

## A RIDDLE OF THE SEVENTEENTH CENTURY THE LINACRE LECTURE\*

BY

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It is not necessary to enlarge to the Master and Fellows of this College on the emotions engendered by an invitation to give the Linacre Lecture. The pleasure of being selected is immediately counteracted by the grave responsibility involved, and when one considers the list of previous holders of this office it is not possible to avoid acute anxiety as to whether one will be able to justify the choice. In this particular year even greater responsibility is imposed on the lecturer in view of the fact that Linacre was probably born just 500 years ago.

I am not unaware that others who have preceded me have prefaced their lectures with a brief portrait of some aspect of the life and work of the man whose memory is enshrined in them, and I could perhaps follow the same pattern with more reason in view of the special significance of this occasion. Furthermore, I have the honour to be Harveian Librarian of that Royal College for whose foundation Thomas Linacre was so largely responsible and towards the establishment of whose library he gave so many of his own books, all of which unhappily perished in the Great Fire of 1666. A similar fate also befell almost all the books from the library of William Harvey.

There is a striking similarity between Linacre and Harvey in some respects; both were benefactors of the College of Physicians; both instituted lectures, though with slightly different intent; both were associated with Canterbury and Cambridge; and both acquired medical degrees at Padua. In fact, Linacre has been called "the intellectual grandfather of Harvey" (Payne, 1897).

### The Terms of the Lecture

It is interesting to note that, whereas successive Harveian Orators have been unanimous in their praise of William Harvey, Linacre lecturers have varied from praise, damning with faint praise, and even passing harsh judgment. But by what standards are we to assess the achievements of Linacre? Surely by taking note of the times in which he lived and not by appearing to be wise after those events. When he set up his two series of lectureships at Oxford and Cambridge it is not certain what end he had in view. It has been suggested that his act was simply that of a wise old man to encourage the study and teaching of medicine (Osler, 1908). In the original deed, however, so far as the Cambridge lectureship is concerned, no provisions were made for the subject of the lectures. Some 50 years passed before it was laid down that the lecturer was to expound Galen's *De sanitate tuenda* and *Methodus medendi* as translated by Linacre. He was to be at least a Master of Arts who had studied Aristotle and Galen, and, while he was in office (it was a four-year period), he was not allowed to practise medicine. These provisions operated until well on into the nineteenth century (Rolleston, 1932).

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When Linacre returned from Italy about the end of the fifteenth century, no control was exercised over the medical profession in this country. In 1511 an attempt at such control was made in an Act which forbade anyone to practise as a physician and surgeon unless he had been examined by the Bishop of London or the Dean of St. Paul's (beyond London, the bishops or their vicars-general). In the examination of candidates they were to have the assistance of four doctors of physic and for surgery other expert persons in that faculty. How effective these powers were may be open to doubt, since seven years later, on the instigation of Thomas Linacre, the Royal College of Physicians of London was incorporated and given power to proceed against any person practising medicine in London and within seven miles without the Licence of the College. This power was confirmed and extended in 1522, when the College was given authority to examine and license all physicians in England except graduates in medicine at Oxford and Cambridge, over whom they had no control as long as they kept out of London. Surviving records contain nothing to suggest that in making provision for the control of the profession Linacre also envisaged any scheme of medical education.

I have already referred to the syllabus of the Linacre lectures, which continued without revision for several centuries. This stagnation is reflected in medical teaching in the universities. So far as Cambridge was concerned, from the birth of the university until the nineteenth century medical teaching consisted in the reading and expounding of Hippocrates, Galen, and Aretaeus, and was devoid of the experimental method in which Harvey could have led the way (Rolleston, 1932). University education was theoretical rather than practical. At Oxford the situation was much the same. The obligation to take an M.A. degree before a degree in physic meant studying for fourteen years. By the eighteenth century no lectures were delivered; no attempt was made to enforce medical study; and degrees were granted without any test. The only real medical education was that to be obtained on the Continent at Padua or later Leyden, for example.

### The Importance of Padua

Linacre's achievement in translating into Latin the works of Galen has been criticized as labour spent "in putting a brighter polish on the fetters that held medicine in thrall" (Topley, 1940). The contrary view, and one that is no less justified, was expressed by Professor Charles Singer in his Lloyd Roberts Lecture, though Linacre is not mentioned by name: "Among the welter of inter-related activities that characterized the period [the Renaissance], there were four that made a special impact on anatomy. Between them they determined its course and deflected it from being a mere rebirth of the ancient discipline." One of those factors was the publication of the ancient anatomical texts from which

the new anatomy could take its start. By the new anatomy is meant the work of Vesalius and his successors. The place primarily associated with this is Padua, and it was to Padua that another physician prominent in the annals of the Royal College of Physicians went—namely, John Caius. It is generally held that at Padua he came under the influence of Vesalius, whose *De Fabrica* was published in 1543. On his return to this country from Padua, Caius made what some have considered his greatest contribution to the course of scientific progress in medicine. I refer to the lectures and demonstrations in anatomy he instituted at the Barber-Surgeons' Hall about 1546: it was there rather than at the College of Physicians, because the latter had no facilities for obtaining dead bodies for dissection. This deficiency was remedied in 1564 by the granting of the Charter of Anatomies, probably at the instigation of John Caius; anatomical lectures were established soon after, and an anatomical theatre built some years later.

It was to the refounded Caius College that William Harvey went, but this was not until about ten years after the foundation of the new series of Lumleian Lectures (1582), with which his name will be for ever linked. These lectures were originally offered to the Barber-Surgeons, but for some reason were declined. The original syllabus was an ambitious one extending over six years. The first lecturer was Dr. Forster, who discharged his duties in such a manner that "each one present inducted with judgement, conceived such hope of the doctor, touching the performance of all actions incident unto him by that place, as some of them continued his auditors in all weathers, and still hold out" (Holinshed, 1808). The fact that the appointed lecturer was expected to spend two years travelling in France, Germany, and Italy "to the intent that he may hear the best and most learned and expert surgeons read and see their practices; whereby he may be the better able to execute the said place of reading in the said College of Physicians in London at his return into England" (Goodall, c. 1680-1700) showed that the intention of the founders was sound enough and that they realized that anatomical and surgical knowledge on the Continent was far in advance of that in England.

While Dr. Forster was reading his lectures, William Harvey was already, by his studies at Cambridge and later at Padua, fitting himself to become Fellow of the College (1607) in line to succeed to the Lumleian Lectureship in 1615. Robert Forster had been succeeded first by William Dunn and then by Thomas Davies, who died in August, 1615. It was some time during this series that Harvey demonstrated the circulation of the blood and committed the results of his experiments to print in 1628. It is, perhaps, not untrue, therefore, to claim that Linacre, through Caius, was the intellectual grandfather of William Harvey.

#### "De Motu Cordis"

At the time of Linacre's life and active period medicine was dominated entirely by the Galenical tradition. The whole of pathology and origin of disease was described and attributed to variations of the four humours. This system has been so thoroughly described that it is not necessary for me to go into it here. The whole of medical treatment was based on this principle, and of course the practitioners had no knowledge of basic physiology as evinced particularly by the circula-

tion. Many writers and teachers have an entirely erroneous view of the growth of knowledge in the seventeenth century. To students it is frequently indicated that prior to the time of Harvey all was utter chaos, and that after Harvey all was scientifically straightforward. The publication of *De motu cordis* in 1628 provided, for those who read it, a logical account of the circulation without which no system of physiology, pathology, or therapeutics could survive. An impression exists in many students that immediately after the publication of *De motu cordis* a complete revolution took place and that modern medicine was actually born from that date. In my capacity as Harveian Librarian at the Royal College of Physicians for the last ten years I have made a very special study of the writings of eminent physicians from the Harvey period up to the end of the eighteenth and nineteenth centuries, and it is my purpose to present to you in the course of this lecture a problem to which I myself have found no adequate answer. My problem is why did these brilliantly clever men who followed Harvey fail to apply the lessons of *De motu cordis* to their own practice of medicine? We might even go farther and try to inquire what effect Harvey's researches had on his own practice of medicine. Let us begin by glancing at the famous book *De motu cordis*. This appeared in 1628, and was published at Frankfurt, where the annual Book Fair would ensure wide publicity. It was not known exactly how many copies were printed, but possibly the number was 500-1,000. To-day, some 50 examples of the first edition are known. Written in Latin, the book is certainly not easy to read, but fortunately the student of to-day is provided with the excellent translation of Professor Kenneth Franklin. This sets out in very great clarity the striking way in which Harvey drove home the points of the circulation. To those who are not familiar with the work I would like to quote one or two passages from Franklin's translation:

"But there are also very many points about the arteries which similarly illustrate and bear out the truth of my contention. For instance, Why does the vein-like artery not pulsate, though it is numbered among the arteries? or, Why is a pulsation felt in the artery-like vein? The answer to both questions is that the inthrust of blood into the arteries is the cause of their pulsation (and such inthrust occurs into the latter vessel, but not into the former one). Again, one is asked, Why do the arteries differ so much from the veins in the thickness and strength of their walls? To this the reply is that it is the arteries which bear the brunt of the heart's vigorous outthrust and of the blood's violent inflow. Hence, as perfect Nature makes nothing in vain and is sufficient in all respects, the nearer the arteries are to the heart the more they differ in structure from the veins, and the stronger and more ligamentous they are than these. On the other hand, in their most distant disseminations, such as those in the hand, foot, brain, mesentery and the spermatic ones, the two sets of vessels are so similar in structure that it is difficult, from a visual inspection of their coats, to distinguish one from the other. This is, however, justly so, for the farther the arteries are from the heart, the much smaller is the force with which they are struck by the cardiac impulse, weakened as it is by the great distance which it has travelled. Further, though the impulse in question must have been adequate, for the blood in all the arterial trunks and their branches, it is reduced by some fraction at each division so that the ultimate hair-like arterial branches seem to be veins not only in structure but also in function. For their perceptible

pulsation is either nil or intermittent, and even in the latter case occurs only when the heart beats with unusual violence, or an arteriole is dilated or over-widely open in some small section. It is on this account that we are able, at some times but not at all times, to feel a pulsation in the teeth and in swellings, and in the fingers. Children always have quick and rapid pulses, hence it is only through the sign which I have mentioned that I have seen for certain that they were labouring under fever. Similarly in tender and delicate subjects I have readily been able, by pressing on the fingers, to ascertain from the digital pulse the time of a febrile attack. On the other hand, when the heart beats over-languidly, it is not only in the fingers but also in the wrist or temples that I have failed to detect a pulse; this I have experienced in cases of fainting, of onset of hysterical symptoms, and of asphyxia, also in over-weak subjects and in those about to die" (Harvey, 1958, pp. 108-9).

Another passage almost at the end of the work indicates quite clearly Harvey's grasp of the situation.

"A related question is, Why in making an anatomical dissection do we find the vein-like artery and the left ventricle so very full of blood which is identical with that filling the right ventricle and the artery-like vein and, like it, dark in colour and tending to clot? The reply is that the blood passes through the lungs from the latter site to the former one. Two final queries are, first, Why does the artery-like vein, as it is commonly called, have the structure of an artery and the vein-like artery that of a vein? The answer is that, contrary to general belief, the former is in truth an artery, and the latter a vein, both functionally and structurally and indeed in every respect. The second query is, Why has the artery-like vein so wide an opening? and the answer to it is, Because it carries much more than is necessary for the nutrition of the lungs" (ibid., p. 111).

These two brief quotations indicate quite clearly that a detailed and striking description was given of the systemic and of the pulmonary circulation. It must of course be remembered that the capillary connexion between arteries and veins had not yet been discovered owing to the lack of the microscope, but Harvey very clearly saw that there must be some connexion between the arteries and the veins in order to allow for the return of blood from the venous system to the arterial system.

Harvey's book came to be recognized throughout the world as a scientific and physiological classic, and, while certain attacks were made on his theories, these very quickly subsided and in about 50 years' time the concept of the circulation was recognized. By universal approval Harvey is regarded as one of the greatest scientists of all time, showing these great characteristics of the true scientist—namely, excellent experimental technique, accurate observation, and correct and brilliant deduction.

### The Domination of Galen

Now, as we have already said, medical thought of Harvey's day was dominated by the Galenical conception of the four humours, and it is obvious to us that this doctrine is quite incompatible with that of *De motu cordis*. The humoral theory was based entirely on speculation and had no experimental evidence in its favour at all, and one would have thought that Harvey would have immediately discarded it and that his own brilliant investigations would have been reflected in his treatment. Unfortunately little direct information of a documentary character exists as to his actual prescriptions, but a study of contemporary literature, particularly

that of those who were his companions, would indicate that his treatment was quite unaffected by the brilliant discoveries that he had made. He appears to have continued to practise in the Galenical method based on the theory of the four humours. A study of the writings of Ent, Harvey's great friend and, as we know, the one responsible for the publication of Harvey's *De Generatione*, shows that there could have been nothing unorthodox from the Galenical point of view concerning the Master's treatment, as undoubtedly a contemporary such as Ent would have referred to it in his writings. We are therefore driven to the conclusion that Harvey himself continued in this old-fashioned and obviously useless treatment which his own researches should have ousted.

What is the reason for this? How can it be that a man of such astonishing intellect and clarity of vision could still continue with this mediaevalism? Before trying to answer this, let us look at some other figures of the same or just later period. Students of the history of medicine are unanimous in agreeing that Thomas Sydenham was one of the greatest figures of seventeenth-century medicine. Born in Dorsetshire in 1624, and educated at Oxford, he interrupted his studies at that University to serve in the Civil War on the side of the Parliament. He later returned to Oxford and studied medicine. He was created a Bachelor of Medicine in April, 1648; he became a Fellow of All Souls and continued to practise medicine in Oxford. In the year 1661 Sydenham came to London and settled in Westminster. He was admitted a Licentiate of the College in 1663, and thereafter developed one of the greatest practices in the City of London. He wrote extensively, particularly on the treatment of fevers and on the plague. These works are available to us in an excellent translation provided by the Sydenham Society. A careful study of these works indicates an astonishing clinical grasp of a very wide number of diseases; in fact it is generally admitted that the descriptions given of clinical conditions have never been bettered. Sydenham also brings a considerable scepticism with regard to treatment, and it is from this point of view that it is very interesting to see what his views were about the circulation. Having studied the writings carefully one is convinced that he also was under the influence of the old Galenical tradition, and that the truth of the circulation, if known to him, certainly did not affect his treatment. Sydenham's views are best summed up in his own words in Section 1 of his "History and Cure of Acute Disease":

"Such are the conditions complicating and interwoven with the very essence of humanity, and that so closely that no one can succeed in wholly making himself independent of them. Hence Nature, in the concatenation of symptoms, has provided a method for the elimination and exclusion of the peccant and foreign matter, which, otherwise, would undo the whole fabric of our frame; and infinitely oftener than we find to be the case would she gain her end, and attain the restoration that she aims at in these ungrateful remedies, if she were not diverted by ignorant men from the straight way that, of herself, she holdeth" (Sydenham, 1848, i, pp. 29-30).

With such a declaration of faith one would have anticipated the sweeping away of the old-fashioned and outmoded treatments, but if we turn to the details of his treatments we find that they are all influenced and clouded by the old humoral theory. In his remarks about the treatment of apoplexy by bleeding it is quite

clear that he is concerned with removing the poisonous humour rather than relieving a medical condition. This is borne out by the following sentence:

"But forasmuch as bleeding alone may not be effectual enough to remove the cause of this mischief, it is necessary to attempt remedies that evacuate by purging which, though it does not immediately reach the morbid matter as bleeding, yet by emptying the humours of the blood into the bowels, it diverts the more plentiful recourse of humours to the brain; and, by turning the stream inward upon the bowels, doth much restrain their impetus upon that part which is the seat of this disease" (ibid., ii, p. 345).

One could quote many other examples in Sydenham's work, and we must come to the conclusion that this most enlightened physician who did so much to remove from medicine many of the useless practices of Galenism still was not influenced by the work of Harvey on the circulation.

The ablest men in the medical profession, if they had a strong scientific bent, turned their attention to subjects other than medicine—to biology, physics, geology, psychology and philosophy, or demography. It seemed that as a field for scientific research medicine offered them little attraction as distinguished from empirical and traditional routine.

#### The College and the Royal Society

Why, it may be asked, could such men not find at the Royal College of Physicians of London opportunities to conduct their research? Was it outside the terms of reference of the College? Was the College opposed to such advance? There is nothing to show that Harvey's discovery, and the spirit in which he made it, was in any way unacceptable to the College. There was, however, one man who, though not holding the office of President, could, as Treasurer, exercise considerable influence. He was Baldwin Hamey. His biographer writes:

"Chemistry too now began to come in vogue which Dr. Hamey could not well be reconciled to, from his Galenical principles and at his age. Neither . . . was there room for the new scientific ideas, which had found a forum at the Royal Society. For Hamey this new foundation represented a destructive rival to his beloved College and a threat to the integrity of the profession for which it stood. He was uncompromising in his loyalty, and found it impossible to reconcile Fellowship of the College with Fellowship of the Society; in the action of Sir William Petty, Dr. Willis, Dr. Glisson, Dr. Goodall, Dr. Millington, and other Fellows who contributed to the new Foundation, he saw an act of betrayal. The Restoration was awakening forces which had to be resisted. He felt it was a time when the College required their undivided allegiance" (Keevil, 1953).

The records of the College contain no reference to the Royal Society, and one is therefore led to the view that this objection was individual rather than collective, although, as we have said, Hamey's influence within the College was considerable. It may therefore well have been that, rather than come in conflict with a beloved benefactor of the College, for such Hamey had been, Fellows felt inclined to find an outlet for their scientific activities in the Royal Society.

To return to those Fellows, it is curious to notice how Willis, deservedly famous for his book on the anatomy of the brain, in his tract on fevers published in 1659 recognized that the discovery of the circulation had

established a new foundation for medicine, and discredited the Galenical doctrine of the constitution of the blood; and then to notice how he clears away the ancient rubbish of the four humours only to replace it with another hypothesis, and takes this for the basis of all his reasonings on fevers, for his explanations of their phenomena and his indications for their treatment.

Richard Lower in his *Tractatus de corde* (1669) refers to his experiments in connexion with blood transfusion, experiments originally reported in the *Philosophical Transactions*. At least in this direction it might be reckoned that Harvey's discovery was capable of application to the treatment of disease. In 1652 Francesco Folli wrote: "I have read William Harvey's book which treats of the movement of the heart and of the blood. This reading with some ideas I had on the grafting of plants gave rise in my mind to the problem that, the circulation of the blood existing, it would be possible to perform the transfusion by means of which one would not only cure but rejuvenate and make robust" (quoted in Graham, 1953). Folli, however, made no attempt to put his ideas into practice. Pepys wrote of the experiments at the Royal Society in his diary (November 14, 1666): "Dr. Croone told me that at the meeting at Gresham College tonight there was a pretty experiment of the blood of one dog let out till he died into the body of another on one side, while all his own run out on the other side. The first died upon the place and the other very well and likely to do well. This did give rise to many pretty wishes, as of the blood of a quaker to be let into an Archbishop, and such like; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health for the amending of bad blood by borrowing from a better body" (Pepys, 1900). No one realized, however, that the success of Lower's experiment was due to transfusion of blood from one animal to another of the same species. So perhaps it was as well that 150 years were to elapse before further and more successful experiments were tried by James Blundell. "In view of the complete ignorance of asepsis, or immunology and of the processes of coagulation, it is indeed fortunate that the pressure of administrative and ecclesiastical disfavour brought about a cessation of further attempt at human transfusion. While the motives and methods of the opponents to transfusion are not above criticism, their effects were of service in postponing further experimentation until the advance of scientific knowledge made conquest of some of the danger and difficulties possible" (Zimmerman and Howell, 1932).

It is, perhaps, pertinent to ask whether Harvey had any conception as to the possible relation between his discovery and the advance of medical treatment. Stimulated by the example of Riolan, Harvey had intended to publish his *Medical anatomy* or *Anatomy in its application to Medicine*. In it he proposed to "relate from the many dissections I have made of the bodies of persons diseased—worn out by serious and strange affections—how and in what way the internal organs were changed in their situation, size, structure, figure, consistency, and other sensible qualities, from their natural form and appearances. . . . For even as the dissection of healthy and well-constituted bodies contributes essentially to the advancement of philosophy and sound physiology, so does the inspection of diseased and cachectic subjects powerfully assist philosophical pathology" (Harvey, 1847).

An accurate knowledge of anatomical changes that take place in disease was of importance both for diagnosis and for treatment, but the man who created the science, who taught us to think anatomically of disease, was Morgagni, whose *De sedibus et causis morborum per anatomen indagatis* was published in 1761. Harvey refers on many occasions to his medical observations, but, like his medical anatomy, these too have been lost. In the seventeenth century the profession was literally ravaged by theories, schools, and systems; iatro-mechanics, iatro-chemistry, humoralism, the animism of Stahl, the vitalistic doctrines of Van Helmont and his followers. Moreover, if we may properly judge from the only surviving letter from his professional correspondence, concerned with directions for treating a lady "affected with a choleric passion of a hot and bilious nature" by blood-letting and purging, Harvey's practice showed no advance on theories obtaining at that time (Willis, 1878). If we may judge from Aubrey: "All his profession would allow him to be an excellent anatomist, but I never heard any that admired his therapeutic way. I knew several practitioners in this town [London] that would not have given 3d. for one of his bills; and that a man could hardly tell by one of his bills what he did aim at. He did not care for chemistry, and was wont to speak against them [sic] with undervalue" (Aubrey, 1813).

"In connection however with experimental medicine, it may be mentioned that in Harvey's *De generatione* of 1651 (Exerc. 57) a striking observation was recorded, which has been overlooked. Harvey related that he had occasionally pricked his hand with a clean needle and then rubbed the same needle in the 'teeth' of a spider and pricked the hand in another place; the skin pricked with the envenomed needle became red, hot and inflamed, and it collected and girded itself for a contest with the poison for its overthrow. No modern experimental pathologist need be reminded of the numerous problems connected with these few words! It came as a shock to find nothing of the same nature was experimentally investigated till the advent of bacteriology in the nineteenth century" (Bayon, 1938).

One of the eminent physicians of the eighteenth century was Richard Mead. He was born in 1673 into a highly intellectual family, his father being a very distinguished nonconformist clergyman. After a liberal education he went to Leyden, where he spent some considerable time. He was a contemporary of Boerhaave, with whom he maintained a lifelong friendship. Time does not permit us to go into details of Dr. Mead's career, but, to give you a general picture of his position in scientific and medical society, we can point out that he was elected a Fellow of the Royal Society in 1703, became a member of Council in 1706, and a Vice-President in 1707. He also occupied a very high position in the Royal College of Physicians, being Censor, Harveian Orator, and Elect, and in 1744 was chosen by the Elects as President but declined the honour. Despite this highly scientific background, if we turn to Richard Mead's published works we can see that he was still dominated by the old humoral pathology. The following quotations from *Medical Precepts and Cautions* indicates quite clearly that the lesson of Harvey had miscarried:

"Of these [fluids] the principal is the blood, from which are derived the several humours subservient to the various uses and purposes of life; and in particular that subtle

and remarkably elastick fluid, generated in the brain, and known by the name of animal spirits, the instrument of sense and motion which functions it never could be capable of executing, were it not contained in proper organs" (Mead, 1762).

Again a study of the writings of Boerhaave, who was by far the most prominent scientific physician in Europe at the time, indicate quite clearly that he had failed to discard the old Galenical concept of disease.

Whilst this ignoring of the practical side of Harvey's teaching is practically universal in the eighteenth and early nineteenth century, one can point to a few published works that indicate that their authors had at least profited by the labours of Harvey. Giovanni Lancisi, 1654-1720, very clearly had an excellent concept of the circulation and was able to develop what is really the basis of modern cardiac pathology. Another physician of the late seventeenth and early eighteenth century, Vieussens, described a number of cases with valvular disease of the heart almost certainly due to syphilis. Whilst Vieussens was given to more airy speculation than Lancisi, a study of his papers does bring a refreshing modern touch, and it is quite clear that he understood the mechanics of the condition, this being possible only if one had a full grasp of the Harveian doctrine.

It would be possible to go on almost indefinitely quoting examples of the lack of understanding of the physiological principles in the seventeenth and eighteenth century, and for every single case for understanding such as those two just quoted there would be certainly many on the other side. We therefore must accept that Harvey's teaching failed for certainly a century and a half to rid medicine, and particularly therapeutics, of the mediaeval rubbish.

#### Tyranny of the False Doctrine

It is interesting to look at other sciences to see if we can find a parallel to this tyranny of a false doctrine. If we look at chemistry we find that it was clouded with a whole series of false doctrines, but they did not persist for the same length of time as the Galenical work did in medicine. One of the greatest bars to progress in chemistry was the famous phlogiston theory. This was introduced by Stahl and persisted for nearly 100 years, being finally disposed of by Black and Lavoisier at the end of the eighteenth century. Until this unnatural theory had been got rid of it was absolutely impossible to have any progress in chemistry, since the whole mechanism of oxidation, one of the most vital processes, could not be understood. Nearer to our times there was the great controversy on chemical formulae. Up to the middle of the last century organic compounds were analysed for their elementary composition—say, carbon, nitrogen, hydrogen, oxygen, sulphur, and so forth—and the molecular formula was simply calculated on the basis of the atomic weight of the individual atoms. This gave a formula which indicated that the compounds contained so many atoms of carbon, so many of hydrogen, so many of nitrogen, and so forth, but gave no idea at all as to their grouping. Many of the great organic chemists maintained that it was impossible ever to tell the grouping of the elements and that the atoms were probably arranged in molecules and held together by some kind of chemical affinity possibly in a state of violent agitation. In its way this was just as serious a bar

to progress as the humoral theory was to medicine, because until one could get a concept of the spatial distribution of the atoms modern synthetic organic chemistry could not be born. The actual modern system of recording chemical formulae was not thrashed out until the latter half of the last century, and the names of Kekulé, Cooper, Van Hoff, and Le Bel introduced the kind of formula that we are dealing with to-day. It is interesting to know that the chemical bars to progress were got rid of more quickly than those in medicine, the reason probably being that the subject of chemistry is much more easily subjected to direct experimental approach, whereas in medicine only years of clinical experience can really test a new theory or displace an old one.

Let us turn now to the title of this lecture: "A Riddle of the Seventeenth Century." The *Shorter Oxford Dictionary* describes a riddle as follows: "A question or statement intentionally worded in a dark or puzzling manner, and propounded in order that it may be guessed or answered." I trust I have avoided presenting my riddle in a dark manner, though I must confess that I cannot escape the accusation of puzzling. Why was it that this brilliant group of men to whom I have referred failed in the years following Harvey's death to apply the science that had developed at their time to therapeutics? What was the curious lack of association between the Royal Society and the Royal College of Physicians due to? I have quoted Dr. Keevil's remarks about Hamey, and it seems difficult to believe that Hamey, despite his great influence, could have influenced the College to such an extent that no reference at all appears to the Royal Society in the annals. A study of Hamey's life showed that he retired from London to Cheltenham in 1665, and that from then onwards he had only very sporadic connexions with the College. He died in 1676.

The Royal Society was founded and incorporated in the year 1660, and we are now approaching the tercentenary celebrations. Many founding members of the Royal Society were members of the College, and a number of them were high officers of the College. For example, the first Harveian Librarian, Christopher Merrett, was a founding member. Those who are familiar with descriptions of the early meetings of the Royal Society will remember the enthusiasm with which the early Fellows disproved the myths of science that had clouded knowledge in the past, and yet these men, the very medical Fellows who were taking part in them, still continued in their extraordinary mediaeval practice.

Looking back from the present century, one can see that reasonable therapeutics must rest on a tripod; one leg being a knowledge of the circulation, a second a knowledge of the infective nature of most diseases, and the final leg an adequate variety of powerful drugs. The seventeenth and eighteenth centuries lacked the last two legs, and it is therefore perhaps not surprising that physicians continued with their old practices. Again, as we have already pointed out, the seventeenth and eighteenth centuries were characterized by a flood of new theories indicating that physicians were constantly trying to get something more plausible than the Galenical doctrines.

If we look at the therapy of this century before the advent of the sulphonamides and antibiotics, many of the practices look little less foolish than those to which we have referred. This may be the answer to the

riddle, though it still seems strange that they should go on with the weird practices. There is one passage in the *De motu cordis* which seems to indicate that Harvey did have some perception of the revolutionary nature of his discovery, and I propose to conclude in no better way than quoting from him:

"When I finally reckon up the number of questions that can be settled, doubts resolved, and obscure places made clear, given this illuminating truth, in every part of medicine (physiology, pathology, semeiotics, therapeutics), I find a field of such vast extent that, if I explored it fully in all directions, not only would this treatise of mine turn, contrary to my plan, into a full-sized book, but the rest of my life would perhaps not suffice for my writing of it" (Harvey, 1958, p. 96).

It is a pleasure to acknowledge the help received from my friend, the Librarian of the Royal College of Physicians, Mr. L. M. Payne. I am grateful to the President of the Royal College of Physicians for the facilities of the Library.

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A plan for dealing with large-scale accidents or disasters devised last year by Dr. JAMES FAIRLEY, senior administrative medical officer of the South-east Metropolitan Regional Hospital Board, was tested in February by the exercise "Cascade." The site of the mock disaster was St. Olave's Grammar School for Boys in South-east London, and at least 170 "casualties" took part. Summing up at the "post mortem" after the exercise, Dr. Fairley said that local disaster plans must exist and be constantly reviewed. The board's overall plan had proved to be workable, but the closest collaboration between police, fire, ambulance, and hospital services was necessary, and the role of communications was obviously vital. The senior medical officer at the site must be easily identifiable by day or night, and he and the senior ambulance officer must work in close collaboration. A glossary of inter-service terms should be prepared so that all four services used the same terminology, and the Metropolitan police should decide what particulars were to be obtained from casualties. The hospital alerted first should be known as the "designated hospital," which would automatically provide the senior medical officer.