THE HARVEIAN ORATION,

DELIVERED BEFORE

THE ROYAL COLLEGE OF PHYSICIANS,

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IN the deed by which Harvey provides for the delivery of the Harveian oration are set down three purposes which the orator was to place before him. One of these was that he should exhort the Fellows of the College to concord; the second, that he should call to mind our benefactors, and stir up others to follow their example; and, finally, that he should exhort the Fellows and Members of the College to study and search out the secrets of nature by way of experiment. As regards the first of these objects, the exhortation to concord, I have, I think, good reason for omitting it to-day—partly because I can offer no better motive to concord than that which Harvey himself offered, namely, that *concordiâ res parvæ crescunt, discordiâ magnæ dilabuntur*; but chiefly because we have so recently fulfilled in spirit another of Harvey's directions by partaking ourselves, and by inviting others like-minded, in reverence for Harvey's memory, to partake with us of a "feast of love and affection".

As regards our benefactors, I am required to mention them by name. I believe, however, that I shall sufficiently fulfil Harvey's wish if I name one benefactor who has recently aided us by the gift of a sum of money for the investigation of the nature and cure of Rabies and Hydrophobia. I do so not merely in expression of the gratitude which we all feel to Mr. Bennett-Stanford for his liberality, but in the hope that, to use Harvey's words, others may be led to imitate his good example. The investigation of so difficult a question must necessarily require the co-operation of many persons over long periods of time, but the object is well worthy of the liberality of enlightened and philanthropic men; for, although the number of sufferers from hydrophobia is small, there is none which is more formidable in its aspect; and, secondly, because there is good and reasonable ground for the expectation that the proposed inquiry, if undertaken in the right way and with sufficient means, will lead to valuable results as regards the prevention, if not the cure, of hydrophobia. If there ever were a disease to which the term "preventable" could be properly applied, it is hydrophobia. That efficient means have not before now been taken to prevent it is, no doubt, mainly attributable to the uncertainties which have existed as to its mode of origin; for in matters of prevention knowledge is power. Another reason why so little has been done may be that the number of cases of hydrophobia has hitherto been too inconsiderable to attract attention; but, now it has prevailed so extensively in Lancashire and the neighbouring counties, as well as in Somersetshire, that its elimination must be regarded as a matter of public interest.

And now let me pass on to the principal purpose of our meeting to-day, namely, that I should, to the best of my ability, exhort the Fellows and Members of the College "TO SEARCH AND STUDY OUT THE SECRETS OF NATURE BY WAY OF EXPERIMENT".

We have heard so lately from the eloquent lips of Mr. Huxley, as well as on former occasions like the present from Harveian orators more capable than myself, in what the merits of Harvey as a discoverer consisted, that I may venture to take for granted that all my hearers are already conversant with his life and work, and am the more willing to do so that there seems no reason to believe that Harvey intended that his own achievements should form any part of the subject of the oration which he instituted. What we have to-day to consider is, not what he did, but how to follow his example. It is as our example, therefore, that I shall speak of him. For he deserves our gratitude, even more than for the discovery itself, for this: that, in his investigation of the circular motion of the blood, he gave his successors a model of the method by which all discoveries in physiology and in pathology have been made since. The essence of that method is expressed by him in the one word—experiment.

In these memorable words of Harvey, Nature is presented to us, not as an arrangement of objects in a museum for our contemplation, but under the figure of a living personality, of whose unceasing activity we and all other creatures are the expressions, and whose will is opposed to our efforts. Nature has secrets which she desires to conceal from

us; her whole proceedings, indeed, are so recondite that the naturalist who contents himself with the mere contemplative study of her external aspects is, unless he wisely abstain from all attempts to interpret what he sees, more likely to fall into mistakes than to arrive at just conclusions—a fact which was expressed by Hippocrates in his often misquoted words *experientia fallax*. The idea which is expressed in the words of the injunction is that of struggle, of striving for the mastery. Nature on her part is at work on all sides of us, and desires to conceal from us her operations. We, on our parts, by virtue of the faculties with which she has herself endowed us, have the power, and along with the power the right and the duty to wring from her her secrets, as it were by force. Unquestionably there never was a more splendid exemplification of this struggle with Nature, than is afforded in the history Harvey has himself given us of the discovery of the circulation.

Actuated by that insatiable curiosity, that lust for knowledge of the unknown, which is the essential characteristic of the naturalist, not being satisfied with what he could learn of the motions of the heart and arteries by contemplation of the external phenomena of the circulation, or with any conjectures which he could draw from his very complete anatomical knowledge of the structure of the heart and blood-vessels, he had the boldness to open the chest of a living animal, to inspect the motions of the living heart, to feel with his own hands how it hardened in contraction and grew soft in diastole; to observe what happened when, by ligaturing the aorta, he prevented it from discharging its contents, or when, by similarly occluding its afferent veins, he deprived it of its supply of blood; his purpose in all these experiments being, not to verify a preconceived inference, but simply to strip nature of her concealments, and to force her, thus exposed to the light of day, to reveal her own secrets in language so plain, that it needed no interpretation, so convincing that, to those who believed the evidence of their senses, it required no enforcement by argument.

And here let me notice that Harvey has been represented by many of his most ardent admirers as much more of a reasoner and much less of an observer than he really was, and that some have been thus led to a mistaken view of his method. The discovery of the circulation may, in a certain sense, be truly described as "one of the most admirable and useful inferences ever given to the world"; but no one who had realised the nature of the moral and physical difficulties which Harvey had to overcome would so characterise it.

As a true interpreter of phenomena which his predecessors had seen but misunderstood, Harvey is worthy of all honour, nor will anyone who has attentively studied his writings, particularly the treatise *De Motu Cordis*, be disposed to underrate his power in this respect. But the essence of his achievement was, that he saw and felt for himself the motions of the heart and arteries, and displayed and demonstrated them to others in such a way that their meaning could not be misunderstood, nor their truth disputed. It was the certainty of the facts and the simplicity of the language they spoke that constituted the greatness of the discovery.

In the present lecture, I propose to illustrate, by reference to the history of some of the investigations which have served as the foundation of so much of our present knowledge in relation to the circular motion of the blood as was concealed from Harvey, the methods by which now, as in Harvey's day, all natural knowledge is acquired, and then to offer practical suggestions for the guidance of those who, being like minded with Harvey, desire to fulfil its injunctions and to follow his example.

The shortness of the time at my disposal compels me to limit myself to one subject; namely, to the researches by which, in the first half of the present century, the Harveian doctrine of the working of the circulatory apparatus was developed. I had intended to give some account of the still more important discoveries of the last thirty years, but have found it impossible. It is satisfactory to reflect that England, which gave birth to the author of the doctrine of the circulation, was also the country of the two men (living at intervals of a century from each other, and the first of them a century after Harvey) who, in great measure, completed it. Stephen Hales, a Doctor of Divinity and a beneficed clergyman of the English Church, was an active member of the Royal Society when Sir Isaac Newton was President, a contemporary of Locke, of Cheselden, and of the anatomist Cowper. Hales was a genuine naturalist. His writings afford evidence that he was a firm believer in the religion of which he was the minister; but in matters of science he was a sceptic, accepting nothing short of "the united testimony of many good and credible experiments" as the foundation of knowledge. He was no less distinguished for his physical than for his biological experiments, most of which were undertaken for useful purposes (as, for example, those relating to the distillation of sea-water for ships, and those on the preparation of salt provision); but others, in the conviction that "the obtaining a better insight into

the nature of plants or animals would improve the arts of medicine and agriculture."

The purpose of Dr. Hales's experiments was to measure the force and velocity of the circulating blood, and to obtain thereby data, as he expresses it, "on which to found just calculations". In planning his experiments, he was guided by as clear a comprehension of the relation between physical laws and vital phenomena as he could have possessed had he lived a century later. Inasmuch, he says, as "the animal fluids move by the same hydraulic and hydrostatic laws as those which prevail outside of the body", they must be investigated by the same methods of mensuration. Hence, if we compare his work with the best researches of the present epoch, the difference is found to consist only in the comparative incompleteness of his appliances; for his experiments on animals, although made by what we should call rough methods, were yet right in principle, were carried out with great care, and were frequently repeated under varied conditions. Hence his results, although they have been vastly amplified, have needed no correction. In this way, Hales acquired a title to the highest commendation that can be bestowed on a naturalist; he made no mistakes of observation; but it was also true that he made few errors of inference; for, while recognising that "from conjecture fresh discoveries take their rise", he was yet content "to imitate children who, for want of better skill and abilities, and of more proper materials, amuse themselves with slight buildings".* In point of fact, he did not extend his inferences so far as he might have done, leaving it, as we shall see, to another to give the full interpretation of his observations.

Dr. Hales had no immediate followers, nor did either of the three great physiologists of the eighteenth century—Haller, Spallanzani, and John Hunter—add materially to the knowledge which Hales gained by experiment as to the motion of the blood and its causes. As regards Hunter, indeed, it must be admitted that the doctrine of the "action of the arteries", to which he was led by his pathological researches, was a step in the wrong direction, though it might be shown that it contained the forecast of a great truth.

The next step was accomplished eighty years after Hales's discoveries had been communicated to the Royal Society, by a man who, though a distinguished Fellow of this College and an eminent physician, was a much greater mathematician and physicist. Dr. Thomas Young, to whose transcendent merits in the more exact department of natural science full justice has been done by his successors in the same lines of inquiry, was as far in advance of the other men of his time in physiology as he was in natural philosophy. That he was so must be attributed principally to his extraordinary intellectual powers—powers which, even in early youth, made him a prodigy of erudition in spite of great disadvantages of education; so that, at the age of eighteen, he had acquired such proficiency in the classical languages that, as Dr. Peacock says, he was already prepared, on questions of Greek criticism, to enter the lists with the most distinguished scholars of his time. That even at this early period his attainments in mathematics and natural philosophy must have been equally remarkable, we have evidence in the facts that he was only twenty when he offered to the Royal Society the paper (on the Mechanism of Accommodation) which procured him the honour of its Fellowship; and that he was only twenty-seven years old when he made the first of that series of investigations relating to the interference of sound and of light which have placed him, to quote from Sir John Herschel, in the "highest rank of scientific immortality". The same year that he became Fellow of the Royal Society, he entered as a student of St. Bartholomew's Hospital. Afterwards he went to Edinburgh, and thence to Göttingen, whence, like Harvey, he returned in due time to graduate in medicine at Cambridge.

In order that we may be in a position to discuss Dr. Young's researches as to the mechanism of the circulation, let me define the limits of the existing knowledge of the subject at the time when that he undertook them. As regards the fundamental facts of the circulation—viz., that every particle of blood which leaves the heart by the aorta comes back to its starting-point—Harvey's knowledge was, I need not say here, as nearly as possible complete, the only gap being that he was unacquainted with the nature of the channels by which the smaller arteries discharge themselves into the veins—a blank soon to be filled up by Malpighi. The gap was a very trifling one indeed; but Harvey deliberately avoided supplying the missing link by conjecture or inference, leaving the question of the channels of communication between arteries and veins entirely open. As regards the cause of the motion of the blood, Harvey's knowledge was also wonderfully complete; but there was one deficiency. There is evidence in his works that he knew much more clearly than many subsequent writers that the force

by which the blood is impelled along the arteries is a muscular one, and that the heart in its systole grasps and squeezes out its contents. All this is perfectly clear; but it is no less certain that he did not know what was the cause of the venous blood-stream. The explanation which he gives for the return of the blood from the periphery to the centre is very much this: the blood spontaneously inclines from the circumference to the centre, just as drops of water spilt upon a table concentrate to the mass of liquid. Consequently, although the progress of the blood "from the capillary veins to the larger trunks is aided by the compression of the muscles", it has an innate tendency of its own towards the heart, which, when it reaches, it becomes attenuated by its increasing heat so as to dilate the right auricle. It would be, I think, a mistake to allow our admiration for his intellectual character to blind us to the fact that in these passages* Harvey yielded to a tendency against which I do not think we have any need to guard ourselves at the present moment—a too great reverence for antiquity. The doctrines of diastole by innate heat or fermentation, and that of the innate tendency of the blood to return from the periphery to the heart, cannot be regarded as expressing inferences which he had himself made. But they are of interest, as proofs that he did not clearly understand what was the real cause of the venous blood-stream.

The proof that the flow of blood throughout the whole vascular system is dependent on one and the same cause as the arterial blood-stream—viz., on the difference of pressure between the arterial and venous ends of the system of tubes which constitutes the vascular apparatus—was really contained in the experiments of Hales; but it required the application of the trained mind of Dr. Young, a mind trained in the image of Newton, to expound and develop it. The principle of Hales, that the animal fluids move by hydrostatic laws, which, *mutatis mutandis*, we now accept in its applicability not only to the phenomena of the circulation, but to every other function of the animal body, was enunciated by Dr. Young in the opening paragraph of his Croonian Lecture,† in the following memorable words: "The mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye. . . . As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connection between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body; and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of those powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics."

Dr. Young had already communicated to the Royal Society a remarkable series of physical researches as to the motion of liquids in rigid and elastic tubes—researches which still constitute the basis of our knowledge on that subject. The purpose of the Croonian Lecture was to combine the results of his own investigations of the motion of liquids in elastic tubes with the measurements of Hales as to the arterial pressure and the velocity of the blood-stream in living animals; and on this double foundation of physiological and physical experiment to construct a true doctrine of the motion of the blood in the vascular system. In this purpose he succeeded. The most important result of his investigation being to show that everywhere, whether in arteries, capillaries, or veins, the blood moves forward with a velocity which is directly proportional to the difference of pressure, and inversely to the resistance to be overcome; and, secondly, that inasmuch as the pressure in the smallest arteries is less by only a fraction than the pressure in the aorta itself, the force which is still available at the commencement of the capillary circulation is nearly equal to the total force which is engaged in carrying on the circulation. It thus became evident that the difficulty which Harvey had had in understanding how it is that the blood returns from the peripheral parts to the heart (a difficulty which no physiologist, not even Hales, since his time had explained) arose merely from the circumstance that the laws which regulate the motions of liquids in tubular channels had not been investigated, and therefore could not be applied.

With reference to these inquiries of Dr. Young, there are two things worthy of notice; namely: first, that, being content with the data furnished by the measurements of Hales, he made no physiological experi-

* See Harvey's *Works* (Sydenham Society's edition), pp. 70 and 133.

† The Croonian Lecture "On the Functions of the Heart and Arteries", by Thomas Young, M.D. (*Philosophical Transactions*, 1809, pt. i).

* Preface to Hales's *Hamastatics*, page 6.

ments himself; and secondly, that although his exposition of the theory of the motion of the blood was perfectly clear, and—supposing the data to be correct—absolutely conclusive, it had little or no influence on the teaching of physiology at the time; the general doctrine being that the motion of the blood was not regulated by the same laws as the motions of inanimate liquids, and, in particular, that the blood-stream in the capillary blood-vessels was aided either by what was called the *vis insita* of the blood itself—a mysterious tendency on the part of the blood to move onwards (as Kaltenbrunner supposed)—or to an equally mysterious vital attraction between the walls of the capillaries and their contents, the doctrine first advanced by Bichat. Possibly, if it had not happened that Young was engrossed in the prosecution of those more brilliant investigations which have made his name famous, it might have been otherwise. The reason of this want of influence is to be sought chiefly in the fact that the time for the development of physiology as a science dependent on measurement—that is, as a branch of physics or natural philosophy—had not yet come; for although, as regards the particular case of the circulation, the facts had been observed with sufficient exactitude and completeness to admit of the application to them of theoretical treatment, yet in almost every other direction this was rendered impossible from the absence of physiological data, and the insufficiency of the means which then existed for obtaining them.

It must not, however, be supposed that Young's work was lost, though at the time it appeared so fruitless. Just as his two most important discoveries in optics and acoustics have, during the last thirty years, been developed by Helmholtz, so in like manner his work regarding the motion of the blood has been continued and completed by the veteran physiologist, or rather physiologist and anatomist all in one, whose loss science has had to deplore during the current year.

Ernest Heinrich Weber, who was in many respects a man to be placed side by side with Young, entered on the study of medicine the very year that Young read his famous Croonian lecture. He began his career as a comparative anatomist, and had already achieved so great a reputation as to have become professor in the University of Leipzig, when he undertook those physical experiments relating to wave-motion, by which perhaps he is better known than by his anatomical discoveries. It seems that he was guided thereto partly by accident, for we are told that he was first led to the investigation of wave motion by the observation of the surface-undulations of the mercury which he used for his anatomical injections. But here, as in other instances, it was the character and training of the mind that received it that gave to the suggestion its value. Weber was a mathematician and physicist, and had already worked with Chladni at his well known researches on sound. The *Willenlehre* appeared in 1825, and was soon followed by a dissertation in Latin on the arterial pulse. In this dissertation he showed, by the same methods which Young had applied to the progressive motion of the blood, namely, by combining physical with physiological experiments, the complete identity of the laws which regulate arterial pulsations with those which govern the propagation of pulsations in elastic tubes. The results of Weber's researches on the physics of the circulation were finally, in 1851, embodied in a treatise in which he established the science of hæmodynamics on the solid basis of exact measurement.

And now we have arrived at the middle of the present century, the threshold of a new epoch, differing from that of Harvey in consisting not of one discovery but of a rapid succession of them, agreeing with it, however, in deriving its importance quite as much from the complete revolution in the methods of investigation which marked it, and in the reconstruction of the science which has resulted from that change, as from the discoveries themselves.

We have already seen that Harvey's method was to strip Nature of her concealments, and thus force her, as it were, to reveal her own secrets. During the next century after Harvey, the same simple method of exploration was followed by Haller and Spallanzani; and in the present century by Magendie, all of whom were distinguished by the same wise scepticism, the same love of objective truth, that enabled Harvey to conquer the spirit of his time, even in his own person. But just as it happens in agriculture, that the rougher methods of tillage, which at first are found to be sufficient to produce abundant crops from the virgin soil, become later unavailing, and have to give place to others more systematic and refined; or, as in warfare, every improvement in the methods of defence necessitates a corresponding improvement in those of attack, so it has been in scientific investigation, and particularly in physiology.

By the time that Magendie wrote his celebrated *Compendium*, almost all that could be arrived at by the rougher modes of observation—the method which consisted in removing the veil from the hidden motions of the organism and exposing them to the scrutiny of the unaided

senses—had been already attained, and it was necessary to have recourse to more elaborate and more systematised methods of attack. But for this a leader was needful,—a generaliser even more than an explorer. Such an one soon presented himself in the person of Johannes Müller, a man of similar power to Young, with this important difference, that he was a consummate anatomist and morphologist, and combined therewith so considerable an acquaintance with the physics and chemistry of his time that he was able to grasp the whole range of physiological knowledge as completely as Young did the few subjects to which he exclusively directed his attention. As a mere investigator, he illuminated many obscure regions, as, for example, those relating to the specific endowments of nerves, and to the mechanism of voice and hearing; but it was not merely as a searcher, in the Harveian sense, that he has the chief claim to consideration. The great service that he rendered to physiology was that by the universality of his knowledge and by his power of separating the true from the false in observation and experience, he was able to bring together all that was known as to the chemical and mechanical processes of the body into a system, and thus to lay the foundation on which his pupils—Helmholtz, Brücke, Ludwig, Du Bois-Reymond, Max Schultze, Virchow—and their followers, during the last thirty years, have reared that newly constructed branch of natural science which we now understand by the word physiology.

The necessity for this reconstruction of physiology as a new science, on a new foundation, with new materials, became evident as soon as physics and chemistry were sufficiently advanced to furnish exact methods for the investigation, not merely of such comparatively simple questions as the mechanism of the circulation, but also of the more complicated ones relating to muscular contraction, to the transmission of molecular changes in nerves, to the chemical process of respiration. Thus, in the case of the last-mentioned function, the moment that it was seen that the actual relations of oxygen and carbonic acid in the tissues were entirely inconsistent with notions founded on previous observations—which though correctly made were made with insufficient methods—it became evident that the whole of the facts must be re-examined, whether actually proved to be incorrect or not, by comparison with standards of absolute certainty. For similar reasons, the experimental results of Magnus on the chemistry of respiration, of Matteucci on animal electricity, of Young on the mechanism of accommodation, and many others, have been compelled to give way to new ones, not because these great men made mistakes, but simply because other men, perhaps not so great as they, equally conscientious and trustworthy, and furnished with better appliances, have gone over the same ground and have obtained data which are not only more complete and comprehensive, but possess, so far as they are based upon measurement, an element of permanency which was wanting in those they have supplanted. Thus the modern physiologist aims at presenting, not his perception of a form or of a motion, but the result of his comparison of that form or motion with an invariable standard; and it is this which mainly constitutes the difference between his method and that of his predecessors. The process of investigation consists now as ever in searching out the secrets of Nature by experiment, but the experiments of modern physiology are much more difficult, require for their performance more previous training, quite as much devotion and self-denial on the part of those who engage in them, more combination of individuals of different capacities for the attainment of one result, and consequently more money and more time.

Nothing could be more interesting, had I the ability or the time to do it, than to trace in broad outline the progress of the great revolution of which I have endeavoured to indicate the nature.

But time warns me that the most important duty of the day still remains unfulfilled; namely, that we should endeavour (partially and incompletely it must necessarily be) to consider how best to follow Harvey's example. In all that I shall say on this subject, I must ask to be understood to address myself to those who are in the enviable position of being the youngest members of the College. For us, if we have not already learnt the lesson conveyed to us in our motto, it is too late to learn it now. But for those who have still the best of their lives before them the choice is open.

If research is to be done at all, it must be begun in the morning of life. One reason of this is the internal condition of intellectual fitness; the other the external one of leisure. Of the first I need say little. It can scarcely be questioned that it is in early maturity that the mind is most capable of the continuous struggle of which, as we have seen, research consists. For those qualities of mind which specially belong to the naturalist—the lust of knowledge, the love of truth, and the joy of successful effort—are apt to lose their power over us as life advances. As a rule, therefore, we should begin to work early, and work while it is day, for the night cometh, when no man can work.

As regards leisure, it is, I believe, the result of general experience, that a man who aims at the high professional position occupied by the Fellows of this College may reckon on ten or twelve years which are, for the most part, at his own disposal. How may these years be used as Harvey would have used them?

Harvey began his active career as a physician in 1609, and probably very shortly afterwards became engaged in lucrative practice. He left Cambridge, where he was educated, in 1597, spending the next six years of his life at Padua, and returning to the university to graduate as Doctor of Medicine in 1603. If, therefore, we reckon his mature life to have commenced from his bachelor's degree at Cambridge, he enjoyed at most eight years of freedom from business. Of these eight years, the first five were spent at Padua; and these, in relation to Harvey's development as a discoverer, must be regarded, with reference to their future influence, as among the most important of his life. For during his residence at Padua he was under the influence of the man whom he calls "foremost among the moderns", and places side by side with Aristotle. As to his mode of student-life in Italy we know nothing; but we may be sure that he worked very earnestly under the guidance of Fabricius, particularly at those anatomical researches relating to the formation of the chick which were afterwards to be the point of departure in his inquiries respecting generation. Had Harvey not gone to Padua, he might have been a favoured courtier, a successful physician, an influential citizen—all, in fact, that he actually became—but not the discoverer of the circular motion of the blood.

If we now compare Harvey's use of his leisure with that which is possible to the modern physician, we shall find contrasts which may suggest useful considerations. And, first of all, it is to be noticed that the conditions of modern professional life in London have deprived these first decades of much of their value; for in London, long before a man becomes occupied with patients, he ceases to be master of his time. I need scarcely say that I do not refer to the social engagements of London life, but to occupations of a more serious and less agreeable character—to the wearisome labour of the out-patient room, to the unattractive duty of systematically teaching some subject in which the teacher has no special interest, or, finally, to the literary labours which most men find it necessary to impose upon themselves—engagements which are all of such a nature as to render impossible that earnest and continuous devotion of time and energy to real study which is the first and preliminary condition of success in investigation.

Another contrast between the conditions under which Harvey lived and worked and those of the present moment relates more particularly to his life at Padua before his medical graduation. It is scarcely possible to imagine intellectual conditions more entirely different than the free converse which Harvey enjoyed with such a man as Fabricius, and the strictly regulated studies of an aspirant to the doctorate of the University of London; and out of the comparison arises the question, Which of the two is preferable? The advantage is not so entirely on the side of the modern method of training as it might at first sight appear; for, although it may be in some respects more useful that a man should be conversant with all branches of medical science than that he should be master of one or two, it is not the less certain that for the purpose which we have to-day in view—that of fitting a man for the searching out the secrets of Nature by experiment—that sort of encyclopædic knowledge which he acquires in order to prepare himself for the examiner is of little or no value.

In order to form a just conclusion on this subject, we cannot do better than follow the principle laid down by Harvey in his introductory essay *On the Manner and Order of acquiring Knowledge*, in which he says that no one can properly become a student of any branch of natural science until he is able to rely on his own observations and not on the observations of others. Now it is evident that this does not apply to the mere disciple in natural knowledge, who, so long as the *status pupillaris* lasts, must rely on the experience and knowledge of others. It begins to apply from the moment that the transition takes place from discipleship to studentship; and what we have to guard against in the interests of science—which I regard as identical with those of real study—is, that this transition, this liberation of the student, should not be too long deferred. Harvey was freed from academical restrictions at the age of twenty, when he went to Padua; Young left Edinburgh for Göttingen at the age of twenty-one. In London, a man graduates as Bachelor of Medicine at soonest at twenty three. But, unfortunately, he does not yet enjoy that intellectual freedom which is one of the essential conditions for genuine and productive intellectual works. Does any one doubt that the undue prolongation of the bondage of examinations is a serious evil? Is it too much to hope that the University of London, to whom we owe the establishment of the important principle that scientific training is a necessary preparation for the study of medicine, will some day recognise that she

has still higher functions to perform, and that she has it in her power by bestowing her most valued rewards, not on proficients in the examination room, but on students in the Harveian sense; that is to say, on men distinguished for attainments in some branch of medical research, to do a far greater work than she has yet done for the advancement of medicine?

Having thus endeavoured to illustrate by antithesis the external conditions which I regard as essential to the development of a student after Harvey's model, namely, that he should enjoy sufficiently early in life leisure for real study, and perfect freedom in the choice of subjects and methods, allow me to say a word as to what I believe to be one of the best means of using it. Up to a comparatively late period, it was a common practice for men who aspired to the higher ranks of the medical profession to go abroad at the close of the curriculum. Now, partly owing to the external conditions to which I have already alluded, that is, to the absorption of time and intellectual energy in the work of preparing to meet the examiner, and partly, no doubt, to the improvement of the means of study which offer themselves in London and elsewhere in this country, the custom has been in great measure discontinued. I confess that it seems to me that in this respect our predecessors were more judicious than we are ourselves. If a man's whole object be to furnish himself with the skill and knowledge needful for the efficient exercise of his profession, the schools of London will afford him everything that he can require for the purpose. But it is not about technical training that I am speaking, but research; and, if that be his object, a man cannot do better than take Harvey as his example in this as in more important matters.

Harvey went to Padua, not so much to learn the medical or surgical art, but to study anatomy, the only representative at that time of medical science. He who desires to prosecute those branches of research on which medicine is now founded, can find quite as solid reasons for visiting the schools of Germany now, as Harvey had for going to Italy. What those inducements are, can be stated in few words. It is not merely because the imitation of Harvey is denounced by public opinion and impeded by legislation, that it is desirable that he should cross the Channel, but rather that during the last twenty or thirty years we in England have, like the foolish virgins, been quietly allowing our lamps to go out. While we have been contenting ourselves with putting up statues to Harvey, dining in his honour, recounting his great achievements, we have handed over to others the doing of his work. Until, therefore, England again takes the place that formerly belonged to her in physiological research, those who desire to devote some part of their life to searching and studying out the secrets of nature, will act wisely in so arranging their plans of study, that some years may be spent in the schools of other countries, where, along with leisure for work, they will derive advantages of the same kind with those that Harvey did from his residence at Padua—the co-operation and guidance of the very men to whom the sciences they profess owe their present development.

And let me add that all that has been said as to the study which Harvey had directly in view in the injunction which has served as the text for this address, applies with equal, if not greater, force to those branches of physiology which are more directly connected with the art of medicine.

Germany, recognising that the progress of the art of healing depends as certainly, though not so directly, on science as the art of war, has erected during the last ten years in all the great centres of medical learning specially endowed institutions or schools for the experimental study of those two sciences, which together make up the one science of medicine: namely, Pathology, the knowledge of the cause and nature of diseases; and Pharmacology, the knowledge of the mode of action of remedies.

How does it happen that we in England, who boast of being the countrymen of Bacon, of Harvey, of John Hunter, and of Charles Bell, and who believe that we know better than others the practical value of knowledge, whose material resources surpass those of every other nation, and who in certain favoured directions are willing to spend public money for purposes supposed to be scientific with lavish prodigality, should make no provision whatever, either in our universities or in our magnificently endowed schools of medicine, for the prosecution of experimental research either in pathology or in therapeutics? The question admits of but one answer, namely, that the time has not yet come. Sooner or later—perhaps twenty years hence—it will certainly be recognised that the scientific investigation of the essential nature and origin of disease and of the mode of action of remedies is a matter of no less importance to the public welfare than the application of the sciences of physics and chemistry to the invention of new methods of destroying human life.