Sir Walter Morley Fletcher—1873-1933.

Sir Walter Morley Fletcher died on June 7, 1933, with unlooked-for suddenness when he was just approaching his sixtieth birthday, but when his physique and brain were still those of a man in the most vigorous prime. Years of fruitful work had seemed to lie ahead of him, for he embodied the strength of a family of tenacious vitality. His father and mother had each lived beyond the ninetieth year, and all their ten children were alive when they celebrated the fiftieth anniversary of their wedding day. Walter Fletcher was their youngest and sixth son, being born in 1873.

His parents on both sides were from Yorkshire, and of that independent spirit which showed itself in Nonconformity to the Church. They were Congregationalists, and in each of them religion was blended with a sensivenes to art and culture that made goodness and beauty seem to them almost the same. Walter Fletcher never lost these spiritual impressions that he received in his early years at home. His praise of clean scientific work rose to its highest appreciation when he spoke deliberately of its beauty, and characteristically in public addresses he was wont to choose a phrase from the Bible or related sources when he wished to express emotions that had been stirring deeply within him.

His father, Alfred Evans Fletcher, had considerable intellectual gifts. Like many other Nonconformists of his time he went as a student to University College, London, where mathematics and chemistry, his chief interests in life, were then taught by de Morgan and Hoffmann at least as well as at Cambridge. In 1851 he won the Gold Medal for chemistry in competition with a group of later distinguished chemists, among whom were Sir Henry Roscoe and Sir Frederic Abel, and soon afterwards he became associated with the aniline dye process in the industry first introduced by William Perkins, but the business failed. Alfred Fletcher then accepted a permanent appointment as Inspector of Alkali Works under the Local Government Board, first in Liverpool and later in London as Chief Inspector.

His mother was a Morley, cousin of a notable philanthropist, Samuel Morley, whose son became Lord Hollenden, and cousin through her mother of Herbert Henry Asquith, the late Prime Minister. She was a woman of great ability and strong character, with determination that her children should attain power in affairs.

Chemistry and engineering as technical subjects were well-known aims in the family, but there had been no connection with medicine. The second son, Herbert Morley, recently Senior Physician to St. Bartholomew's Hospital, was the first to enter this field of work; and with him it was a late choice for he had gone up to Cambridge in 1884 with the intention of studying chemistry.
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But he found matter of more interest in the medicine that was associated with Foster's developing school of physiology, and he stayed up for a year of research in that subject. Cambridge also gave him the opportunity of discovering athletic ability in the family, for he ran in the 100 yards and the quarter-mile against Oxford and was President of the C.U.A.C. in 1888.

These doings by his elder brother must have shaped the ambitions of Walter Morley who from his London home was then going as a day boy to University College School, where Michael Foster had been forty years earlier. But the power to realize them was at first of slow development. Beyond being secretary of the boys' scientific society and keeping a small museum he did nothing notable at school; and his rapid growth had taxed his strength, so that it was found advisable to send him abroad for a few months before he went up in 1891 to Trinity College, Cambridge, where he could gain nothing higher at entrance than a sub-sizarship. He at once took physiology as his chief aim, with qualification in medicine as auxiliary. His powers now ripened quickly and, though never a Major Scholar of the College, his grasp of physiology soon won for him many distinctions and finally in 1897 the Trinity Fellowship, which gave security for a life of scientific work in the Cambridge society which he had learned to enjoy so fully in every one of its varied aspects.

His sensitiveness to all forms of artistic beauty and his vivid enjoyment of their pleasures gained for him even in his undergraduate days the close friendship of men who had little contact with the sciences. And at the same time his strength of body and his quickness in all forms of sport, in which and in the society they offered he took a natural delight, opened to him a ready acquaintance with the group of athletic undergraduates who rarely intermingled with men of purely intellectual interests. He ran in the hurdles as second string against Oxford, but it was not until after a visit with the University team to America that he attained a really good style in hurdling, and also learned to throw the hammer so powerfully that his throw, made later when he was studying medicine at St. Bartholomew's, is still a record for the Inter-hospital sports.

These pathways to many friendships were made still easier by a quick attractiveness of manner, a gaiety that broke out unexpectedly and could not long remain submerged by serious thought, and by a never hidden capacity for affection. Fletcher felt, and with him the feeling was at once outspoken, an affection for Trinity College which rivalled his devotion to Foster's school of physiology. Hence, as the years moved on, he welcomed every opportunity for service to his college, and his Tutorship, 1905-14, with its many human interests and its openings to the administrative work that he enjoyed, received a progressively greater share of his time and thought than did his own research in physiology. In the University he served as a Proctor, a vigorously successful and yet a popular one, for the year before his Tutorship; and he was President
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of the Pitt Club, an undergraduate society which selected its entry on principles that were emphatically not proletarian, from 1899 until he left Cambridge in 1914.

Fletcher began research with some minor work, published in 1898, on the anatomy of sympathetic nerves which had obviously been suggested to him by Langley; but very quickly he found his own subject, that of the "respiration" of muscle. No one at the time was working on this question in the laboratories at Cambridge, and it may have been that Fletcher's choice was determined partly by a leaning to chemical thought and exact measurements derived from his father, and in particular by the opportune fact that F. F. Blackman in the botanical laboratory had recently devised an extremely sensitive apparatus for studying the gaseous exchange of leaves, which made it possible to measure the discharge of very small quantities of CO₂ at frequent intervals of time.

Methodical study of the time relations of tissue respiration in exact reference to each physiological phase were not then in familiar use. Workers lacked the sensitive apparatus needed for frequent small measurements, and they had perforce been content with a gross contrast between what was regarded as the beginning and the end of the phenomenon. This missed all the subtle gradations of the biological change; and the results of different observers were often antagonistic because they had chanced on different end points. But even worse than the tangle caused by conflicting experimental evidence was the intricate chemical theory of muscular contraction that had been devised by German physiologists and seemed almost to deny any clear pathway to further experimental enquiry.

Animal respiration in its simplest formula consists in the intake of oxygen and the exhalation of CO₂ and water. But it had been proved by Spallanzani (1803) and Liebig (1850) that muscle could contract and relax in the absence of oxygen, and it had further been shown that lactic acid was produced in fatigued or dying muscle. Out of these facts and some inadequate additional data Hermann (1867) had synthesized his conception of a giant "inogen" molecule in muscle. This according to Pflüger (1878) stored up intramolecular oxygen and then disrupted at the instant of mechanical contraction, yielding as immediate products of combustion CO₂, water, lactic acid, and some other nitrogenous residua. This conceptual pattern of the biochemical processes in muscular contraction was soon extended to other tissues; it was interwoven with a coloured fancy of anabolism ever rising up to protoplasmic mysteries, and catabolism in return thrusting all back to simpler bodies. Verworen pictured in each cellular activity some such complicated processes, and his giant Biogen molecule straddled across the whole breadth of the road of all who sought experimental access to knowledge of the modes of cell metabolism.
Fletcher made no mention of these difficult theories in his earliest papers. He simply took up in turn the two main products, CO₂ and lactic acid, and sought to determine precisely the time relations of the appearance of each in reference to muscular contraction and to the supply of oxygen. His first paper in 1898, embodying work that had gained his Fellowship at Trinity, analysed the progressive discharge of CO₂ from excised frog's muscle in air, that is with "survival respiration," under various conditions of rest and contraction. Errors due to the respiration of contaminating micro-organisms were carefully excluded. When the muscle was stimulated to contract that sudden outburst of CO₂, which all the text-books of the day described in deference to prevailing theory, was found to be non-existent.

Research work, though not the routine of Cambridge teaching, was now interrupted and during the next three years, 1897–1900, Fletcher obtained his medical qualification by travelling thrice a week to St. Bartholomew's Hospital, where he gained that knowledge of clinical problems which in after years was to be of such undreamt of importance to his work. His only period of study abroad followed three years later in Professor Meyer's laboratory at Marburg, where he did a brief piece of work, published in Brain, 1903, on tetanus dolorosus. The underlying hope seems to have been that tetanus toxin, which was apparently incapable of transference across cell synapses, might be employed as an instrument for physiological dissection of the central nervous system, in a way similar to Langley's memorable use of nicotine on ganglia of the autonomic system. He found that the toxin did not irritate the cells of either the superior cervical or the posterior root ganglia and the results, though clear, did not seem to justify further work in that direction.

After his return from London to the Cambridge laboratory the next step (1902) was to ascertain the influence of oxygen and of its absence, by using an atmosphere of nitrogen, on the progressive evolution of CO₂ and on the vitality of excised muscle. The experiments showed that frog's muscle was capable of repeated contraction and relaxation in the absence of oxygen and that in such circumstances it produced very little CO₂; but that its recovery from fatigue or from approaching rigor after such anaerobic contractions was greatly aided by oxygen, which then led to a discharge of CO₂ during this process of recuperation but not during the contraction.

These results became comprehensible when measurements were made of the simultaneous formation of lactic acid, and in the technique of this essential work Fletcher now had the important co-operation of Hopkins, who had recently gone to Cambridge from the physiological laboratories of Guy's Hospital. Their joint work was published in 1907, and it made a great clearing in the place of the old tangled confusion. Previous observations on the formation of lactic acid in muscle had been at variance because no one had started from the base line of a genuinely resting and uninjured muscle. Also
no one had examined the possibility that lactic acid after formation might be progressively removed by oxidative processes within the muscle itself. Fletcher and Hopkins proved that resting and uninjured muscle contains very little lactic acid; that excised muscle when fatigued by contraction forms lactic acid up to a definite maximal percentage at which irritability is lost; and that a free supply of oxygen can then cause the lactic acid to disappear with restoration of irritability and a coincident evolution of CO$_2$, which is, however, not fully equivalent to that which would come from complete combustion of the lactic acid. But the repeated cycle of the formation of lactic acid by contraction and its subsequent removal under the influence of oxygen did not seem to deplete the storehouse from which the acid could appear in excised muscle, for at the end of a long series of contractions and recovery the muscle could still yield its normal higher percentage of lactic acid in the irrecoverable death of heat rigor.

With these important facts established Fletcher now returned to a fuller experimental analysis of his own earlier curves on the progressive discharge of CO$_2$ from excised muscle; and in the last paper published by him, with G. M. Brown in 1914, he showed that not a single one of the processes leading to the liberation of CO$_2$ was directly and immediately concerned with muscular contraction. Some CO$_2$ appeared as the freshly formed lactic acid neutralized preformed carbonates in muscle; more appeared as the lactic acid was removed during recuperation under oxygen; and still more could be released if the muscle was destroyed by boiling, when its complex constituents were dissociated. There was no escape now from the conclusion that that part of the respiratory process which absorbed oxygen and evolved CO$_2$ did not belong to muscular contraction as a combustion, but that it was a subsequent act concerned with the removal of fatigue products and with the supply of energy needed for replacing lactic acid in the molecular position from which the stimulus to contraction had in some way displaced it.

In this paper Fletcher at last allowed himself to discuss the inogen theory and Pflüger's views of intramolecular oxygen, to show the imperfection of the experimental evidence upon which they had been built, and to give in their place the facts which are the bedrock of modern work on muscular contraction. Only on one main line was his work at variance with subsequent discoveries. He insisted that the oxidative removal of lactic acid ceased when the muscle was cut up and its architecture destroyed; and he believed that it was a subtle process intrinsic to living muscle and incapable of repetition as a simple chemical reaction under the influence of extracted oxidases.

Fletcher's own work in the laboratory ended early in 1914, but the whole story of the respiratory processes in muscle was reviewed by Fletcher and Hopkins in their joint Croonian Lecture of 1915 to the Royal Society. The argument was given a wider scope by reference to A. V. Hill's new measure-
ments of the heat production of a single contraction of muscle, which made it possible to trace the successive thermodynamic changes as lactic acid was produced anaerobically during contraction and then slowly removed under the influence of oxygen. Very tentatively it was suggested that lactic acid, which could no longer be regarded as a mere waste product of combustion, might actually be the very agent of the tensile changes, and that in the muscle at rest it was resynthesized to the simple carbohydrate from which it had emerged. But unhesitatingly all the old mysterious conceptions of cellular activity as moving through phases of complex biogen molecules were swept aside. The main biochemical processes involved in muscular contraction appeared to be as simple as they were swift, and all the experimental results fell within a view that the peculiar machinery of the muscle held some simple stuff akin to sugar within itself, and was moved this way and that by changes in the latter.

This simplification of thought gave a most important analogy for experimental enquiry into the metabolism of other tissues. The peculiar protoplasm of the cell might be dealt with as a machinery which passed through it many food stuffs that never rose above lower levels of simple chemical changes, and were therefore accessible to direct analysis. The identification of these simpler changes and of the enzymes and catalysts determining them has become a most fruitful aim of modern biochemistry; and the possibility of such work arose when Fletcher’s accurate analysis of the respiratory processes of muscle had dispelled the old myths. His work, and particularly that on lactic acid with Hopkins, was therefore of cardinal value, and on it pivoted a great change in the directions of biological thought. For this reason the work is almost always referred to by modern physiologists as “classical.” The experiments were what Claude Bernard himself would have described as “good”—clearly planned, accurately recorded, and yielding results that allowed important conclusions to be drawn from them in all security. Subsequent developments of knowledge, first with regard to glycogen and then to the importance of phosphate-containing substances in muscular contraction, have in no way invalidated the original observations on lactic acid, and these recent discoveries would in all probability not yet have been made but for the interest in lactic acid awakened by the work of Fletcher and Hopkins. In the larger view, Fletcher’s analysis of the respiration of muscle, as distinct from the general respiration of the body, was among the first of the modern quantitative studies that are transforming biochemistry from a mere “anatomical” identification of substances that can be extracted from tissues to a science describing the physiology of the chemical changes within the cell itself.

The Cambridge school continued to make notable advances in the study of the physical side of muscular activity, and two men who had been closely associated with Fletcher as his pupils at Trinity were conspicuous in this work. But he probably gave no more to either Keith Lucas or A. V. Hill than a keen
interest in physiology and unlimited encouragement; and it was Langley who in 1909 made to the latter the fruitful suggestion that he should attempt thermodynamic studies of frog’s muscle under the various conditions recently analysed from the chemical aspect by Fletcher and Hopkins.

Indeed, from 1905 onwards, when he accepted a Tutorship at Trinity College, Fletcher allowed his wider interests to withdraw him more and more from daily life in the laboratory, so that other active workers missed the fresh help that would have come from talks at any chance time with him. The affairs of Trinity, a society which he loved in every aspect of its old and present life, and his countless friendships and interests in the University outside the circle of physiology called to him with many voices and were always heard. After 20 years at Cambridge he came to that critical period, “nel mezzo del cammin di vita,” when men who know their powers for administration and see around them occasions that would with certainty yield good results to their controlling hands, are inclined to abandon the laboratory and its slowly moving quest for knowledge. A call to a Mastership of some College might have followed the expiry of the Tutorship, and his active interest in physiology might then have waned. But there was no such eclipse of his scientific genius. Fortune granted the conjunction in 1914 of the newly formed Medical Research Committee in London with Fletcher as its first Secretary, and his peculiar gifts thenceforward shone with the brightest power for the aid of medical science.

Fletcher was elected a Fellow of the Royal Society in the year after he had taken up his new position in London. The next twenty years until his death gave him no leisure for personal research, but in them he worked harder, and even more fruitfully, for the advance of science and all the aims to which the Royal Society is pledged, than ever he had chosen to do at Cambridge. Laboratory work alone had never given enough to satisfy his quick nature, and so came the paradox that at Cambridge, where the looms for every pattern of web in biological knowledge were all in such busy activity, he did not feel that call to devoted labour which he obeyed so unhesitatingly and so completely when, from his new post in London, he looked out over England and saw how little her people were receiving in bodily welfare from all the newly woven science. The need for a fuller use of biology in every branch of statecraft became the theme which he later urged so strongly in public addresses, when his experience with the Medical Research Council had matured to a fuller comprehension of what might be done not only at home but over the Empire, and especially in India, by a right use of medical knowledge and of the men who should be trained to further its progress. It was not the delight of the intellect in research, nor the ambition for distinguished work in a post where the world could see and reward good service, that now became his deeper and lifelong motive. The spirit innate in him, akin to that which had impelled one
of his sisters to missionary work in India, felt instinctive sympathy with the high and selfless aims in public service of Sir Robert Morant, whom he now met at Whitehall on matters concerning the official organization of the M.R.C. Between these two men, alike in big physique, in strong personality, and in culture, friendship grew strongly; and they were as one in their desire to use science as an instrument of service to the country. But both were heedful that it should be in no way blunted by common use, and Morant was as determined as Fletcher that this new Government department for the application of research to medicine should be freed from political influence and put in close alliance with the Royal Society. Its eight scientific members have, with few exceptions, been Fellows, and the Charter provides that no new appointment of a scientific member can be made except with the approval of the President of the Society. Fletcher's share in the important steps through which the Committee ultimately became detached from the Ministry of Health and received independence as a Research Council under the Privy Council has been outlined in an obituary notice in *Nature* (July 1, 1933) and need not be repeated here.

The outbreak of the war a month after Fletcher had actually begun his work as Secretary turned thought away from the new and difficult task of organizing research in civil life, and during the next four years the resources of the Committee were given almost wholly to problems of war-time. Most of the work fell on Fletcher, for decisions had often to be taken quickly in the swift current of affairs and much depended on personal acquaintance with men. The experience was of the utmost value for his later work, but in gaining it he overworked recklessly, and in the winter of 1915–16 he nearly died from an attack of pneumonia which left behind it a fibrosed area of lung, where at last flared up the sudden infection that took away his life. It is impossible to disentangle the work done during the war by the M.R.C. from that of the officers, permanent and temporary, of the R.A.M.C. Each relied on the other, but the security and success of that alliance was largely due to the trust men felt in Fletcher and to the energy and insight with which he made the fullest use of the paths so opened. For his services at this time he received in 1918 the K.B.E.

The M.R.C. had gained during the war the full confidence of the medical profession, while academic workers had from the very beginning gladly given all their aid to a body composed of men of known scientific standing. Hence the next fifteen years found the Council in the full tide of success and actively helping almost all the notable advances of medical science made in Great Britain, either directly through its own staff of full-time workers and the peculiar advantages created in its National Laboratory at Hampstead, or indirectly through various committees and men working on grants at either the universities or medical schools of the country. Each year this work was summarized by Fletcher in his general introduction to the Report of the Council.
and these accounts, written in his admirable English and with the clearest grasp of every problem presented, will be important documents for those who wish to trace the recent progress of medical research in Britain. But whoever may have been foremost in the studies of vitamins, of virus diseases, or of the use of insulin and other therapeutic substances, it was Fletcher who continually emphasized the need for the application of every discovery to the control of ill-health and who urged the measures that were taken to test or use their value. His ardour for such practical fruits at times overcoloured his judgment, but mistakes were few.

From the very first the M.R.C. had wished to further clinical research and had even proceeded so far as to consider plans for attaching a research hospital to the Lister Institute. Ultimately they were able to do what was desired more economically by employing in whole-time work physicians on the staff of existing hospitals such as that of University College. Fletcher understood and helped this new development with an insight and determination fully as keen as the interest which he felt in pure laboratory work. Never having been immersed in the daily routine of clinical medicine, it was natural for him to take a different view from those whose duty is first to care for the individual patient, and he visualized almost faster than they could be attempted clean scientific studies of the problems of human disease. So there were times when his grasp exceeded human reach and his criticisms of medical practice made him appear unduly intolerant to those in Harley Street. But of all who were struggling to gain opportunities for research he was the ideal helper; intellectually competent to grasp and criticize every plan put forward, whatever the department of research might be; eager to look forward with the worker himself to the results that might be won; and fighting vigorously to ensure that no man or piece of work of promise should be neglected. Where principles affecting freedom for scientific research seemed to him to be at stake his courage to resist was unfluctering and outspoken. “The man was like a rock” was a phrase most justly used by a colleague who wrote of Fletcher in The Times.

To a man of such mind the Royal Society was an ideal. He was proud to serve for the routine term on its Council and to help in the work of many of its committees. But far more important than this open share was his effective insistence on a policy for the Government departments, to which his duties as secretary of the M.R.C. gave him access. Langley wrote of the work by Sir Michael Foster as secretary of the Royal Society “that his influence was constantly exerted in convincing the Government that they could obtain sound information and advice on scientific questions from the Society.” Fletcher followed this lead, as he was glad to follow any act or thought of Michael Foster, but “could obtain” was soon transmuted by him to a more forceful “must obtain.”
It is related that when Fletcher was meditating his early work on the chemistry of muscular respiration he went to Foster and asked him if he thought there was anything more to be got out of chemistry for physiology. Foster rolled his beard up with both hands over his mouth and chuckled in reply. Fletcher had no such device for putting younger workers at their ease, nor did he need one; there was ever the frankest appeal to youth in his boyish smile, his quick manner, and the irrepressible sense of humour that would often break aside into a tale or even jesting mischief, for dullness was intolerable to him. It is a pleasant fancy to think how admirably his qualities would have suited him to the Society of its Royal Founder's days; enough of a Cavalier to have been a close friend of Prince Rupert, loving mankind and its gossip much as Pepys did and sharing his affection for Cambridge; and yet at the back of all this sociability and grace holding an intellect keen to criticize each new discovery and a staunch morality drawn from Puritan stock.

But Fletcher was more fortunate in his own period and its great opportunities. Even as Christopher Wren built many a smaller church as well as the cathedral of St. Paul's in the London that was reconstructed after the Fire, so did Fletcher find in these modern days, when the endowment of research is recreating medicine, many opportunities for his guidance in addition to the central work with the M.R.C. His high ardour and the steady light of his judgment made him a lodestar of benefactors. Buildings and great endowments for the medical sciences in London, Oxford, Cambridge and elsewhere were in large measure due to the confidence felt in his advice by the Rockefeller Foundation and by the Sir William Dunn Trustees. His influence in such ways was of the highest and widest value, often in a form that was publicly unknown, but sometimes more clearly evident, as when his visit to India in 1928 as head of the Indian Government Committee for the Organization of Medical Research was followed by a gift in memory of Lady Tata of £250,000 for medical research, and he was appointed chairman of the scientific committee in Europe which was to advise the trustees in Bombay. He served on many Advisory Committees to Government departments at home; and he was a member of the important Royal Commission of 1919-22 upon the reorganization of Oxford and Cambridge Universities.

In 1904 he married Mary, daughter of Charles J. Cropper, of Ellergreen, Kendal; their son recently surpassed at Cambridge the achievements of his father in his own undergraduate years by rowing in the boat against Oxford, and by winning a Senior Scholarship at Trinity together with a First Class in the Natural Science Tripos. These tokens of inherited powers gave to his father a pride that he made no attempt to conceal, and they came as a joy to him in the year of his final illness.

Fletcher himself had a splendid athletic build with tireless energy, and he kept his muscle and his quickness of movement unspoiled by the rounding
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After months of sedentary life in London he could turn to a hard day's deerstalking or shooting on the moors and be as fresh next morning as his country-living host. His conversation told at once of his pleasure in all things, animate and inanimate, for his talk was as free as his thoughts, though in moments of excitement it would often break out with a hovering stutter which was well known to his friends. These were many, and yet all felt distinguished by his friendship. One who had known him for 40 years, Dr. Montagu James, now Provost of Eton, wrote well of him as "the truest and kindest of counsellors, and magnificently keen in his appreciation of all that is best in the works of God and man." His ardent courage and his frankness made him a vigorous critic, sometimes too trenchant in his denunciation of others, and often too outspoken in giving unsought advice. But his heart held neither malice nor bitterness, and he was always eager to make amends if he hurt unwittingly. In later years his preoccupation with ever growing plans for medical science filled the field of his daily thought more obviously, and he grew impatient of any in power who did not care to use their influence for the advancement of the cause to which his own life had been devoted since Cambridge days. His work for that cause had been inspired by great motives and it had far-reaching success. But a man does not win long remembrance even for the best of administrative work. Fletcher's name will probably be kept in most enduring recollection for his research in the physiological laboratory and his life at Cambridge. That would be right, for he loved Cambridge first and foremost; and when the London work had come to its sudden end, it was at Cambridge, passing from Trinity College Chapel to Trumpington Churchyard, that he was buried, and so returned to the place which he always looked on as his home.

T. R. E.