Peninsular Railway Stock to add to the Sharpey Scholarship Fund, the trust for which had been accepted by the Council on 1 July, 1871, and the object of which was to be 'the promotion of Biological Science', more especially 'by the encouragement of the practical study of Physiology in the College.' (University College London Trust Funds Book).

<sup>10</sup> The name Sharpy, or Sharpey, appears to be very uncommon. There exists, however, in the British Museum a pedigree of the family at Sharpeith, written in the seventeenth century. (Pedigrees and Arms of Kentish Families, B.M. MS 5509) and the name may have been formerly less rare, at least in Kent. Sharpey himself died unmarried, and no near relation survived him long. None bore his name.

<sup>11</sup> Genealogical information from tombstone in Arbroath Abbey churchyard re-erected by W. S. in 1873, and from *Eminent Arbroathians; being sketches historical, genealogical and biographical 1178–1893*, by J. M. McBain F.S.A.(Scot.), Arbroath, Brodie & Salmond, 1897.



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1839 was elected to the Fellowship of the Royal Society. He served on its Council, and was Secretary from 1853–72. He was at various times a member of many other official and learned bodies; in particular he was for fifteen years a Crown appointee on the General Medical Council. As a young man, Sharpey travelled extensively on the continent of Europe. He spent nearly a year, from 1821–22, in Paris, studying under Dupuytren and Lisfranc, and went there for a further year after his graduation in 1823. From 1824–6 he was in practice with his stepfather in Arbroath, but, having only then, according to the Royal Society obituary notice, ‘finally resolved not to embark in medical practice, but to devote himself to anatomical and physiological studies, for which he had long had a predilection’,<sup>12</sup> he set out on a much more extensive European pilgrimage and spent two years, from the autumn of 1827 to 1829, successively in Italy, in Austria and in Berlin. He spent a further three months in Berlin in the summer of 1831. His familiarity with French and German scientific literature, and the European contacts and friendships which he made and maintained, were acknowledged by his contemporaries to have been amongst his greatest assets. He gradually gave up original work in favour of teaching and administration and the encouragement of others in physiological research, in all of which activities he appears to have been eminently successful. He remained mentally and physically vigorous until about his seventieth year, after which increasing deafness and bilateral cataract led to his retirement from his Chair in 1874. He died in London on 11 April 1880, and is buried in the churchyard of Arbroath Abbey.

These short accounts that we have of Sharpey<sup>13</sup> fail to answer a number of questions. For one whose influence was so great, he appears to have published remarkably little original work. Indeed, near the end of his life he gave up any claim to priority in the discovery of those fibres which are a feature of the fine structure of bone, and to which his name had been attached by his friend Kölliker.<sup>14</sup> D’Arcy Power states that he was remembered mainly as a teacher, and for his services to the Royal Society. According to Sharpey-Schäfer ‘his lectures were largely anatomical’ and ‘he carried out no physiological researches’.<sup>15</sup> Allen Thomson, on the contrary, says that ‘from his extreme fastidiousness . . . he was much too sparing of authorship. Accordingly much of the original observation and thought on scientific subjects which cost him prolonged labour and involved much research, was made known by him only through his lectures or was published in a more or less fragmentary form in connection with such systematic works as Baly’s Translation of Müller’s Physiology, and Quain’s Anatomy. . . .’<sup>16</sup>

Again, although we are told by Thomson that Sharpey had become well known, both in Edinburgh and at a distance, as an investigator and teacher,<sup>17</sup> Bellot says that his appointment to the University College Chair caused surprise,<sup>18</sup> and reference to the medical journals of the day shows this to be very much an understatement. It

<sup>12</sup> Obituary, *op. cit.* note 7, p. xi.

<sup>13</sup> Only one other is known to me, that by Broucher in *Ann. med. Hist.*, 1927, 9, 124–28, which contains no new information.

<sup>14</sup> See Bibliography, No. 15; the draft MS is in the Sharpey-Schäfer papers.

<sup>15</sup> Sharpey-Schäfer, *op. cit.* note 6, p. 1.

<sup>16</sup> Obituary, *op. cit.* note 7, pp. xi.

<sup>17</sup> *Ibid.*, p. xii.

<sup>18</sup> H. Hale Bellot, *University College London 1826–1926*, London, University of London Press, 1929, p. 166.

seems justifiable then, at this distance in time, to ask what was the real value of his work, and whether or not he made any substantial discovery for which, owing to his diffidence in publishing, others have been given the credit. We may ask with equal interest, in view of his very great influence in the biological world, and of the calibre of some of the pupils whom he inspired (for example, Lister, Michael Foster, Schäfer), what sort of a conspectus of physiology it was that he purveyed, and how did he react to the great movements in physiological thought and the transformation of the science that occurred during the forty years of his active teaching life?

## II. EDINBURGH PERIOD

About Sharpey's early years in Edinburgh, and his wanderings on the continent of Europe, we possess tantalisingly little information. Of the authors of the three main memorials to him, two, Schäfer and D'Arcy Power, knew him only as an old man, and there is no evidence that Allen Thomson, who was seven years his junior, knew him before they became colleagues in 1831. Sharpey himself makes virtually no reference to friends or teachers in any document known to me. He does not, for example, mention any name in his formal letter of application to University College,<sup>19</sup> nor in an extant draft of that letter.<sup>20</sup> As a medical student from 1818–21, he must have sat under a number of men who were recognized as very fine teachers—for example, James Gregory and John Playfair, to both of whom Sir Robert Christison paid tribute;<sup>21</sup> and, possibly—since many students at that time found the teaching of Monro *tertius*, to say the least, unrewarding—John Barclay, a teacher of anatomy whose brilliance was outshone only by his successor, Robert Knox. But we know nothing of Sharpey's response to such men. His name appears in the list of elections to the Medical Society of Edinburgh on 1 January 1819,<sup>22</sup> and in a class list showing that he attended Hamilton's lectures on Obstetrics in 1820.<sup>23</sup> Otherwise we are left to speculate.

One of his oldest friends was James Syme, whom he met in Paris in the early summer of 1822. It was to Syme that Sharpey dedicated his probationary essay for admission to the Royal College of Surgeons of Edinburgh in 1830 'as a tribute of friendship',<sup>24</sup> and at the time of Sharpey's application for the University College Chair, Syme wrote him a testimonial saying that there was no one in Britain better qualified for the position.<sup>25</sup> Paterson, in his *Memorials of the Life of James Syme*, quotes from twenty-one letters written by Syme to Sharpey between 1823–49 but only two of the latest tell us anything at all about Sharpey himself.<sup>26</sup> Another acquaintance of this period

<sup>19</sup> University College London, College Correspondence 3688.

<sup>20</sup> MS. in Sharpey-Schäfer papers.

<sup>21</sup> *The Life of Sir Robert Christison, Bt.*, edited by his sons. 2 vols., Edinburgh & London, William Blackwood & Sons, 1885–6, vol. 1, pp. 42, 79.

<sup>22</sup> *List of Members, Laws and Library Catalogue of the Medical Society of Edinburgh*, printed for the Soc. by William Aitken, Edinburgh, 1820. p. 71. 'Jan: 1 W. Sharpey—Forfarshire.'

<sup>23</sup> MS. in the Library of the Royal College of Physicians, Edinburgh. James Hamilton the younger. Item 10. List of Students attending the lectures in obstetrics, 3 vols., Edinburgh, 1802–31. 'Winter course—Willm Sharpey. 1. Arbroath. 671. Mrs. Anderson's, 12 Buccl. Place.' Sharpey's certificate of satisfactory attendance at his undergraduate courses between 1817 and 1821, signed by Andrew Duncan, Jr., his diplomas of 1821 and 1830, and those from many academic bodies, conferred on him *honoris causa*, are in Arbroath Public Library.

<sup>24</sup> Bibliography, No. 2.

<sup>25</sup> MS. in Sharpey-Schäfer papers.

<sup>26</sup> Robert Paterson, *Memorials of the Life of James Syme*, Edinburgh, Edmonston & Douglas, 1874. John A. Shepherd, in his *Simpson and Syme of Edinburgh*, London & Edinburgh, E. & S.

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was apparently Thomas Hodgkin, who also wrote him a testimonial.<sup>27</sup> We are told that during Sharpey's visits to Italy and Germany in 1827–29, he studied with Panizza, Rudolphi and Tiedemann, but of his reactions to them we know nothing. Much later, a letter to the Rev. J. G. Macvicar<sup>28</sup> refers briefly to 'the days when we gathered mosses on the Pentlands'.<sup>29</sup> There is evidence too, that Sharpey was on terms of intimate friendship with David Masson,<sup>30</sup> but Masson was a graduate of Marischal College, Aberdeen, who did not go to Edinburgh until 1865, and presumably the friendship began with his appointment to University College in 1852.

Sharpey's M.D. thesis, 'De ventriculi carcinomate', was dedicated, in 1823, to two men—to his stepfather, William Arrot, and to Robert Knox. Lonsdale states that '... Knox had numbers of graduation theses, "Probationary Essays", and poetical effusions of students dedicated to him. The present distinguished Dr. Sharpey, though not a pupil of the Doctor's, acknowledged his regard for the great teacher in this way. . . .'<sup>31</sup> The relations between the two men may well have been much closer than this suggests. Godlee in his essay on Wharton Jones, who was Knox's assistant at the time of the great Burke and Hare scandal in 1828, wrote that 'Jones . . . felt the public odium acutely at the time. It also affected him in later life when he became a colleague of his two Edinburgh friends, Robert Edmund Grant, and William Sharpey at University College London, and all three were Fellows of the Royal Society; for Sharpey had been intimate with Knox, and, it is said, contemplated a partnership with him, but, when the scandal arose, he joined in the hue-and-cry against him, while Grant and Jones supported him. This led to a coolness which subsequent events did not tend to diminish.'<sup>32</sup>

### III. THE LONDON APPOINTMENT

From 1831 to 1836 Sharpey was a successful extra-mural teacher of anatomy in Edinburgh. During that period the greater part of his published work appeared (see Bibliography). In 1836, he applied for, and was elected to the Chair of Anatomy and Physiology in University College London, which he held for the ensuing thirty-eight years, during which time his influence on medical and biological science, as teacher and administrator in the College and Secretary of the Royal Society, was by all contemporary accounts, immense.

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Livingstone, 1969, says (p. 252) that he failed to trace any large collection of letters such as those between Syme and Sharpey which must have been available to Paterson.

<sup>27</sup> 'One of the most interesting [testimonials] which indicate the high attainments of Dr. Sharpey and the opinion and expectations formed of him by his medical friends at a very early period is from Dr. Hodgskin [sic] of Guy's Hospital who was his fellow student in Paris.' (University College London Council Minutes, dated 6 August 1836. See note 41). This testimonial is not to be found with the others amongst the Sharpey-Schäfer papers, but it is clear from the same Minutes that additional late testimonials were received. Hodgkin, like Sharpey, graduated M.D.(Edin.) in 1823.

<sup>28</sup> Rev. J. G. Macvicar (1800–1884). See *Dictionary of National Biography*.

<sup>29</sup> Letter dated 27 February 1865 in the Library of the Royal College of Physicians, London.

<sup>30</sup> Letter dated 11 March 1868 in the Library of Edinburgh University.

<sup>31</sup> Henry Lonsdale, *A Sketch of the Life and Writings of Robert Knox, the Anatomist*, London, Macmillan, 1870, p. 275.

<sup>32</sup> Sir Rickman J. Godlee, Bt., K.C.V.O., *Brit. J. Ophthalm.*, 1921, 5, 101, 107. Knox was nine years older than Sharpey, but both men were in Paris in 1821–22, Sharpey as a young postgraduate, Knox after his years in South Africa (see Isobel Rae, *Knox the Anatomist*, Edinburgh & London, Oliver & Boyd, 1964, p. 21). It would be interesting to know what influence, if any, Knox had on Sharpey's decision to pursue an academic career. After being in partnership with Barclay, Knox fell heir to the latter's School of Anatomy late in 1826.

Sharpey's qualifications for the post were considerable. Although too much stress need not be laid on the evidence of open testimonials, all emphasize his breadth and depth of knowledge in both anatomy and physiology, his familiarity with French and German, as well as English medical and scientific literature, his originality, and his skill as a teacher.<sup>33</sup> Nevertheless in some quarters at least, he was regarded as a virtually unknown outsider and his appointment was unexpected. Allen Thomson maintained that he was well known both in Edinburgh and at a distance.<sup>34</sup> Perhaps the distance between Edinburgh and London was too great! At all events the appointment stimulated one London editor to write:

Dr. Sharpey of Edinburgh is elected to the Chair of Anatomy and Physiology, vacant by the resignation of Dr. Quain. Of the talents and acquirements of Dr. Sharpey we know nothing personally, but he bears a high character as an anatomist, we understand, in Edinburgh; how far, however, the Council will serve the interests of the University, by electing this gentleman in preference to such well-known individuals as DR. GRANT, DR. LIZARS, or MR. GRAINGER, may meet with a difference of opinion; it may not be out of place to state, that the decision of the Council has given great and general surprise to the profession. . . .<sup>35</sup>

The *Lancet* was more outspoken. On 13 August 1836, the editor wrote:<sup>36</sup>

The appointment of DR. GRANT to the Chair of Physiology, and the induction of MR. QUAIN to the Chair of Anatomy would, we are convinced, in the absence of public competition have given the most unequivocal satisfaction to the profession. . . . The Professors . . . have advised the conjoint appointment of MR. R. QUAIN and a Doctor SHARPEY of Edinburgh to the Chair of Anatomy. . . . Here we have DR. GRANT, who is, beyond all dispute one of the most highly-gifted physiologists in Europe and whose reputation has extended everywhere, made, by his own colleagues, to give place to a Doctor SHARPEY, who has not the felicity of being known out of Edinburgh! . . . under cover of recommending a joint Professor of Anatomy, the Professors really believe that they are promoting the election of a person who will be required to undertake the duties of a Professor of Physiology; and thus the unknown Dr. SHARPEY of Edinburgh is preferred by the wise and impartial Medical Faculty, to DR. ROBERT GRANT, who is a renowned member of the world of science. . . .

It was admitted that Grant has not necessarily been a candidate; nevertheless: 'If there be a concours where is the man who will enter into a competition with Dr. GRANT? Will Dr. SHARPEY of Edinburgh? We smile at the thought.'

On 3 September 1836, the attack continued. The *Lancet* maintained that its complaint was against the system of monopoly in appointment—although it would have been happy to see Grant appointed by just that mechanism!—whereas:

. . . in the instance before us, we have hypocrisy, treachery, envy and fraud, superadded to the one ancient evil, *love of pelf*. Not only is monopoly rendered rampant once more, but the very

<sup>33</sup> There are nine of these in the Sharpey-Schäfer papers—all copied in Sharpey's hand. They were written by Dr. John Abercrombie (physician), Sir George Ballingall (Prof. of Military Surgery), Dr. W. P. Alison, Dr. R. Christison, Dr. D. Craigie (Editor, *Edin. med. surg. J.*), Dr. John Thomson (Professor of General Pathology), Dr. R. Jameson (Mineralogist), Dr. D. Maclagan (former P.R.C.S.E.) and James Syme. All were Edinburgh men.

<sup>34</sup> Obituary, op. cit. note 7, p. xii.

<sup>35</sup> *Lond. med. surg. J.*, 1836, 9, 965.

<sup>36</sup> *Lancet*, 1835-6, ii, 675 et seq. Robert Edmund Grant (1793-1874), comparative anatomist, was Professor of Comparative Anatomy and Zoology in the University of London (later University College), 1827-74. He was elected F.R.S. in 1836. He was a graduate in medicine of Edinburgh, and a friend of Darwin in his youth. For an assessment of his teaching, see pp. 135-36. The *Lancet* sprang to his defence again in 1846 (Part II, Section VII) and later devoted a 'profile' to him (*Lancet*, 1850, ii, 686-95).

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triumph of the monopolists serves as an announcement that the community, the profession, and the students of the institution HAVE BEEN BETRAYED. Who is Doctor SHARPEY? Where is he known as a discoverer—as a physiologist? Who is Professor GRANT? Where is that distinguished man *not* known as a discoverer and a physiologist?<sup>37</sup> Yet a majority of Council votes go ‘in favour of the UNKNOWN MAN OF THE NORTH.’<sup>37</sup>

Both the man and the system were defended by Professor A. T. Thomson in the ‘Address, delivered at the commencement of the Medical session of 1836–37, at the London University’, in which, after eulogising Jones Quain,<sup>38</sup> Sharpey’s predecessor, he made it clear that the method of election adopted was that which was usual on such occasions; that the opinion of the Committee was unanimous respecting the testimonials of Dr. Sharpey and that they felt themselves fully justified in recommending him as the fittest person to fill the vacant Chair.<sup>39</sup>

There were further repercussions, this time from Edinburgh. In an address delivered at the beginning of the 1836–37 session in the Argyll-square medical school in Edinburgh,<sup>40</sup> Dr. McIntosh commented that if success as a teacher were the criterion, the post ought to have gone to Lizars. Sharpey had ‘risked his success upon the effects produced by flattering certificates from his friends’, and, contrary to his claim to have made a class for himself by his own exertions and abilities, even with ‘the patronage of almost all the influential medical men in Edinburgh, and amongst these several of the Professors in the University, his class when he left Edinburgh, after a trial of several years, stood in the third rank only.’ Thus, to the strictures of the *Lancet* that Sharpey was eminent neither as a discoverer nor a physiologist, and the suggestion that he was inferior as an appointee to at least three other gentlemen, there were added the scarcely-veiled accusations of influence at court and juggling with figures—hardly an auspicious beginning to the tenure of such an important post. Given this degree of acrimony, it is interesting to examine the evidence provided by the records of University College.

Council at its meeting of 16 July 1836 received and accepted a letter of resignation from Dr. Jones Quain, dated 16 June 1836 at Cork. Five members of the Medical Faculty (Drs. Carswell, David, Grant, Mr. R. Quain and Dr. Thomson), attending on request, said that in this case the vacancy should be advertised. (A Senate minute of 31 March 1835 had recommended to Council the setting up of a committee of five, when vacancies occurred, to look out for fit persons and to decide whether a post should be advertised or not.)<sup>41</sup>

At the Senate meeting of 23 July 1836, the Dean of the Medical Faculty presented a four-point plan for filling the vacant chair.<sup>42</sup> Of the two professors recommended

<sup>37</sup> *Ibid.*, pp. 789–80.

<sup>38</sup> Jones Quain (1796–1865), anatomist, and brother of Richard Quain, was Professor of general anatomy at University College London, 1831–6. He was author of *Elements of Anatomy*, eds. 1–4, the next four editions of which were edited by Sharpey and others. Richard Quain (1800–87), anatomist and surgeon, and brother of Jones Quain, was Professor of Descriptive Anatomy, 1832–50, and of Clinical Surgery, 1846–66, at University College London. For both men, see *Dictionary of National Biography*, and Bellot, *op. cit.*, note 18.

<sup>39</sup> *Lancet*, 1836–7, I, 75 et seq.

<sup>40</sup> *Ibid.*, I, 327–28.

<sup>41</sup> The references in this and other sections are to the MS. minutes of the Proceedings of Council and of Senate in the College Archives, University College London.

<sup>42</sup> ‘(1) That there shall be two Professors of exactly equal rank with the titles of Professor of Anatomy and Physiology by whose joint labours the lectures at 11 and 2 o’clock and the business of the dissecting room shall be conducted.



it was clearly intended that Mr. Richard Quain be one. The plan was duly put before Council at its meeting of 30 July 1836. However, it was agreed that its discussion be deferred until after consideration of the merits of the different candidates. On 3 August Council forwarded to Senate applications from fourteen candidates. Senate on the following day set up, as the result of a ballot, a committee consisting of Carswell, Elliotson, Malden, Thomson and Turner, which reported on 6 August. The report was received and adopted by six votes to two. The dissentients were presumably Ritchie and Grant, since at the same meeting, Ritchie read a letter signed by Grant, Liston and himself, urging the claims of Mr. Grainger. Meanwhile at a Senate meeting on the previous day, it had been resolved that Sharpey's testimonials be read, and it was also 'resolved that the committee had come to a sound conclusion in not considering Dr. Grant a candidate for the Chair now vacant namely that of Anatomy and Physiology'.<sup>48</sup> Grant was not present at this meeting; Quain, although a candidate, was present, at Turner's insistence, as being 'more able than any other to give anatomical advice'.

On 6 August, the Senate report—a very full one—was read to Council. It is quite clear that the Committee had found no difficulty in eliminating eight of the applicants. Lizars had sent no testimonials. Grant was dismissed for the reason shown. Four candidates, Grainger, Hart, Sharpey and Quain, were reported on at length. It was said of Grainger that there was no evidence of his possessing a sufficiently extensive knowledge of Physiology, which the College wished to promote. 'They [the Committee] may add that they have no evidence that Mr. Grainger is a gentleman of cultivated mind and extensive general knowledge. He had not the advantage of a regular Academic Education.' For that reason he had been turned down in favour of Jones Quain in 1831. Also 'the Committee are informed by Dr. Davis that Mr. Grainger's intonation in lecturing is in his opinion peculiarly unpleasant.'

Hart appeared to be well qualified from his testimonials despite a weakness in the literature in foreign languages, especially German, but 'it is to be observed that of these testimonials rather a large proportion are in the form of letters addressed to Dr. Hart—a circumstance which may perhaps give a higher tone to their mode of expression'.

The Committee were 'unanimously and decidedly of the opinion that the best qualified candidate is Dr. Sharpey.' They were impressed by his publications, especially that on 'Cilia', and by his high testimonials. 'To Physiology he has paid special

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(2) The class at 11 o'clock to be on Descriptive and Comparative Anatomy. It should consist of a full course of descriptive Anatomy, more extended than formerly to be given entirely by Mr. Quain and which will occupy the greater part of the Session. The remainder of the Session will be employed by the new Professor in repeating the more important parts of descriptive Anatomy.

(3) The class at 2 o'clock to be specially devoted to general or structural Anatomy & Physiology. It will consist of a complete account of the Anatomy of Textures, development of Organs etc. & Physiology, more complete and extensive than formerly, to be given by the new Professor along with so much descriptive Anatomy, as is required for the elucidation of these subjects, and as the time necessary to their full consideration will permit. This will occupy the greater part of the course. The remainder will be occupied by Mr. Quain with an account of regional or surgical Anatomy.'

(4) [This paragraph is entirely financial—D.W.T.]

<sup>48</sup> 'As Dr. Grant has applied for the Physiological Department only in case Physiology should be separated from Anatomy, the Committee has not considered him as a candidate, and make no report respecting him.' (Council Minutes, 3 August 1836).

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attention and although less his professional subject as a lecturer than Anatomy, it appears to be his favourite pursuit.' He was well read in English, French and German, 'an original philosophical thinker . . . cautious and logical in his deductions', and in exposition 'plain but peculiarly precise and clear, and consequently agreeable and attractive'.

Richard Quain was regarded as 'a first-rate teacher of Anatomy; his class during the last session has been the largest Anatomical class in the Empire', with the smallest rejection rate at the Royal College of Surgeons. As a scholar and gentleman he was held to be the best of the lot, with strong claims to the vacant chair. 'The Committee, however, are of the opinion that the acquisition of Dr. Sharpey would be a very great gain to the University', and they recommend that both men be got, by making Quain Professor of Anatomy, pointing out that 'a plan for thus equalising the Professors has already been recommended by the Senate to the Council'.

There was then read the letter from Grant, Liston and Ritchie advancing the claims of Grainger as an anatomist against 'the appointment of a teacher from a distance whose merits and success are comparatively unknown'. The Senate minute goes on to record that Grant was to be asked to explain the meaning in his application of the 'proposed division of the Anatomical Chair', and if he 'desire to be considered a candidate in case of that division and not otherwise.'

Turner, Elliotson and Quain were then admitted to the meeting at their own request. 'Dr. Turner and Mr. Quain made statements as to the course they had pursued relating to the vacant professorship.' At the Council meeting on 11 August 1836, Grant's letter of explanation was received. The text is not recorded in the minutes. Later, 'Dr. Booth stated that he had authority for withdrawing the name of Mr. Quain from the list of candidates', and 'after debate Mr. Booth moved that Dr. Sharpey be appointed Professor of Anatomy and Physiology'. This was duly passed and so we find in the Senate minutes of 13 August, that 'Dr. Sharpey was introduced and took his seat', and later, on 29 October 1836, a request to Council, in place of the recommendation of 23 July, 'to confer on Mr. Quain the title of Professor of Anatomy'. This was done on 5 November 1836.

As far as can be judged, the appointment of Sharpey was made entirely according to the letter of the law, whatever the *Lancet* might think of that law, but there are a number of features about the case, which make one more than a little suspicious of some intrigue.

It is difficult to escape the feeling that Grant was actively unwanted. He was already a professor in the university and a colleague of nearly ten years' standing. Between 1825–36 he had published thirty-five papers, some of which were on purely physiological topics. His classes were not compulsory for medical students and were consequently poorly attended, with a substantial effect on his emoluments; in fact he appears to have spent almost all his life in near-penury. He had thus every reason to apply for a more lucrative post. However, he chose to put his application in a way that assumed a particular division of the Chair, and this led to his brusque rejection. We do not know the date of his application, relative to the first publication by the Medical Faculty on 22 July 1836 of its plan for dividing the Chair, and therefore his grounds for supposing that there might be a division. It was clearly in his interests

to promote Grainger, a pure anatomist, given that the Chair might be divided and given the College's stated desire to enhance the status of physiology. Schäfer wrote, many years after Grant's death: 'There is no doubt that as a teacher Grant was impossible. I believe that the course of lectures, which he gave in 1834 was the finest which had ever been given in English upon the subject; but then in 1867 they were the *same* lectures given in the *same* words'.<sup>44</sup> He was certainly unlikely to have been a successful academic politician. At all events, the *Lancet* continued for many years to imply that he had been hardly treated.

The part played by Richard Quain remains rather mysterious, and indeed, at one stage as we have just seen, Council demanded an explanation of his conduct.<sup>45</sup> He was already an independent professor in the university, whose interests seem to have been entirely anatomical and surgical. If the 'plan' had gone through his position was assured, and one would have thought it equally so had Grant been appointed to a chair purely of Physiology. However he seems to have felt as a threat the decision to defer consideration of the 'plan' for the Chair until after the appointment had been made. This seems to be the only explanation for the date of his own application, namely 2 August 1836, just after learning of the Council's decision. He presumably thought thus to forestall, on the strength of his own status in the college, the election of Grant or anyone else—other than Sharpey. When it was clear that Sharpey's appointment was virtually certain, his own application was withdrawn—not surprisingly, since there is incontrovertible evidence that he was instrumental in getting Sharpey to apply in the first place. University College archives contain a letter from Quain to Sharpey, dated well before the publication of any 'plan', and referring to a conversation that presumably took place not long after the announcement of Jones Quain's resignation.<sup>46</sup> The letter is addressed simply: Dr. Sharpey, Lecturer on Anatomy etc., Edinburgh, with a note on the outside 'Excuse the address. I know not your residence. Write to say if you will come and when.' The letter reads:

In much haste for post

My dear Sir

I spoke to you of a vacancy in our School of Medicine occasioned by the resignation of Dr. Quain—If you continue to think an appointment with us desirable. I suggest the prudence of your making an application within a week or so. I am strongly of opinion it would be to your advantage to come to town **within a week** or *ten days* or a little more—*the sooner the better*—The exact nature of the office, the probable emoluments and all particulars could best be known by your personal application here—you may get a substitute for your Demonstrations or give them twice a day—or return to finish them after a short delay.

23 Keppel St.

London

Saturday 16 July

I am Dear Sir

very truly yours

R. Quain

*do not omit*

*testimonials etc.*

*if to be easily had.*

It was publicly hinted during the rumpus arising from the resignation of Samuel Cooper (see Part II, section VII) that Quain had been guilty of nefarious conduct at the time of his brother's resignation. His anonymous obituarist recalled half a century later the quarrels of those days, in which the brothers were so often found on opposite sides and said also that shortly after Liston's arrival 'the fame of Dr. Sharpey . . . having

<sup>44</sup> Schäfer, *op. cit.*, note 1, p. 215.

<sup>45</sup> A letter written by him to the Council (University College London, College Correspondence 3701) suggests that the plan was evolved by the whole Medical Faculty in response to a private letter to himself from a Member of the Committee of Management of the Council.

<sup>46</sup> MS. S. R. Misc. 3Q in Library of University College London.

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reached London, Mr. Quain paid a visit to the northern capital and attended Dr. Sharpey's lectures incognito for the purpose of ascertaining whether the reports of his excellence were correct.<sup>47</sup> Unfortunately we do not know the exact date of this visit. In all this, there is no suspicion of any intrigue on Sharpey's part, but it was not by any means the last time that he was to have aspersions cast on him by the *Lancet*.

IV. ORIGINAL WORK

Sharpey was thus installed in the Chair which he was to occupy with such acceptance for so many years. At this point it may be useful to consider the nature of his contribution to scientific knowledge. The appointing committee recorded that they were impressed by his published work, especially that on the subject of cilia. Prior to his appointment, Sharpey had published (see Bibliography) his two Edinburgh theses, five papers, and one of his contributions to Todd's *Cyclopaedia*. The theses are, naturally, entirely clinical. The articles for the *Cyclopaedia* are long, comprehensive and clear, but in the nature of things are scarcely primary vehicles for communicating the original work of the author although we have Sharpey's own testimony that they certainly did contain some.<sup>48</sup> This leaves the five papers listed 3–7 in the Bibliography. The first of these, 'On a peculiar motion excited in Fluids by the surfaces of certain Animals' (Bibliography, No. 3), is interesting in a number of ways. While working on the development of the tadpole, and having cut off a piece of the external gills in order to determine the size of the 'blood globules', Sharpey noted that the globules from the cut part of the gill 'were moved rapidly along its surface . . . in a constant and uniform manner', clearly due to a surface current. Taking the observation further, Sharpey found that nearly the whole body surface had the same property; that it appeared before the tadpole left the egg and gradually vanished with the growth of the animal; and that a gill fragment, in water, if unimpeded, moved in a direction opposite to that of the current. He thought that the function of such currents was probably respiratory. Other species were examined and the phenomenon detected in a number of invertebrates, particularly *Mytilus edulis*, and shown to be due to the activity of cilia. He could not detect any such thing in the developing chick, nor was he able to see cilia in batrachia. In marine species, immersion in fresh water was observed to stop the currents, and the oscillations of the cilia. Sharpey emphasized that this was essentially a preliminary communication about work still in progress, and the only reference is to Grant's observations on sponges. Nevertheless it is in many ways a very complete paper, admirably clear in its setting out of a number of distinct and important points, and affording good evidence of the capacity of its author to grasp the physiological implications of such a phenomenon. Sharpey returned to this topic in his paper of 1835 (Bibliography, No. 7) which is largely a translation of a paper by Purkinje and Valentin in Müller's *Archiv*, 1834, announcing their discovery of ciliary motion in the oviduct of birds and the Fallopian tubes of mammals, and in the air-passages of both groups. In a prologue to the translation, Sharpey admits that he was preceded in his discoveries of the 1830 paper by a number of earlier workers, although most of these had observed only the currents and not

<sup>47</sup> *Lancet*, 1887, ii, 687.

<sup>48</sup> See Bibliography, no. 7, p. 116.

the cilia causing them, and did not always correctly interpret their observations. To the translation Sharpey appends an addition of his own, confirming the results of Purkinje and Valentin, extending their applicability to some other species, and reporting the determination, in some instances, of the direction of current flow.

The paper on the 'supposed spontaneous motion of the blood' (Bibliography, No. 4) is very short. It is, in fact, in the form of a two-page letter to the editor of the journal suggesting that the circumstances under which some, at any rate, of the observations in question had been made, pointed to ciliary motion as the real explanation. It will be considered in greater detail elsewhere in this paper. The paper on the anatomy of the blood vessels of the porpoise (Bibliography, No. 6) is an abstract, the full text of which exists in manuscript amongst the Schäfer papers, and is purely descriptively anatomical. The paper of 1833 on Ehrenberg's researches on the Infusoria (Bibliography, No. 5) does not claim to be original. The observations reported and Sharpey's opinions of them, again make detailed consideration more appropriate at a later stage.

We have to admit that, all in all, this is not a particularly impressive catalogue. Johannes Müller, with whom Sharpey has been compared in terms of the influence he exerted on others, published nearly two hundred papers, fifty-nine of them between 1822–36, and other Continental workers of equal reputation had similar records of publication. Even in Britain, where the physiological scene at that period was so deplored by Sharpey-Schäfer, writing nearly a century later—even in Britain, men such as Charles Bell, Grant, Carpenter, Bostock, Allen Thomson, and others had publications which in quantity, at least, if not in quality, were considerably in excess of those of Sharpey. This of course in no way reflects on the correctness and wisdom of Sharpey's appointment to the University College chair. Not all of these men were Sharpey's immediate contemporaries, and only Grant was a competitor for the position. However, when we remember that after 1836 he published no original work directly under his own name, even allowing that he lived before the efflorescence of British physiology, which he did so much to bring about, these figures demonstrate that Sharpey's record of published original work was meagre, and that his influence was certainly not that of a great research worker gradually building up a school. We have, of course, the opinions of Thomson and of Syme that Baly's translation of Müller's *Handbook of Physiology*<sup>49</sup>—which, incidentally, departs from the original in a number of ways, not all made clear in the translator's preface, where his debt to Sharpey and others is acknowledged—and successive editions of Quain's *Anatomy*, owed much directly to Sharpey.<sup>50</sup> He is certainly acknowledged as the source of one 'foot-note' in Müller's book<sup>51</sup> on the structure of the decidua, which runs to nearly eight pages of very small print, contains important histological and embryological observations, and is easily the equivalent of a conventional scientific paper. Kölliker, too, in his textbook<sup>52</sup> paid tribute to Sharpey and to his work on the histology of bone. We may note here that Sharpey was always extremely ready to forgo any claim to priority in discovery, a trait which doubtless contributed appreciably to the

<sup>49</sup> J. Müller, *Elements of Physiology*, trans. from the German with notes, by William Baly, M.D., 2 vols., London, Taylor & Walton, 1838–42.

<sup>50</sup> See p. 129, and also Paterson, *op. cit.*, note 26, p. 98.

<sup>51</sup> Müller, *op. cit.*, note 49. Vol. II, 1842, pp. 1574–82.

<sup>52</sup> A. Kölliker, *A Manual of Human Microscopic Anatomy*, London, John W. Parker, 1860 (see also dedication).

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respect and affection in which he was so widely held. We have evidence of this in his second paper on cilia;<sup>53</sup> and near the end of his life, in an addendum to a paper by Schäfer,<sup>54</sup> he renounced priority in the discovery of the perforating fibres of bone (still referred to as Sharpey's fibres) in favour of the eighteenth-century Neapolitan surgeon, Michael Troja.

We must now consider what evidence there is that Sharpey did any substantial amount of experimental work which for one reason or another he never saw fit to publish. Thomson cited Sharpey's lectures as a medium whereby he communicated much original research and thought. There are in existence five sets of manuscript lecture-notes, taken from Sharpey's lectures at various periods between 1836 and 1867. The earliest of these was written by J. P. Potter in the session 1836–37, and consists of lectures 1–26 on general anatomy, and lectures 27–54, on the anatomy and physiology of digestion.<sup>55</sup> These notes appear to have been taken verbatim. Of less interest is a notebook of unknown authorship entitled 'Epitome of Physiology. Dr. Sharpy [sic] 1837–8'<sup>56</sup> and as we might expect, highly condensed. For 1840–41 we have a complete set of notes on Sharpey's lectures, said by the writer, Edward Ballard, M.D., in a covering letter dated 27 May 1891, to have been taken verbatim.<sup>57</sup> Fourthly and of great and obvious interest, is a very full, if not necessarily verbatim set made in 1849–50 by Joseph Lister,<sup>58</sup> with many additions, mostly dated 1852, which show that Lister in that year attended at least parts of Sharpey's course for a second time. Finally, for the session 1867–68, we have the notes of Sir George Thane, as he later became, which are much less detailed, in fact in several places distinctly scrappy.<sup>59</sup> These sets of notes, with the disadvantages inevitably inherent, in varying degree, in such material, would seem to be our only means of judging a statement such as Allen Thomson's, for he makes it clear that Sharpey, apart from the introductory lecture, never wrote out his lectures,—in fact, the first five lectures of his 1841–42 course were published in the *Lancet*<sup>60</sup>—and that he spoke with the assistance only of 'very short jottings on small slips of paper'.<sup>61</sup>

In 1836, Müller and Schwann published a paper on the *in vitro* digestion of coagulated egg albumen in which they confirmed and extended the work of Eberle who had found that whereas neither dilute acid nor mucus had of themselves the capacity to digest certain organic substances, such as albumen and muscle, this capacity was possessed by acidified mucus and by an acid extract of mucosa.<sup>62</sup>

<sup>53</sup> See Bibliography, No. 7, p. 116. <sup>54</sup> See note 14.

<sup>55</sup> J. P. Potter, 'Lectures on Anatomy and Physiology, delivered in the University of London by Wm. Sharpey, M.D., Session 1836–7', 2 vols., MS. in Library of University College London.

<sup>56</sup> Warren R. Dawson, *Manuscripta Medica. A Descriptive Catalogue of the MSS. in the Library of the Medical Society of London*, London, John Bale, Sons & Danielsson, 1932, p. 17. 'Lecture Notes on Physiology. 16 mo. 70 ff. f2. Epitome of Physiology. Dr. Sharpy [sic] 1837–38.' MS. now in Library of Wellcome Institute of the History of Medicine.

<sup>57</sup> E. Ballard, 'Notes of Lectures on Anatomy and Physiology delivered by Dr. Willm. Sharpey at University College London. Session 1840–41', 2 vols., MS. in Library of University College London.

<sup>58</sup> *Manuscripta Medica* (op. cit., note 56). 80. 'Sharpey's Lectures on Physiology. 4to ff. 429 dated 1849–50. Many of notes initialled J. L. and script is that of Lister.' MS. now in the Library of the Wellcome Institute of the History of Medicine.

<sup>59</sup> G. Thane, No title. MS. in the Library of University College London.

<sup>60</sup> *Lancet*, 1840–41, i, 73–77, 142–47, 281–84, 425–28, and 489–92.

<sup>61</sup> Obituary, op. cit., note 7, p. xiv.

<sup>62</sup> J. Müller and Th. Schwann, 'Versuche über die künstliche Verdauung des geronnenen Eiweisses', *Arch. Anat. Physiol.*, 1836, 66–89. The work to which the authors refer is that of J. Eberle, *Physiologie der Verdauung*, Würzburg, 1834.

Müller apparently began experimenting on his own account in the winter of 1834/5 and was delighted at being able fully to confirm Eberle's results, despite initial doubts ('... [er] begann die Versuche zur Prüfung der Entdeckung von Eberle im Winter 1834/35 nicht ohne Zweifel; er war um so mehr erfreut, als er bei den ersten und folgenden Versuchen die volle Bestätigung der Resultate von Eberle erhielt').<sup>63</sup> This work was continued by Müller and Schwann together during the winter of 1835. In all their experiments they used mucosa from the fourth stomach of the calf, which they washed until it was no longer acid to litmus, and then dried, later extracting it with dilute hydrochloric acid. They were well aware of Eberle's contention that any mucosa was suitable if treated with acid, the active principle, he thought, being inherent in mucus from different mucosae, but they confined themselves to the one source, since mucus was not always easy to obtain pure and in sufficient quantity, and mucosa from the calf stomach was very readily prepared.<sup>64</sup>

These experiments were dealt with very fully by Sharpey in his lectures on digestion in session 1836–37.<sup>65</sup> 'Thus Müller and Schwann', he said, 'confirm the statement of Eberle as far as the mucous membrane of the stomach is concerned, but it is unfortunate that they did not also experiment on other membranes, because it is an important part of Eberle's statement that the mucus from any mucous membrane would equally answer the purpose.' In fact, although Müller and Schwann regarded themselves as having confirmed Eberle's splendid discovery ('die glänzende Entdeckung von Eberle') they do in the last few lines of their paper imply awareness of this omission when considering possible applications of the discovery as a diagnostic test for mucus in the presence of pus.<sup>66</sup> Sharpey himself, however, performed just such a series of experiments as were required to settle this point, and these he discussed with, and demonstrated to, his class. He used (i) mucus from the bladder of an ox, (ii) mucosa from the same source, (iii) mucosa from a pig's stomach in the fasting state, (iv) mucosa from the fourth stomach of a sheep 'in the act of secreting gastric fluid at the time the animal was killed'. When dilute hydrochloric acid was added and the effect on coagulated egg-white observed in the usual way, (i) and (ii) gave no result, and (iii) was appreciably less effective than (iv). Thus 'although it requires many careful negative experiments to counterbalance a positive experiment of a physiologist like Eberle, as far as my experiments go, they are certainly opposed to the statement of Eberle'. Schwann wrote a second paper on the same subject in which he uses the term pepsin, and which has since become a classic in the history of physiology.<sup>67</sup> It is very long and detailed, and makes no concessions to the reader by way of sections or subtitles. The aim of the work described therein was to find out what were the active materials in gastric juice and how they acted. Only in one small part of the work was anything used other than calf gastric mucosa prepared in the usual way. This was when, in order to see whether mucus, or rather something derived from it,

<sup>63</sup> Müller and Schwann, *op. cit.*, note 62, p. 68.

<sup>64</sup> *Ibid.*, p. 72.

<sup>65</sup> Potter MS. Vol. II, p. 239 et seq.

<sup>66</sup> Müller and Schwann, *op. cit.*, note 62, p. 89. 'Ist in der That aller Schleim wie Eberle angiebt, zur Bereitung des chymificirenden Principis geeignet, was weiter untersucht werden soll, und ist das fragliche Princip nicht vielmehr in gewissen Schleimarten nächst den Schleimhäuten enthalten, so dürfte sich der Schleim vielleicht daran erkennen lassen, dass sein säuerliches Extract das geronnene Eiweiss auflöst, und heirdurch müsste sich selbst der mit Eiter vermischte Schleim erkennen lassen.'

<sup>67</sup> Th. Schwann, 'Über das Wesen der Verdauungsprocesses', *Arch. Anat. Physiol.*, 1836, 90–138.

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was the active principle, Schwann used pure mucus from human saliva. This material gave a faintly positive result after forty-eight hours, from which he concluded that 'Der Schleim spielt also wirklich bei der Verdauung eine wesentliche Rolle'. He also used the filtrate obtained from saliva incubated with hydrochloric acid for up to seventy-two hours, but got a negative result after several days' exposure of egg albumen to this fluid.<sup>68</sup> These experiments were not entirely satisfactory, as Sharpey pointed out, and they were anyhow undertaken to show whether mucus as such, or a derivative of it was the active principle, and not to confirm or refute Eberle's point that mucus or mucosa from any source, and not merely from the stomach, possessed activity. Sharpey on the other hand tackled just this problem and showed that Eberle was wrong. Given the state of the subject at the time, his experiments seem to have been eminently worthy of publication, and this might have given him a modest niche in the history of the topic. However he did not publish, and the 1840–41 notes deal rather briefly with the matter, referring merely to the discovery of pepsin by Eberle and Schwann.

Sharpey refers in these early lectures to an experiment carried out by Allen Thomson and himself using a frog's leg and galvanism, and undertaken because they did not consider to be entirely conclusive the demonstrations of Glisson and of Swammerdam that a contracting muscle did not increase in volume. He was mistaken however in his supposition that Swammerdam used only the frog heart for this purpose.<sup>69</sup> Nevertheless, the evidence as far as it goes, seems to indicate that most of Sharpey's experiments were done simply to demonstrate to his own satisfaction facts that had already been discovered by others, as, for example, Spallanzani's observations on the intermittent flow of blood in the gills of the tadpole, and certain observations on the clotting of blood, to both of which Lister referred.<sup>70</sup>

Sharpey's relations with Lister are interesting, and tell us something of him both as a physiologist and a teacher of physiology. Godlee, in his life of Lister, says that 'amongst all these men of science who watched over his earlier years, Wharton Jones and Sharpey apparently exercised the greatest influence'.<sup>71</sup> Lister himself wrote that 'As a student at University College I was greatly attracted by Dr. Sharpey's lectures which inspired me with a love of physiology, that has never left me'.<sup>72</sup> Lister's work was bitterly criticized by Wharton Jones in later years<sup>73</sup> but with Sharpey he remained on the friendliest terms. It was Sharpey who sent him to Syme, who introduced him to such authorities as Kölliker<sup>74</sup> and to whom he turned for advice at various critical moments in the earlier part of his career. Many years ago there were published a number of letters from Lister to Sharpey that enable us to envisage well the esteem in which each held the other.<sup>75</sup> Of the eight letters, one speaks in detail of Lister's application for the Glasgow chair and another deals at considerable length with

<sup>68</sup> *Ibid.*, pp. 111–13. This whole passage is of great interest for the history of the physiology of digestion.

<sup>69</sup> F. J. Cole, *A History of Comparative Anatomy*, London, Macmillan, 1949, figs. 127, 128.

<sup>70</sup> See note 75, below.

<sup>71</sup> Sir Rickman Godlee, *Lord Lister*, London, Macmillan, 1917, p. 16.

<sup>72</sup> *The Collected Papers of Joseph, Baron Lister*, 2 vols., Oxford, Clarendon Press, 1909, vol. II, p. 515.

<sup>73</sup> Godlee, *op. cit.*, note 32.

<sup>74</sup> Godlee, *op. cit.*, note 71, p. 64.

<sup>75</sup> C. R. Rudolf, 'Eight letters of Joseph (Lord) Lister to William Sharpey', *Brit. J. Surg.*, 1932–3, 20, 145–64, 459–66.



questions of medical education, particularly with regard to examinations. Lister had for three years been using the system of class certificates in Glasgow rather than one all-important final examination, 'thanks to the advice you gave me (which advice as coming from you was I believe a main means of enabling me to introduce the thing)'.<sup>76</sup> Lister did not agree, however, that patients should be used in clinical examinations, especially where large numbers of students were involved. The remaining six letters are concerned with Lister's early work on inflammation and, stemming therefrom, on the function of the pigment cells of frog skin, and on vasomotor control. It was Sharpey who sponsored his early papers to the Royal Society and whose advice was sought and abided by on very many points of detail. There was some argument for example, about Lister's use of the word 'paralysis' to describe the changes undergone by pigment cells and by cilia during inflammation. Sharpey preferred to talk of 'arrestment' since this did not imply any prior judgment about the physiological nature of the normal control over these functions. Lister was persuaded, and announced his intention 'to act on your suggestions from first to last. The section on the state of the tissues in inflammation will, I believe, be very much improved by remodelling it on the plan you have suggested, viz: speaking first of the facts actually seen in the pigment cells, viz: the "arrestment" of concentration and diffusion and then discussing the question of "paralysis"'.<sup>77</sup> In the same letter is discussed the question of whether or not the tissues exert an active influence to prevent the *in vivo* clotting of blood; here too, the delay involved was admitted later to have had an advantage 'viz., it has shown that I was wrong and you were right after all about the matter of coagulation of the blood. Your view was that the reason of coagulation on removal from the vessels or on injury or disease of the vessels was a difference of surface in the surrounding objects.'<sup>78</sup> On another occasion Lister discussed his experiments on 'the so-called Hemmungs nervous system'.<sup>79</sup> This is a long and detailed letter, with many references to continental work, in which are described the effects of different strengths of stimulus (irritation of the skin) in causing contraction or relaxation of arterial musculature, and, by analogy, the effects of different strengths of galvanic stimulation on intestinal muscle. Many years later, in 1884 Lister wrote: 'I happened, I believe, to be the first to use the word "inhibitory" in English physiology, by the advice of my old friend Dr. Sharpey, with reference to an early paper I was about to publish on what the Germans term the "Hemmungsnervensystem"'.<sup>80</sup> The whole discussion must in fact have supposed a complete familiarity on the part of Sharpey with this sort of work and with the associated literature. That such a supposition was in fact justified is reasonably clear from a letter from Augustus Waller to Sharpey, dated 19 February 1853, in which the writer does himself 'the pleasure of forwarding to you a short notice of the experiment which I performed yesterday in the presence of yourself, Dr. Carpenter and Mr. Edwards'.<sup>81</sup> This was an experiment, the converse of that performed a little earlier by Claude Bernard, in which Waller showed the effects of stimulating

<sup>76</sup> *Ibid.*, Letter dated 16 November 1857.

<sup>77</sup> *Ibid.*, Letter dated 28 November 1857.

<sup>78</sup> *Ibid.*, Letter dated 28 November 1858.

<sup>79</sup> *Ibid.*, Letter dated 28 June 1858.

<sup>80</sup> Lister, *op. cit.*, note 72, vol. II, p. 296.

<sup>81</sup> MS. in Sharpey-Schäfer papers.

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the cervical sympathetic on the pupil, nictitating membrane, and ear vessels of the cat, and it has an important place in the history of the physiology of the circulation.<sup>82</sup> There are other scattered references, in Lister's various writings, to experiments carried out by Sharpey. Sir John Erichsen, as a member of the Cardwell Commission, in 1875 (see Part II, section VII) spoke of experiments on asphyxia published under his own name many years before, but performed in fact by himself and Sharpey together. However there seems to be no evidence of any sustained piece of experimental work by Sharpey that remained unpublished. Rather, he experimented casually, to satisfy his curiosity at the time, or to enable him better to assess the statements of others.

#### V. SHARPEY'S TEACHING IN THE CONTEXT OF THE PHYSIOLOGY OF HIS TIME

The biological sciences developed with great rapidity throughout the nineteenth century. The advance was particularly impressive in the two decades from 1840 to 1860, a period that saw the publication of the best work of Claude Bernard and of Helmholtz, of Virchow's *Cellular Pathology*, of Pasteur's early experiments, and in 1859 of Darwin's *The Origin of Species*, to mention only the most outstanding. During this period, as it happens, Sharpey was at the height of his long career as a teacher, and he was thus peculiarly well placed to survey, and in his teaching to reflect, the very great changes in the methods and concepts of his subject that came about with such rapidity.

Sharpey was an excellent lecturer. Schäfer's assertion that his lectures were largely anatomical and rarely illustrated by experiments<sup>83</sup> is somewhat at variance with the impression which the reader of notes of his lectures receives. He may indeed have had little by way of apparatus; his use of his old chimney-pot hat to illustrate the principle of the kymograph is part of the Sharpey tradition! But it is clear that he did perform experiments before his class and Lister, for example, in his evidence to the Royal Commission in 1875 remembered 'the flood of light thrown upon Dr. Sharpey's physiological class by his demonstration to us of the effects upon the circulation, of the division of the sympathetic nerves in the neck.'<sup>84</sup> Sharpey himself told how 'five-and-twenty years ago, I was one among the very few medical teachers in this country who exhibited objects to students with the microscope. Indeed at that time we had to meet and answer objections to its employment; whereas now it has become almost a household instrument'.<sup>85</sup> It seems that for years he had, in fact,

<sup>82</sup> Fulton (J. F. Fulton, *Clio Medica series: Physiology*. New York: P. B. Hoeber, 1931) gives to Waller equal credit with Bernard and Brown-Séquard for the discovery of the role of the sympathetic nerve fibres in the control of the peripheral blood vessels. Waller, writing from London on 26 February 1853 (*C. r. hebdom. Séanc. Acad. Sci., Paris*, 36, 378–82) acknowledged Bernard's demonstration of the effects of section of the sympathetic nerves. Bernard, however, also claimed priority in the discovery of the effects of stimulating the cut peripheral end, reported in *C. r. Soc. Biol.* in November 1852 (*C. r. hebdom. Séanc. Acad. Sci., Paris*, 36, 417). The matter has been well reviewed recently by Hoff and Guillemin in *Claude Bernard and Experimental Medicine* (ed. Francisco Grande & Maurice B. Visscher. Cambridge, Mass., Schenkman, 1967, pp. 75–104) and also, from a different angle by A. E. Best in his paper 'Reflections on Joseph Lister's Edinburgh experiments on vasomotor control', (*Med. Hist.*, 1970, 14, 10–30). Waller's letter ends: 'With many thanks for the kind interest you have manifested in these inquiries, and the facilities you have afforded me for prosecuting them at University College.'

<sup>83</sup> Schäfer, *op. cit.*, note 1, p. 238.

<sup>84</sup> Royal Commission Report, p. 214, para 4313. See notes 204–5 below.

<sup>85</sup> See Bibliography, No. 13, p. 164. It is permissible to see in Sharpey's early attachment to the microscope the influence of his visits to Germany, where it was much more widely used at that time than in France.

only one microscope, which ran on a circular rail embedded in a table round which the students sat! The device is still to be seen in the office of the Thane Librarian in University College.

Our evidence for the content of his teaching is largely that of the various MSS. lecture notes, but from what has been said already it is obvious that Sharpey put much of his intellectual energy into his lectures, in which he often referred to original authorities and which, from their frequent 'stop press' flavour, must have had great appeal for the abler students. This was clearly the case with Lister who noted<sup>86</sup> concerning the relation of spermatozoa to ovum, that '... the seminal animalcules are seen adhering. Uncertain whether they penetrate or not. I [i.e. Sharpey] am not prepared to say the spermatozoa do not pass through the membrana pellucida. They certainly seem to have a very penetrating character . . .'. On the opposite page,<sup>87</sup> we find: 'This year [1852] Dr. Sharpey has given it as his opinion that the spermatozoa do not penetrate into the yolk; for within the last 3 weeks he has seen the experiments of Mr. Newport who has gone over them with the Dr. . . . J. L. 4th mo. 5th 1852'. Then in micrograph at the foot of the same page is written: 'It is now several months since Dr. Sharpey told me of the fallacy of the above statements, Newport and Dr. Sharpey being then both satisfied that the spermatozoa did penetrate the albuminous layers . . . J. L. 11th mo. 12th 1852'. That both teacher and pupil were in this case unusual men is perhaps shown by the following note concerning the decidua, on which Sharpey was an acknowledged authority. 'Dr. Sharpey mentioned this hypothesis again this year: [1852] but on speaking to him afterwards I found him not prepared to maintain it strongly. On the contrary he appeared to agree with my suggestions.'<sup>88</sup>

There has been some argument<sup>89</sup> about the reception in Europe of William Beaumont's work on the physiology of gastric digestion. Rosen,<sup>90</sup> while admitting Miss Miller's criticism of his original statistics, later amplified by Poynter,<sup>91</sup> maintained that with the exception of Andrew Combe the overall response in England was unenthusiastic when compared with that in Germany. However Sharpey, at least, referred in his undergraduate lectures late in 1836, to Beaumont's experiments, and the notes contain Beaumont's table of the digestibility of different foodstuffs.<sup>92</sup> Possibly his information came from Müller's *Handbook*, the original German edition of which quotes the experimental details at very great length. These were omitted by Baly in his English version.

To the overwhelming majority of the physiologists of the later nineteenth century, the word 'vitalistic' was a term of abuse. Burdon Sanderson, in his essay on Ludwig, clearly implies that an important reason for Sharpey's greatness as a teacher in his early days was his opposition to vitalism. He writes:

<sup>86</sup> Lister MS. f. 141 r., dated 10 April 1849.

<sup>87</sup> *Ibid.*, f. 413 v.

<sup>88</sup> *Ibid.*, f. 412 v.

<sup>89</sup> Geneviève Miller, Review of G. Rosen, *The Reception of William Beaumont's Discovery in Europe* (New York, 1942), in *Bull. Hist. Med.*, 1943, 13, 111–13.

<sup>90</sup> G. Rosen, 'Notes on the reception and influence of William Beaumont's discovery', *Bull. Hist. Med.*, 1943, 13, 631–42.

<sup>91</sup> F. N. L. Poynter, 'The reception of William Beaumont's discovery in England. Two additional early references', *J. Hist. Med.*, 1957, 12, 511–12.

<sup>92</sup> Potter MS., Vol. II, pp. 184, 191, and facing p. 205.

### *The Life and Teaching of William Sharpey (1802–1880)*

There were great teachers (such, for example, as Sharpey and Allen Thomson in England, Magendie in France, Weber in Germany) who discarded all vitalistic theories, and concerned themselves only with the study of the time- and place-relations of phenomena; men who were before their time in insight, and were only hindered in their application of chemical and physical principles to the interpretation of the processes of life by the circumstances that chemical and physical knowledge was in itself too little advanced.<sup>95</sup>

The vitalism of the first half of the nineteenth century was not, however, so simply explained as Sanderson thought, nor is it necessarily easy to allot a particular figure to one side or the other without more ado, as Temkin and others have shown.<sup>94,95,96</sup> That there were vitalists of different sorts and degrees, is evident from the application of the term to men so diverse as Barthez, Bichat, Liebig and Müller. Moreover men might and did change their views with the passage of time.

On one question, that of the forces required to circulate the blood, Sharpey took an uncompromisingly anti-vitalistic stand. In the 1830s the notion that the blood had a 'vitality' of its own was one of great antiquity and respectable ancestry, Harvey's strictures notwithstanding.<sup>97</sup> Stephen Hales had suggested that 'as electrical Bodies acquire greater Degrees of Electricity when rubbed in a cold than in a hot Air, so it is reasonable to think that the globules [of the blood] may acquire considerable degrees of elastic vibrations in passing the lungs.'<sup>98</sup> John Hunter in attempting to explain coagulation referred to 'the life of the blood'.<sup>99</sup> Johannes Müller thought it 'beyond dispute that the blood must be regarded as a living fluid', but added that 'hitherto observers have failed to detect a vital act in it as a visible phenomenon'. He went on to dismiss the ascription to vital forces, by Treviranus and other observers, of the motion of the globules seen for some seconds when a drop of freshly-shed blood is viewed microscopically, and could not confirm the description by Heidmann of contractions and dilatations in coagulating blood. Nevertheless he still thought that the blood 'must be regarded as endowed with life, for its actions cannot certainly be comprehended from chemical and physical laws', and that 'the blood also manifests organic properties; it is attracted by living organs which are acted on by vital stimuli; there subsists between the blood and the organised parts a reciprocal vital action in which the blood has as large a share as the organs in which it circulates'. He refused to admit however, that the blood was propelled through the vessels by any force other than that of cardiac contraction.<sup>100</sup>

Sir Charles Bell published in 1819 *An Essay on the Forces which Circulate the Blood*, in which he stated that 'the twin coats' of the blood vessels might abolish 'the common

<sup>95</sup> *Sir John Burdon Sanderson. A Memoir*, by the late Lady Burdon Sanderson, completed and edited by his nephew and niece. With a selection from his papers and addresses. Oxford, Clarendon Press, 1911, p. 284.

<sup>94</sup> O. Temkin, 'Materialism in French and German physiology of the early 19th century', *Bull. Hist. Med.*, 1946, 20, 322–27.

<sup>96</sup> G. J. Goodfield, *The Growth of Scientific Physiology*, Hutchinson, London, 1960.

<sup>97</sup> T. O. Lipman, 'Vitalism and reductionism in Liebig's physiological thought', *Isis*, 1967, 58, 167–85.

<sup>98</sup> *The Circulation of the Blood*. Two anatomical essays by William Harvey together with nine letters written by him. trans. K. J. Franklin. Oxford, Blackwell Scientific Publications, 1958, pp. 37–38.

<sup>99</sup> Stephen Hales, *Statical Essays: Containing Haemastatics*, 1733, (Reprint: New York and London, Hafner, 1964, p. 97.)

<sup>100</sup> John Hunter, *A Treatise on the Blood, Inflammation, and Gunshot Wounds*, with notes by James F. Palmer, Philadelphia, Haswell, Barrington & Haswell, 1840, p. 120 et seq.

<sup>100</sup> Müller, op. cit., note 49, Vol. I, pp. 152–54.

attraction of cohesion' which the heart by itself could not overcome.<sup>101</sup> Alison in his textbook thought 'that the Vital Affinities, which regulate the chemical changes that are wrought on the blood in the capillaries, must likewise be a cause of its movement there', and that whether the powers, admittedly auxiliary, residing in the small arteries 'consist merely in contractions of solids, or partly in *peculiar attractions* of solids and fluids, we cannot doubt that they are truly *vital*'.<sup>102</sup> Carpenter believed that 'we should altogether dismiss from our minds the idea of any *mechanical* assistance afforded by the action of the capillaries to the movement of the blood . . . something different from mechanical power must be the agent that operates in all the foregoing cases'.<sup>103</sup> He felt however, that Alison's vital attractions and repulsions, while very possibly real, were too similar to the processes of physics and chemistry to merit the description of vital.

Sharpey was opposed to all these theories. In his paper of 1831<sup>104</sup> he stated that he had never observed any spontaneous motions of the blood of the sorts described either by Treviranus and others,<sup>105</sup> or by Heidmann, and he suggested that a recent report by Czermak of spontaneous motion of the blood globules of certain invertebrates was in fact due to ciliary motion. He referred to the problem of the movement of blood in the capillaries of cold-blooded animals after the removal of the heart in his article on 'Cilia' in Todd's *Cyclopaedia*,<sup>106</sup> pointing out that

Haller . . . attributed it to some unknown power which he conceived to be exerted by the solid tissues on the blood and also by the globules of blood on each other. . . . The same opinion or a modification of it has been taken up by succeeding physiologists; accordingly many maintain the existence of a peculiar propulsive power in the coats of the capillary vessels different from contractility, or that the globules of blood are possessed of the power of spontaneous motion. Among others, Dr. Alison has adopted and extended this view in so far as he regards the motion of the blood in the capillaries as one of the effects produced by what he calls vital attraction and repulsion, powers which he conceives to be general attributes of living matter, or at least to manifest themselves in other processes of the living economy besides the capillary circulation. The motion in question has certainly not been as yet satisfactorily accounted for by referring it to the operation of known causes. . . . The necessity of the case therefore, though great, seems scarcely such as alone to warrant the assumption of a peculiar attractive or repulsive power acting on the blood at sensible distances, of whose existence in the animal economy we have as yet no other evidence.

He must have shown a draft or proof of his article to Alison, for the latter wrote to him claiming politely but firmly that he had been misrepresented, and that Sharpey was in error in some points.<sup>107</sup> Haller's observations for example, although most easily made in cold-blooded species, were not confined to them, while he himself had never implied that vital attraction and repulsion were the *only* forces acting on the blood in the capillaries. Alison was a believer in 'forces peculiar to life', and wrote

<sup>101</sup> Quoted by O. Temkin, 'Basic science, medicine, and the romantic era', *Bull. Hist. Med.*, 1963, 37, 97–129 (p. 115).

<sup>102</sup> William Pulteney Alison, *Outlines of Physiology*, Edinburgh, William Blackwood; and London, T. Cadell, 1831, pp. 42–44.

<sup>103</sup> W. B. Carpenter, *Principles of Human Physiology*, London, John Churchill, 1842, p. 416. See also pp. 417–18.

<sup>104</sup> Bibliography, no. 4.

<sup>105</sup> He lists Kielmeyer, Treviranus, Kreyssig, Carus & Döllinger—all of them Schelling's medical disciples.

<sup>106</sup> Todd, *Cyclopaedia* (see Bibliography, No. 8), p. 637.

<sup>107</sup> W. P. Alison to W. S. dated 24 May 1836. In Sharpey-Schäfer papers.

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at some length in an attempt to prove his case since, ‘having a strong conviction of the importance, both in Physiology and Pathology of establishing the principle of *Vital Motion in Fluids—independent of contraction and impulse in solids*, I am anxious to have you on my side in maintaining that principle which I know is only to be done by convincing you of its truth’. There exists, too, a draft of Sharpey’s reply to Alison’s letter, equally politely, but with equal firmness rejecting the criticisms;<sup>108</sup> the lack of other evidence for attraction and repulsion acting on the blood at sensible distances was ‘a point I am ready to defend against you, in all good humour’, and although ‘I should feel happy to be on the same side with one whom I so much esteem—in the meantime I must satisfy myself with being engaged in the same search after truth’. Despite disagreement, the two men seem to have remained on good terms, and Alison was one of Sharpey’s referees for the London chair a few months later.

All such theories, whether of attraction between the blood and the vessel walls, or of vital attraction and repulsion in blood itself, as Alison maintained, were discussed by Sharpey, at length, and with a wealth of evidence from experiment and observation, in the Potter, the Ballard and the Lister MSS. He showed by the simple expedient of injecting fluid into the aorta via a tube connected to a haemodynamometer, another similar device being inserted into the vena cava, that a pressure less than that capable of being exerted by the heart could keep up an appreciable pressure in the vein; thus clearly without the aid of any vital forces the heart could easily send blood from arterial to venous side of the circulation. He pointed out that those who admitted this, but still said there was a new force applied to the blood in the vessels, were of course very difficult to refute, but granted that in the foetus the blood circulated through the lungs for nutritional purposes, ‘it is singular how immediately after birth the attractions and repulsions should be changed, venous blood being attracted and arterial repelled when just before arterial was attracted and venous repelled’.<sup>109</sup> Sharpey in fact, appealing to the work of Müller, Poiseuille and others, and to the results of experiments done by himself, taught what we would regard as the basic principles of haemodynamics. Nevertheless, as late as 1857, we find Lister writing ‘it is not only in Edinburgh that persons think the vital affinities a cause of the blood’s movement; witness Dr. Carpenter’.<sup>110</sup>

The Potter MS. contains two passages which read like an anti-vitalist’s credo.

To the series of changes, actions, and phenomena which happen to an organised being from the time of its formation to that of its destruction, we apply the term life. But the term has also been applied to a supposed cause for these phenomena; for as we cannot explain these actions by reference to any of the Laws of Natural Philosophy (as gravity, capillarity, etc.) many refer them to supposed agency called Life or Vitality. We shall not however discuss the propriety of this as it would lead us too far into *hypothesis*.<sup>111</sup> . . . When, in Physiology, we say that any phenomenon takes place from vitality or life, it is generally only another way of saying we know little or nothing about it. When we cannot refer it to any Physical cause, with which we are acquainted, we are always ready to ascribe it to vitality which is only a vague way of getting rid of the subject.<sup>112</sup> . . . We shall not here notice any arguments for the theory of Hunter

<sup>108</sup> W. S. to W. P. Alison dated 26 May 1836. In Sharpey-Schäfer papers.

<sup>109</sup> Ballard MS., Vol. I, p. 190.

<sup>110</sup> Rudolf, *op. cit.*, note 75, p. 146, letter dated 16 November 1857.

<sup>111</sup> Potter MS., Vol. I, p. 5

<sup>112</sup> *Ibid.*, Vol. 1, p. 193.

respecting the life of the blood, but I may just refer to one objection which is often urged against it which is certainly not valid. It is often said that it is absurd to suppose a fluid to be possessed of vitality, though we can understand how a solid could be a living body. Now this difference between solids and liquids is perfectly arbitrary and entirely gratuitous, we see no reason why one should not possess life as well as the other, nay, we have an example of a fluid which evinces all the signs of life shown by any solid. I allude to the fluids of the egg, we see them undergoing vital changes, we see them resisting putrefaction, although when dead they soon decompose. This therefore is not a valid objection.

This passage is by no means the only one in which Sharpey uses the word 'vital' but elsewhere he does so with a somewhat different intention. It occurs several times in the Ballard MS., for example:

Irritability is that vital property by which muscles contract.<sup>113</sup> . . . Tonicity is a vital property not found in dead muscle.<sup>114</sup> . . . all the phenomena observed in the animal economy cannot however be explained by application of the laws of chemistry and physics. Take for instance the function of respiration. There is here 1° the physical or mechanical act of inspiration or expiration, 2° the chemical change which occurs in the air and blood and 3° the muscular contractions by which the physical act of inspiration is affected: this is neither a chemical nor physical process but depends on an inherent property of muscle when in a state of life and is therefore a *vital property*.<sup>115</sup>

Müller's theory of penile erection<sup>116</sup> as a vital attraction between the internal lining and the blood was put forward. The process was similar, Sharpey said, to the increase in blood flow to a secreting gland, a vital turgescence.<sup>117</sup> He maintained that the action of pepsin, however, was analogous to the action of yeast on sugar, and it was obviously erroneous to say that a vital power was involved.<sup>118</sup>

A decade later, the Lister MS. contains a lecture devoted to a consideration of the vital and physical properties of the animal body.<sup>119</sup> Matter assimilated undergoes a change in form, taking on a definite shape. In many cases there is a change in chemical properties, and:

Some have proposed to distinguish the forces occasioning these.

Vis plastica that force by which matter undergoes a change in form.

Vis metabolica that by which chemical changes are brought about, this also called Vital or Chemico-vital affinity.

Except in an<sup>1</sup> & veg<sup>1e</sup> world we know of no force that effects similar change. For though the results are chemical and physical, the actions cannot be explained by known chemical & physical laws.

'Vital Contractility or Irritability' is discussed. 'The will, & emotions of the mind, cause the development of this force . . . Haller [& Sharpey] consider the irritability to reside in the muscle but the nerves to have the power of communicating this; hence he speaks of two properties, Vis insita and vis nervosa'. Sharpey's apparent agreement with Haller is interesting. He had earlier described Haller's *Elements* as 'not only the best work on Physiology but the most philosophical elaborate and complete work which has appeared in age on in any country'.<sup>120</sup>

<sup>113</sup> Ballard MS., Vol. 1, p. 40.

<sup>114</sup> Ibid., Vol. 1, p. 44.

<sup>115</sup> Ibid., Vol. I, p. 14.

<sup>116</sup> Müller, op. cit., note 49, pp. 224–25.

<sup>117</sup> Ballard MS., Vol. II, p. 179.

<sup>118</sup> Ballard MS., Vol. I, p. 158. This is rather differently put in the Lister MS. (f. 207) viz. 'It is possible no doubt that pepsin possesses vital props. that lie dormant; more probly a chem. change.'

<sup>119</sup> Lister MS., ff. 11–13.

<sup>120</sup> Potter MS., Vol. I, p. 2.

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Physiologists at this period were indeed in something of a dilemma when it came to evolving explanatory concepts for their observations. By the 1830s it was obvious that earlier objections to measurement in biology such as those of Bichat, and to the experimental approach to the problems of function in living organisms (held not only by extreme vitalists but also by sceptical materialists such as Rudolphi)<sup>121</sup> had lost much if not all of their support. Most physiologists were prepared to grant that many of the phenomena relating to living things fell into the same category as those of inanimate nature. On the other hand there were many properties of living organisms which could not be so classified. Faced with these, the physiologist could refuse to pronounce, or could offer an explanation which was either a declaration of faith that, given time and effort, the phenomena of life would be shown to be explicable in terms of physics and chemistry—as did, for example, Du Bois-Reymond and his colleagues in declaring their intention ‘die Wahrheit geltend zu machen dass im Organismus keine anderen Kräfte wirksam sind als die gemeinen physikalisch-chemischen’<sup>122</sup>—or finally, he could have recourse with more or less grace to some sort of ‘vital’ force. Even Magendie was forced to admit that ‘in truth living organs present phenomena which can never be explained by physical laws’ and to reduce the appearances ‘to two principles, which are nutrition and vital action’. He pointed out, however, that to make the insensible molecular motion underlying these principles depend on a ‘vital force’ is tautology, not explanation. ‘Moreover’, of all the illusions of modern physiologists, ‘the most deplorable has been that of believing that by forging a new term *vital principle* or *vital force* they have done something analogous to the discovery of gravity’.<sup>123</sup> Magendie’s scepticism was extreme for his time. Goodfield implies very properly, that it would be unfair to assume that the sole motive for using vitalistic concepts is the concealment of ignorance. A vitalist such as Liebig had many reasons, including religious ones, for using the concepts he did.<sup>124,125</sup> Goodfield, indeed, suggests that to him should be accorded some of the credit for the enunciation of the principle of the conservation of energy.<sup>126</sup> Another vitalist, W. B. Carpenter, stated quite clearly that he started from the assumption that all force not arising from the will of created sentient beings, derived from the Will of the Creator. Carpenter was a good physiologist and all-round biologist, and a writer of widely-read textbooks, of which a contemporary wrote that ‘It is hardly an exaggeration to say that the majority of medical practitioners . . . owe all that they know of physiological science to some one or other of Dr. Carpenter’s treatises’.<sup>127</sup> In his paper ‘On the Mutual Relations of the Vital and Physical Forces’,<sup>128</sup> he imagined a relation between vital forces

<sup>121</sup> ‘On the Life and Writings of the later Professor Rudolphi. By his successor, Professor Müller of Berlin’, *Edin. New Phil. J.*, 1838, 35, 221–42 (p. 326).

<sup>122</sup> Quoted by K. E. Rothschuh in his *Geschichte der Physiologie*, Berlin, Göttingen, Heidelberg, Springer-Verlag, 1953, p. 140.

<sup>123</sup> F. Magendie, *Elementary Compendium of Physiology*, 4th ed., trans. E. Milligan, Edinburgh, J. Carfrae, 1831, pp. 14–19.

<sup>124</sup> Goodfield, *op. cit.*, note 95, p. 135 et seq.

<sup>125</sup> Lipman, *op. cit.*, note 96, p. 185.

<sup>126</sup> Goodfield, *op. cit.*, note 95, p. 147.

<sup>127</sup> Quoted by E. Gaskell, in *Medicine and Science in the 1860s*, ed. F. N. L. Poynter, London, Wellcome Institute of the History of Medicine, 1968, p. 290.

<sup>128</sup> W. B. Carpenter, ‘On the mutual relations of the vital and physical forces’, *Phil. Trans. R. Soc., Lond.*, 1850, 140, 727–57.



analogous to that shown by Faraday and others between physical forces. Cell force is channelled in a particular direction by a particular tissue. Electric force acts on a nerve for a short distance and is then transmuted into nerve force; the reverse holds in the case of the electric organ. Chemicals act on nerves, and nerves act on organs to change the chemistry of their secretions. At death all materials and forces are restored to the inorganic universe. Sharpey's scepticism thus seems to bring him nearer to Magendie than to many of his contemporaries. He had recourse on occasion to the language of vitalism, but only to describe and not to explain, and only because he admitted frankly that there was much in physiology not to be explained in terms of physics and chemistry, as things then stood. Incidentally, he appears, in his introductory lectures to owe something to Magendie's *Compendium* although it was not in his list of recommended books.<sup>129</sup> A table of differences between the inorganic and the organic is identical with that in Magendie's volume, as is a table of the differences between animal and vegetable kingdoms and Sharpey's division of man's needs into the functions of nutrition, relation, and reproduction.<sup>130</sup>

Sharpey visited Germany on at least three occasions between 1823 and 1831.<sup>131</sup> German physiology throughout that period was still deeply impregnated with *Naturphilosophie*. Amongst the few who remained hostile or merely indifferent, were Rudolphi, under whom Sharpey studied, and E. H. Weber, with whom he seems to have been on friendly terms. Nevertheless there is a little evidence which suggests that Sharpey was not entirely unaffected by doctrines of which he could scarcely have failed to be aware. Many years later he referred to Oken whom he heard in the German Association of Naturalists in Berlin in 1828, as 'one of the most distinguished members of that institution'.<sup>132</sup> One of the testimonials (that of Craigie) accompanying his application for the University College London Chair stated that he was well qualified in anatomy, both general and *transcendental* (my italics—D.W.T.) and in their application to physiology. And in the last of the five lectures printed in the *Lancet* in 1840–41 he admits that there is good reason for accepting the vertebral theory of the skull (but so did Rudolphi to some extent).<sup>133</sup>

Sharpey strongly disbelieved in spontaneous generation.

This is a subject which has long been agitated, namely whether it was necessary to suppose in all cases the existence of parents or whether in some of the lowest tribes of animals they may arise independently of parents. Some of the older naturalists made use of the doctrine when there was no necessity for it as in accounting for the generation of animals by putridity. They even went so far as to imagine that mammiferous animals might arise by spontaneous generation as mice from the mud of the Nile—Modern Physiologists have restricted the doctrine to those

<sup>129</sup> These were, in 1840, the books of Müller, 'the only work . . . which contains a sufficiently full statement of the present knowledge of physiology', Carpenter, Bostock, 'a vast store of reference to the literature', but out of date, Elliotson, and finally Haller 'The great founder of modern physiology . . . still a book which I find indispensable for both anatomy and physiology' (*Lancet*, 1840–1, i, 142–43 and Ballard MS.). In 1849, the recommended books were those of Kirkes, Paget, Carpenter, Todd and Bowman, Müller, and Wagner. (Lister MS.).

<sup>130</sup> Of course, Magendie may have borrowed in his turn. The inorganic as of angular form and indeterminate volume, as opposed to the organic, of spherical form and determinate volume, was in fact a tenet of 'Naturphilosophie' (see E. S. Russell, *Form and Function*, London, John Murray, 1916, p. 98).

<sup>131</sup> That he was in Berlin in October 1823 is clear from a matriculation receipt in Arbroath Public Library. Müller also went to Berlin in that year to work under Rudolphi.

<sup>132</sup> See Bibliography, No. 14, p. 77.

<sup>133</sup> Müller, op. cit., note 121, p. 233.

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cases in which new animals of very simple structure have appeared to arise in circumstances which would preclude the possibility of previously existing parents. They thus explain the origin of Infusory animalcules and entozoa—No one has ever seen their actual origin without a parent and it is only inferred from their origin being known to occur where parent animals or emanations from them or ova have been supposed excluded. It is known however that when such animals are once produced they can generate their species in the usual manner. The advance of Physiology and microscopic investigation have shown the error of these conclusions. It was supposed that boiling any matter would destroy any parent animals or their ova but we now know that ova can sustain a temperature much higher than was previously believed—Those animals again which were once supposed not to propagate their species have since been shown to have distinct sexual organs and ova—The doctrine is altogether inconsistent with the general mode in which nature proceeds.<sup>134</sup>

In the early sets of lecture notes, some space is devoted to comparative anatomy. In the Potter MS., there is a review of the digestive organs and their function in the lower animals, extending to the Infusoria which are said amongst other things to possess ‘a wonderfully complex set of teeth’.<sup>135</sup> The inclusion of this material is less surprising when one considers Sharpey’s earlier paper on Ehrenberg’s researches on the Infusoria.<sup>136</sup> In it, he accepts Ehrenberg’s assurance that he has demonstrated in these organisms an alimentary canal with mouth and teeth, a system of muscles, organs of generation, and other structures. Some of these preparations Ehrenberg had shown to him in Berlin in 1831! Ehrenberg’s very beautiful illustrations do indeed show the structural details of the organisms he examined.<sup>137</sup> What calls for explanation is his interpretation—and Sharpey’s. It is clear, however, from Sharpey’s paper that the demonstration of structural complexity in such organisms was regarded by him as strong evidence against, on the one hand, the theory that they were produced by spontaneous generation, and on the other hand, the theory of ‘universally diffused vital matter’, and ‘living organic molecules’.<sup>138</sup> The doctrine that larger organisms were but an aggregation of infusoria was part of the canon of *Naturphilosophie*.<sup>139</sup>

There are many other passages of miscellaneous interest to be found in these three sets of lecture notes—recognition of the fact that ‘a patient suffering under cholera may be relieved (tho’ only for a time) in a most extraordinary degree by the injection of saline solutions into his blood’:<sup>140</sup> a quotation, with apparent agreement,<sup>141</sup> of the view of Tiedemann and Gmelin that fresh pancreatic juice is acid, despite the evidence given in Müller’s *Handbook* (this is corrected by 1849–50): and criticisms of Magendie, ‘(who is never very particular as to the humanity of his experiments)’, such as, for example ‘The argument which M. Magendie has here indulged in [that since swallowing is not prevented by removing the epiglottis, that organ plays no part in the process] seems to me to be as much void of reason as if after amputating

<sup>134</sup> Ballard MS., Vol. II, p. 173.

<sup>135</sup> Potter MS., Vol. II, p. 2.

<sup>136</sup> Bibliography, No. 5.

<sup>137</sup> C. G. Ehrenberg, *Die Infusionstierchen als vollkommene Organismen*, Leipzig, L. Voss, 1838.

<sup>138</sup> See Bibliography, No. 5, p. 289.

<sup>139</sup> Lorenz Oken, *Elements of Physiophilosophy*, trans. from the German by Alfred Tulk, London, printed for the Ray Society, 1847, p. 189.

<sup>140</sup> Potter MS., Vol. I, p. 203. Sharpey doubtless knew of the work of O’Shaughnessy and others during the cholera epidemic of 1831–2. Incidentally, Creighton (*A History of Epidemics in Britain*, London, Frank Cass, repr. 1965) states (Vol. II, p. 833) that saline injections were not infrequently used but with differing opinions about the results, and that one journal provided statistical evidence of their efficacy.

<sup>141</sup> *Ibid.*, Vol. II, p. 165 and Ballard MS., Vol. I, p. 146.

one of the dog's legs and finding he could still walk on three he should conclude that the 4th leg of the dog is useless'.<sup>143</sup> (But see Part II, section VIII for a possible explanation of why Sharpey's charity did not extend to Magendie.) Under the heading of 'Mental Faculties', in the Ballard MS. there is an extensive refutation of the doctrines of phrenology, of which Sharpey was a well-known opponent.<sup>143</sup> The Lister MS. contains a discussion of Bernard's work on the glycogenetic function of the liver, which at the time had not long been published, another example of how Sharpey stimulated the interest of the better students.<sup>144</sup> Sharpey must have been one of the first teachers of physiology to incorporate the new concept of the cell theory into an undergraduate course in general anatomy (or histology) just as he was amongst the earliest to use the microscope in teaching medical students. In the Potter MS., written late in 1836, Bichat's classification of the textures, or tissues, is given in full, as is that of Richerand and Dupuytren.<sup>145</sup> 'Cellular tissue' means what came to be called 'areolar connective tissue'. In the Ballard MS. written in 1840, these classifications are again given, but there is a discussion of the work of Schleiden and Schwann.<sup>146</sup> 'The fundamental part of vegetable structure is the cell, either alone or several added together'. 'Schwann applied these views to animal development'—a correct assessment, in fact. 'As the several textures are modifications of the cell, so is the embryo of animals first a cell. New structures, as diseased growths are developed in the same way, as malignant cancer. This is the same in the repair of each part'. There is a discussion of Schleiden's views on the origin of cells, and of the origin of the nucleus according to various authorities. The Lister MS. also contains a substantial section on the reduplication and transformation of cells.<sup>147</sup>

After Lister's few addenda, written in 1852 to his notes taken in 1849–50, we have no evidence about the substance of Sharpey's teaching until the Thane MS. dated 1867. These notes are much less compendious than are those of the earlier manuscripts. Even so, it is easy to see that physiology had become a much more mature science. The language is in essentials modern, and frequently biochemical. There are references to measurements—of the work of the heart, of the vital capacity, of the gases in arterial and venous blood. Chemical formulae, scarcely to be found in the Lister MS., are widely used. As always, the teaching is up to date, and incorporates the advances in knowledge made in the intervening fifteen years, during which physiology had undergone such momentous development.

In his presidential address to the British Medical Association in 1862, and, much more briefly, in his presidential address to the Section of Biology at the British Association Meeting of 1867, Sharpey adduced five main reasons to account for the impressive increase in physiological knowledge during the previous quarter of a century. These reasons were, the establishment of physiological laboratories, especially

<sup>143</sup> *Ibid.*, Vol. II, p. 46 and pp. 72–73.

<sup>144</sup> Ballard MS., Vol. II, pp. 106–14.

<sup>145</sup> Lister MS., f. 32 and ff. 215–16.

<sup>146</sup> Potter MS., pp. 12–13.

<sup>147</sup> Ballard MS., Vol. I, p. 31–2 and facing p. 30. Sharpey does not state whence was derived this interesting forecast of cellular pathology. According to Th. Schwann (*Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants*, trans. from German by Henry Smith, London, Sydenham Society, 1847. Author's Preface p. xviii, dated March 1839) J. Müller and Henle applied his theory to the most important pathological processes.

<sup>147</sup> Lister MS., ff. 40–44.

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in the German universities, giving encouragement and facilities for research; largely as a result of this, a great increase in the numbers and activity of physiological workers; improved methods and appliances in investigation, particularly improvement in the design, and extensions in the use of the microscope, on which the cell theory, for example, ‘one of the most fertile ideas in modern physiology’, had obviously depended; greatly increased recognition of ‘the importance of exact determination, whether as to time, space, or quantity, and its general application to those phenomena of the animal body which are susceptible of it’; and finally progress in physiological chemistry.<sup>148</sup>

If problems still remained which eluded physical explanation ‘it must at least be counted as no small gain that we no longer seek their solution in the agency of imaginary entities, clothed with mystical attributes like the Archeus of Van Helmont, the Anima of Stahl and the Vital Principle of later theorists, nor vainly strive to square them to the dogmas of some dominant vitochemical or dynamical system’.<sup>149</sup> A spirit of exactness is now seen to pervade physiology. The important questions are the intermediate changes undergone in the body by the items in the chemical balance sheet drawn up by Liebig and his school, and to the solution of which Bernard’s work on glycogen may be the prologue; the demonstration of an exact equivalence between heat and work in living bodies and the chemical forces which recent studies in nutrition have shown to be set in action; and the influence of the nervous system on organic and nutritive function, together with the definition of the nervous force, which has long been likened to electricity, and may be related to it, as other physical forces to each other in the way that first Rumford and then Joule have supposed. The physiology of reproduction poses many problems, although the case for spontaneous generation seems to have been finally disposed of by Pasteur. There is reference to ‘the doctrine of homology which took its rise through the genius of Goethe’ and, as extended by Geoffroy St. Hilaire, Meckel and Owen, has a ‘surpassing beauty and interest’.<sup>150</sup> However in the ovum itself can be detected no feature to indicate the course of its development in form and in time. This remains an unpenetrated mystery—‘unpenetrated I mean as regards the physical or efficient causes of the phenomena; for the purpose or final cause is often patent; and hence we see that teleological explanation holds and doubtless must continue to hold a large place in physiology’.<sup>151</sup> The language of Aristotle is still deemed appropriate! Nevertheless he continues, these facts are no reason for giving up the endeavour. Faint as some may regard the prospect of success for Darwin’s theory, it is not to be condemned on that account. Any explanation of adaptation and harmonious function in living organisms in terms of natural causes can only ‘exalt our conceptions of creative wisdom’.<sup>152</sup>

*To be concluded in our next issue*

<sup>148</sup> Sharpey was well aware of the value of chemistry to physiology, and had Harley appointed in 1856 to teach what was essentially practical physiological chemistry. He had himself attended the class of chemistry along with the students in 1852 (*Letters and Journal of W. Stanley Jevons*, edited by his wife, London, Macmillan, 1886, p. 15).

<sup>149</sup> See Bibliography, No. 13, p. 162.

<sup>150</sup> *Ibid.*, p. 169.

<sup>151</sup> *Ibid.*, p. 171.

<sup>152</sup> *Ibid.*, p. 171.