Papers and Originals

Growth of Harvey's Ideas on the Circulation of the Blood*

GWENETH WHITTERIDGE, M.A., D.PHIL., F.S.A.

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The erroneous belief that Harvey knew all about the circulation of the blood in 1616 came about in this way. There is, as is well known, a manuscript in the Sloane collection of the British Museum, written by Harvey and containing his Lectures on the Whole of Anatomy. In this manuscript we read: "WH It is certain from the structure of the heart that the blood is perpetually carried across through the lungs into the aorta, as by two clacks of a water bellows to raise water. It is certain from the experiment of the ligature that there is a passage of the blood from the arteries to the veins. And for this reason, it is certain that the perpetual movement of the blood in a circle is caused by the heart beat." No plainer statement of the fundamental concept of the circulation could be desired. The experiment with the ligature here alluded to is described in detail in *De motu cordis*, chapter 11, so we may be quite certain that when Harvey wrote this passage the hypothesis that the blood circulated had already taken shape in his mind, and that he had already designed and performed at least one of the experiments by which it could be proved.

The lectures contained in the manuscript were given by Harvey at the College of Physicians as part of the elaborate course of lectures and anatomical demonstrations covering a period of six years which it was his duty as Lumleian Lecturer to give and to perform. He had been appointed Lumleian Lecturer in August 1615, and the manuscript bears on the title-page the statement in his own hand that these lectures were delivered by him on 16, 17, and 18 April 1616. It is not surprising, therefore, that at first glance the date on the title-page and the statement on the circulation should have been associated. The first person to make the association was Sir Edward Sieveking, who saw the manuscript, read the famous passage, and, being much impressed by it, took it as the subject of his Harveian Oration in 1877. The general acceptance of the association of the passage and date was apparently facilitated by Harvey's own statement in the letter to Dr. John Argent which serves as an introduction to *De motu cordis*: "On several earlier occasions in my anatomical lectures, I revealed my new concept of the heart's movement and function of the blood's passage round the body."

As a direct consequence of Sieveking's enthusiasm, a committee was set up by the Royal College of Physicians, and in 1886 the manuscript was published in facsimile with a printed transcript facing the pages of Harvey's handwriting. The express purpose of the publication, as said in the introduction, was "to make public the original notes of the lectures in which Harvey, in 1616, set forth for the first time his discovery of the circulation." The editors who were responsible for the text and who wrote the introduction, however, did not notice that, apart from the famous passage already quoted, there is not one single reference in the whole text to the circulation of the blood. The text which they printed, moreover, contains so many errors

that it is almost impossible to read and make sense of, unless one is in a position to correct it by reference to Harvey's own writing. As a result, since 1886, comment and criticism of Harvey's lectures have, with few exceptions, been based not on the text of the lectures but on what is said about them in the introduction. And a great many of the assertions there made are untrue. In addition, much ink and paper has been spent in attempts to explain why Harvey should have said so precisely in *De motu cordis* that he had demonstrated the circulation for "more than nine years," when he had apparently known it for twelve years.

Harvey's Anatomical Writings

When I had finished the publication of Harvey's notes for his *De motu locali animalium*, written in 1627, the Royal College of Physicians suggested that I should complete the work on Harvey's manuscripts by producing a new text of the *Prelectiones anatomie universalis*, together with a translation, and add to it the text of the lectures on muscles, contained in the manuscript of *De motu locali animalium*. In this way, all his anatomical writings could be put together in one volume. The notes on muscles, although not dated in the manuscript, were datable from the fact that the paper on which they were written was made in Holland in 1618, while an addition to the text of the notes derived from the second edition of Bauhin's *Theatrum anatomicum*, printed in 1621. Between these two dates, therefore, the notes were undoubtedly written. If Harvey conducted his first public anatomy as Lumleian Lecturer in 1616, it followed by the terms of his appointment that in 1619 he gave for the first time the special anatomy of the arms and legs. As the scope of the notes on muscles seemed to correspond well enough with this anatomy, the assumption that they were written in 1619 and represented this special Lumleian anatomy seemed acceptable.

All Harvey's known manuscripts, written before the publication of *De motu cordis*, are therefore now in print: the *Prelectiones*, or Lectures on the Whole of Anatomy, of 1616, the lectures on muscles of 1619, and the treatise on animal movement of 1627. We hoped that out of this might come a complete account of the development of Harvey's thought over the years in which the hypothesis of the circulation must have been forming itself in his mind. But no clear-cut account of the stages of his discovery has emerged. Instead we find ourselves in possession of a number of fascinating and intriguing remarks which, if interpreted aright, may perhaps serve as signposts to the way he travelled.

His Lectures on Anatomy

Harvey's Lectures on the Whole of Anatomy were delivered for the first time, as has already been said, on 16, 17, and 18 April 1616. In keeping with the whole tradition of medical

* Harveian Lecture delivered to the Harveian Society of London on 16 March 1966.

teaching during at least the previous 200 years, the public anatomy lasted three days. On the first day the lower belly and the viscera were demonstrated; on the second the middle belly, or thorax, together with the heart and lungs; and on the third the upper belly—that is, the head and the brain. This order Harvey followed in his lectures. But Harvey did not hold a public anatomy only in 1616. We have reason to believe that between 1620 and 1627 he conducted a general public anatomy every year with the exception of 1621 and 1625, and on these subsequent occasions he also used the same notes.

The only independent witness of his lectures so far known to us is Sir Simonds D'Ewes, who attended them for three days in March 1623. Unfortunately, he merely says that he "gained much profitable knowledge" by his attendance and makes no mention of the topics discussed. The notes themselves, however, show that during these years they were revised by Harvey and many additions made to them. A number of these additions are datable, and the latest derive from the edition of Riolan's *Anthropographiae* printed in 1626. Some of these additions are scribbled between the lines or in the margins, some on left-hand pages which were originally blank. The famous passage concerning the circulation is written by itself at the end of Harvey's notes on the heart, which finish at the bottom of a right-hand page. His notes on the lungs, which follow, begin on the next right-hand page. The left-hand page of this opening was originally blank, and there, on some occasion whose date we do not know, Harvey wrote his statement concerning the circulation of the blood. The handwriting is different from that of the bulk of the lectures written in 1616, it is different from that of 1626. On the evidence of the handwriting there is nothing to prevent us believing that the passage was added by Harvey as late as 1627 or 1628, or perhaps even later.

Corroboration of this palaeographical evidence can be had from the text of the lectures themselves, for they afford abundant proof that in 1616 Harvey did not know of the circulation of the blood. For the basis of his lectures Harvey used the textbook of anatomy written by Caspar Bauhin, the *Theatrum anatomicum*, and published in 1605. As in Padua the text of Mondinus had been used as a basis for commentary on an anatomical dissection, so Harvey used Bauhin. The book was convenient, it was up to date, it had useful notes on the main authorities, and it avoided going into too much detail on points of anatomical controversy. Moreover, Bauhin had himself been a student at Padua twenty-two years before Harvey. His writing was coloured by the Paduan teaching, in direct descent as far as the anatomy was concerned from Vesalius, and in its philosophical flavour Aristotelian. Its outlook was similar to Harvey's own. This book Harvey copied into his notes, adding here and there his own comments, marking from time to time his disagreements. But the result is that whoever reads and quotes from Harvey's lectures should beware lest the passage he has singled out for comment should prove to be not Harvey's opinion but Bauhin's. With care, however, it is possible to disentangle Harvey's own remarks and consequently to know precisely what he thought about the movement of the blood in 1616.

Two New Facts

When he began to write all thinking on this topic was directly or indirectly derived from Galen's teaching on the subject. A certain number of modifications and elaborations had been introduced, with the result that by the seventeenth century it consisted in a mass of confused and confusing opinions, some incompatible with each other, or even directly contradictory, as Harvey himself makes plain in the introductory chapter to *De motu cordis*. It should be remembered, however, that it was the best selection out of these ideas that Bauhin accepted in his

book and that Harvey had learnt as a student. Only two new facts—that is, new since Galen's time and which were to have a direct bearing on Harvey's discovery—were known in 1616. The first was the knowledge of the transit of the blood through the lungs, the second the knowledge of the existence of the valves in the veins.

Whatever we may say nowadays about the discovery of the pulmonary transit, the seventeenth century had no doubt that the credit for it should be given to Realdo Columbus. A contemporary of Vesalius, and his successor in the chair of anatomy at Padua, Columbus published in 1559, in his *De re anatomica*, a description of the pulmonary transit which is brief and to the point: "Between the ventricles is the septum through which nearly every one thinks that there is a way open for the blood to pass from the right to the left ventricle. That it may do this more easily they maintain that it is rendered fine (*tenuem*) in the transit on account of the generation of vital spirits. But they err by a long way. For the blood is carried to the lungs through the pulmonary artery and there made more fine (*attenuatur*). Then together with the air it is carried through the pulmonary vein into the left ventricle of the heart. This up to now no one has either observed or recorded in writing, albeit it was most fit to be observed by all." Columbus based his observation on the simple experiment of cutting the pulmonary vein in a living dog at some distance from the heart to discover whether the vein contained blood or air.

Influence of Fabricius

By the beginning of the seventeenth century the fact of the pulmonary transit of the blood was more or less generally known, and Harvey most probably heard of it at Padua. At Padua, too, he learnt of the valves in the veins from his master, Fabricius of Aquapendente, for, although the treatise *De venarum ostiis* was published only in 1603, Fabricius says that he had seen and demonstrated the valves for many years. It was Fabricius's habit not to rush into print with his newest discovery. He was cautious and allowed his writings to accumulate, subjecting each one to long and careful scrutiny and criticism. His idea of the function of the valves was, however, in the light of Harvey's future knowledge totally erroneous, but given the prevailing idea of the movement of the blood it was the only interpretation possible—namely, that their purpose was to impede the flow of blood from the heart to the periphery, to slow it down and prevent it from collecting at the extremities. Once published by Fabricius, the description of these valves passed immediately into the textbooks and became part of the common knowledge of the seventeenth century.

What Harvey also undoubtedly learnt at Padua from Fabricius was the practice of minute and careful observation. Perhaps this precise attention to detail was already a characteristic of Harvey's own attitude of mind, but it was certainly encouraged and fostered by precept and example of Fabricius. By all accounts Fabricius seems to have been one of the greatest teachers of anatomy, and perhaps one should say of physiology, of the seventeenth century. He was interested not only in human anatomy but also in comparative anatomy, and particularly in the light which this study could throw on the problems of action and function. This characteristic, too, we find in Harvey's works. It is quite obvious from his *Praelectiones* that the mere anatomical description of the parts is not his chief interest. He is far more concerned with their action and function. His interest in comparative anatomy and physiology also probably derives from Fabricius, as does certainly his life-long and consuming interest in the problem of animal generation.

When Harvey was at Padua Fabricius was already nearly 70. He had been appointed to the chair of anatomy in succession

to Fallopius in 1565. His new permanent anatomical theatre had been built and opened in 1594. He was not only responsible for performing anatomies and dissections but also for the teaching of surgery and of those aspects of medicine relevant to surgery. If Harvey arrived in Padua for the academic year beginning in November 1599, then he was there when the students of the German nation obtained from Fabricius a promise to perform for them during that winter both a general anatomy and a special dissection of the lower belly, and to give them some specialized teaching in surgery and a demonstration of all the surgical instruments employed. In January 1600, a period of particularly cold weather, Fabricius performed a long and detailed anatomy lasting a number of days and using for the purpose three bodies, two male and one female. In addition, he gave a separate dissection of the eye. He demonstrated the bones of birds and performed several complete anatomies of animals. In November 1600 he lectured in the lecture theatre on the method to be used in general anatomies, and in the anatomy theatre he demonstrated the anatomy of the foetus and that of a horse and of a sheep. In January 1601 he gave anatomical demonstrations of the foetuses of several animals and then a human anatomy. After that he proceeded to the demonstration of many surgical operations. But in the following November he was at his old tricks again, putting off requests for anatomies made by the students with fair words and golden promises, and he does not seem to have performed another public anatomy until after Harvey had taken his doctorate.

During these years Fabricius also lectured on topics such as: Of similar and dissimilar parts and their actions; Of ulcers; Of fractures and luxations; Of tumours; Of particular diseases. His pupil, Julius Casserius of Piacenza, was also assisting in the teaching, though as yet unofficially, for he was not appointed to a lectureship by decree of the Doge until after Harvey had left Padua. The great interest of Casserius was in comparative anatomy, and to him also Harvey refers in his writings. In addition to the formal teaching by Fabricius, we may be almost certain that Harvey attended private instruction given by him, perhaps at his own house, for this was a common practice in Padua at the time. Fabricius lived near the Hospital of St. Francis. It was in this hospital that in 1543 Giovanni Batista da Monte (Montanus) began to teach medical students practical medicine at the bedside. That Harvey himself attended such rounds is obvious from the references which he makes in his anatomical lectures to cases which he had seen in the hospitals in Italy. There were five professors of the Practice of Medicine in Harvey's time, of whom Johannes Thomas Minadous of Rovigo, appointed in 1596, was the one who examined Harvey for his doctorate and to whom a reference will be found in Harvey's Lectures.

Experience in Autopsies

That Harvey's experience in human dissection was limited to his attendances at the formal anatomies is unlikely in view of the rules of the public health department of Padua in force at the time. Because of the frequent incidence of plague in the city, those who died from unknown causes could not be buried without a doctor's certificate proving that the body had been examined and that the cause of death was not plague. One such certificate from Minadous himself exists from the year 1613. As a result, autopsies were often held on private patients and also on patients in the hospitals. That Harvey attended at least some of those in the hospitals seems likely. In London at this date we have evidence that autopsies were performed in St. Thomas's Hospital, by a surgeon in the presence of the physician, and it seems probable that they were also performed in this way at St. Bartholomew's Hospital. That by 1616 Harvey's knowledge of pathology was extensive is evident from the Lectures.

Training in Aristotelian Philosophy

In addition to this knowledge of things pertaining to medicine, Harvey returned from Padua trained in Aristotelian philosophy and in methods of Aristotelian thought. His doctoral diploma, which is not for medicine alone, but for medicine and philosophy, is signed, in addition to Fabricius, Casserius, and Minadous, by George Raguseus of Venice, who was then, in 1602, second professor of ordinary philosophy. In the preceding October he had succeeded to the chair of Cesare Cremonini when Cremonini had been promoted to the first chair. That Harvey attended Cremonini's lectures is more than probable. He refers to him in his *De generatione animalium*, though over a point in which he is in disagreement with Cremonini. Cremonini was one of the most distinguished of the Aristotelian philosophers who taught at Padua. He was a friend of Galileo, but at the same time a resolute antagonist of the astronomer, a defender of Aristotle against the attacks of Galileo. But all this happened after Harvey left Padua. What is important about Cremonini is that he carried on and developed the philosophical methods of his predecessor Zabarella. He pointed out that while the application of Aristotle's scientific method to the mathematical sciences was not inordinately difficult, its application to the natural sciences was neither obvious nor easy, for in them the knowledge of first principles from which the argument should start was not readily to be discovered. In consequence, in the natural sciences Cremonini advocated ever more careful observation of particulars and analysis of that observation, and would have a man proceed to his conclusions by the ways both of induction and of deduction.

While Harvey was at Padua, Cremonini was lecturing on the *Physics* of Aristotle, and possibly on *De anima* and on *De generatione et corruptione*. It is perhaps not mere chance that the method which Harvey describes as his own in his *De generatione animalium* lays stress on the necessity for frequent observation and on the inductive method of reasoning. Harvey's own ideas on the methods to be used in advancing knowledge are thus seen to derive, at least in part, from what he learnt at Padua. This may also account for his famous but somewhat mysterious remark about Bacon. "He writes philosophy," he said to Aubrey, speaking in derision, "like a Lord Chancellor." While everyone thinks that the remark is witty, no one is quite certain why. But that which is peculiar to a Lord Chancellor is his equity. It was Harvey's friend, John Selden, who wrote of equity that it is "a roguish thing," and that it is administered "according to the conscience of him that is Chancellor. . . . 'Tis as if they should make the standard for the measure . . . a Chancellor's foot. . . . One Chancellor has a long foot, another a short foot, a third an indifferent foot: 'Tis the same thing in the Lord Chancellor's conscience." Trained in the Paduan developments of Aristotelian philosophy, it is possible that Harvey found Bacon's philosophy as "roguish" as his equity, as personal and individual as the judgements pronounced in his own courts.

Alleged Influence of Galileo

Before leaving the question of what Harvey learnt at Padua we might consider for a moment the alleged influence of Galileo. It has at various times been suggested that Harvey attended Galileo's lectures, that he lived in his house as a private pupil, and that Galileo's teaching on the astronomy of Copernicus had a profound influence on Harvey's thinking concerning the circular movement of the blood. First, there is no evidence whatsoever to show that Harvey lived with Galileo. Galileo himself recorded the names of many of his private pupils, and Harvey's name is not among them. They were all, as far as is known, mathematicians, engineers, and architects, not physicians. Secondly, while Harvey was in Padua, Galileo

was lecturing in the University on the works of Euclid and on the Book of the Spheres. His private work was chiefly concerned with the mechanics of Aristotle and with the designing and perfecting of various mathematical instruments. Thirdly, Galileo's first public pronouncement on the Copernican system was made at the end of 1604, and were it not for the fact that in a private letter written to Kepler in August 1597 he reveals his knowledge of Copernicus's theory of the movement of the spheres, it would not have been known that as early as this he had begun to think along these lines himself. His teaching was still in accordance with Aristotelian and Ptolemaic thought. That Harvey attended lectures on mathematics seems unlikely. He makes only one reference to Euclid in his writings, and that is a mistake, for he attributes to Euclid the pronouncement of Archimedes, Give me a lever and I will move the earth. There is little evidence that Harvey in his study of the mechanics of muscle contraction used any other works than those of his master, Fabricius.

Movement of the Heart Observed

Trained, therefore, in habits of precise observation, knowing the use and value of vivisection as well as of animal and human dissection in problems fundamental to physiology, and versed in the philosophy of Aristotle, Harvey returned to London, where presumably in spite of his appointment as physician to St. Bartholomew's Hospital and his growing private practice he still found time to continue the observations which were fundamental to the two great interests of his life: movement as exemplified in the movement of the heart and blood, the movement of respiration, the movement of muscles; and the generation of animals. All these interests were simultaneously present in his mind, and notes on all these topics accumulated over the years. By 1616 some of the observations which within the next few years were to lead through experimental proof to the establishment of the totally new idea of the movement of the blood had already been made and will be found incorporated in the text of his lectures.

With Harvey's anatomical description of the heart in his Lectures we need not bother, for it is taken straight out of Bauhin's textbook and is of little interest. It is Harvey's observations on its movement that are important, for they show that he had already begun to question the usually accepted theory of its action. At this time it was generally believed that the apex beat of the heart occurred during its filling in diastole, that the heart sucked the blood in, and that the arterial pulse was due to the contraction of the arterial wall.

The first questioning of the validity of this description came from Padua. It seems likely that Vesalius himself doubted its truth, for in his public anatomy at Bologna in 1540 he made the students feel the heart of a vivisected dog with one hand and the beating of the pulse with the other to see whether the arteries were compressed when the heart was dilated, or whether they had the same movement as the heart. Questioned, he refused to give his own opinion but merely said: "Feed yourselves with your own hands, and trust them." The reason for his reticence may well have been that his opinion was contrary to the teaching of Galen, and contrary to the generally accepted belief. However that may be, Columbus had published in 1559 his own observations on the movement of the heart and his conclusions that systole is contraction and diastole dilatation, that diastole of the heart corresponds to systole of the arteries, and that this is the contrary of the view commonly accepted. With this passage Harvey was much impressed, for he quotes it at length in his notes and then proceeds to reveal the extent of his own observations. "I have observed these things for hours together and I have not been able easily to distinguish between them either by sight or by touch. Wherefore I would

now lay them before you that you may see with your own eyes and judge for yourselves."

Harvey's Knowledge in 1616

Harvey's conclusions are completely in agreement with Columbus's, and he had, moreover, performed the experiment of making a deep cut in the ventricle and watching the blood spurt out as the heart lifted itself up. If we list the observations made in the lecture notes and compare them with what Harvey says on the subject in *De motu cordis* chapter 2, it is obvious that his study of the movement of the heart was already almost complete and that he had little more to add twelve years later. The same is true of his observations on the movement of the arteries and of the action of the auricles. The material for the greater part of chapters 3 and 4 of *De motu cordis* will be found in the notes. This means that in 1616 Harvey already knew that the principal movement of the heart was its contraction, that is its systole. Observation showed that the contraction of the heart occurred as it lifted itself up and struck the chest wall, and, consequently, that the apex beat coincided with the systole of the heart. By observation also he learnt that the pulse of the arteries could be felt at the same moment as the apex beat, and therefore the systole of the heart coincided with the diastole of the arteries. It also followed that the blood was impelled into the arteries by the action of the heart, "as my breath in a glove," a simile which he used again in *De motu cordis*.

From his observations on the movement of the auricle he knew that it aroused a sleepy heart and that the right auricle was the last part to pulsate when the heart was dying. These observations and experiments he had carried out on diverse creatures, snakes, frogs, and fish. Observations lacking in the notes but to be found in the first four chapters of *De motu cordis* relate to the dependence of the arterial pulse on the vigour and frequency of the heart's contractions, a refinement on the fundamental observations, and all his observations but one on the pulmonary artery. Some of the observations found in *De motu cordis* chapters 5 and 6 also occur in the notes, but we do not find in them any details concerning his work on animals possessing hearts with a single ventricle and no lungs, or on the foetal heart. It would seem that in 1616 he had not realized the importance which he would later attach to both of these in the development and proof of his hypothesis that the blood circulated. The discussion of the transit of the blood through the parenchyma of the lungs occupies chapter 7 of *De motu cordis*, where it is not so much proved as inferred. In the *Praelationes* it is merely stated: "When the heart is relaxed, which happens first, there is an entering into it of blood into the right ventricle from the vena cava, and into the left from the pulmonary vein. When it is lifted up or contracted it drives out the blood as it were by force from the right ventricle into the lungs and from the left ventricle into the aorta, and this is the reason for the arterial pulse."

Dissimilarities with "De motu cordis"

It is when he has reached this point in *De motu cordis* that Harvey explains that everything that he has so far said is more or less in accordance with the findings of Galen and Columbus, but that what remains to be said is entirely new and never before mentioned. From chapter 8 onwards he lists the experimental proofs of the circulation of the blood through the body, and from chapter 8 onwards all parallels with the *Praelationes* cease, until he returns in the last chapter of the book to some more general considerations touching the heart, none of which is of importance in the proof of the circulation of the blood throughout the whole body.

It is not now, therefore, the similarities between the *Prelectiones* and *De motu cordis* which are of significance in determining the state of Harvey's ideas in 1616, but the dissimilarities. In the Lectures he does not mention one of the experiments with ligatures which convinced him of the passage of the blood from the arteries to the veins. His interpretation of the valves in the veins, which, contrary to the assertion made in the introduction to the 1886 edition, he never describes and only once mentions, is as follows: "The valves set in a contrary direction break off the pulse both in the heart and in the other veins. WH and for that reason the veins have very many valves opposed to the heart." When he wrote that it is difficult to believe that he had any idea of the circulation of the blood throughout the whole body. And this is a crucial point, for we know on the authority of Sir Robert Boyle that it was the consideration of the valves of the veins which first "hinted" to him, or "induced him to think of" the circulation of the blood.

Throughout the *Prelectiones* the veins are spoken of as carrying blood to the various parts of the body to nourish them. His remarks concerning the splenic branch of the portal vein are of particular importance. Speaking of the omentum, Harvey says: "it bears the veins from the portal vein to the stomach and the spleen," and of the stomach: "it receives veins from the splenic branch of the portal vein and they bring in blood and take off chyle." The idea of blood and chyle going different ways in the same vein is traditional, and totally contrary to Harvey's subsequent explanation as set out in *De motu cordis*, chapter 16.

Years of Silence

It would therefore seem clear that, in 1616, Harvey knew the movement of the heart in systole and diastole, systole and diastole of the arteries, and the one-way passage of the blood through the lungs and back to the left ventricle and out into the aorta. But there for the moment it stayed. Baldwin Hamey in his *Prelectiones*, delivered at the College of Physicians in January 1648, summed up the situation when he said: "From this left ventricle it is, as I said, expelled by the arteries into the whole body; and there till of late years it was thought to rest, nor was there any further heed taken of it or account given of it than this, that it served for nutrition and augmentation, for generation of spirits and sperm in their due times. But now, by the conduct of our renowned Professor and Colleague, Dr. Harvey, there is a way found to bring the greatest part back again, and yet no part of the foresaid work left undone."

If we are to take at its face value, as indeed we must, Harvey's own statement in *De motu cordis* that he had demonstrated the circulation of the blood for more than nine years, then the way round was discovered on some occasion in 1618 or 1619, but of its finding there is no record. Why do the additions to the Lecture notes made at intervals up to 1626 give no clue? Was Harvey in fact continuing to teach the Galenic notions on the movement of the blood at a time when he was himself convinced of their falsity? The case would be paralleled by Galileo's. Even in 1626, by which date it would seem reasonable to suppose that *De motu cordis* was already taking shape in his mind, we can have no certainty of his position. To the description which appeared for the first time in the edition of Riolan's *Anthropographiae* printed in that year of how, when he blew into the spermatic—that is, ovarian—veins of a woman, he discovered that the arteries along the uterus were inflated together with the veins, Harvey has added the comment: "Or does the reverse occur?" In *De generatione* Harvey refers to the same passage and contradicts it, saying that this "is a prevalent argument for the Circulation of the Blood, which was my invention; for it doth clearly evince a passage from the Arteries into the Veins, but no retreat from the Veins into the Arteries again." By 1626 one would have thought that

Harvey was already in a position to contradict Riolan's findings and not merely to question them.

Work on Animals

De motu locali animalium, written in 1627, gives little help in clarifying his state of mind. There he alludes to the fact that a frog moves after its heart has been excised, and possibly to the movements of an eel in the same condition. But these were experiments he had already performed by 1616, for in the *Prelectiones* he says: "After the heart has been cut out a frog skips, an eel crawls and a dog walks." Moreover, these experiments are not so much concerned with his work on the heart as with his study of nerves and the part which they play in movement. Harvey also refers to the fact that the right ventricle pulsates last, but that observation too occurs in the Lectures. True, there are three references in the book to *De motu cordis*, but each reference is an addition to the text made after the date in 1627, when the greater part of it was written. Only one experiment described in *De motu locali animalium* is significant for our purpose. "A cock's head off, the arteries being tied and artificial ventilation given (*respirationi dato loco*), movements are seen to persist, but they are as the movements of men in delirium . . ." etc. Though not an experiment connected with the movement of the heart or blood, it proves that Harvey in 1627 was in possession of a technique by which he could perform all the necessary animal experiments for the purpose of proving his hypothesis of the circulation. But in order to try to discover how and when he did it, we are forced to interrogate yet more closely the texts of *De motu cordis* and of the *Prelectiones*.

Harvey himself says that he solved the problem by having frequent recourse to vivisections, by employing a variety of animals, and by collecting numerous observations. Numerous observations on a variety of animals will be found in the Lectures, though by no means all those which are recorded in *De motu cordis*; the method of work had been begun before 1616. It is the animal experiments which are lacking in the Lectures. He refers to the vivisection of a dog which he had performed for the purpose of inspecting its thymus, and that his observations on the movement of the heart had been carried out on some living animal is plain. In fact he says, of his conclusions on the arterial pulse: "This can be proved by actual inspection in the living and in the dead." But apart from making an incision in the ventricle and watching the blood spurt out at each contraction, he mentions no other experiments on a living animal. Before 1628, therefore, he had to perform all those recorded in *De motu cordis*: dividing the arteries in dogs, dividing the veins, and in both cases watching and assessing the results; tying the veins in snakes and fish below the heart and watching the space between the ligature and the heart become empty of blood; watching the blood spurt from the proximal side of a cut artery and from the distal side of a severed vein. All these experiments are of the utmost elegance and simplicity and would not have taken long to perform.

Orthodoxy on Formal Occasions

As Harvey says that he had demonstrated the circulation many times before publishing *De motu cordis*, we may well ask when and where he performed these experiments and to whom he showed them. It has been assumed that it was in the course of his formal Anatomical Lectures. But to demonstrate the actual movement of the blood in a corpse is obviously impossible. To do this he must have used living animals. We know that Vesalius in the course of his public anatomies at Bologna in 1540 used living dogs. We know that this method of teaching was also used in Padua, where, for example, in 1584, Fabricius dissected a living gravid ewe. We know that after 1660 experiments on living animals were performed before the

Royal Society meeting at Gresham College. But there is no evidence to lead us to suppose that living animals were used either at the Barber-Surgeons or at the College of Physicians on the occasions of the formal public anatomies held in those institutions in the first half of the seventeenth century. Of the ceremony attendant on an anatomy at the Barber-Surgeons we have record. We know of the mat to be provided for the lecturer's feet lest he took cold, of the two white rods with which he was to demonstrate the parts, and of the clean sleeves which he was to be given to wear. But nowhere is there any mention of animals. It is not to be supposed that the Lumleian anatomies at the College of Physicians, or the College's own annual anatomy, were occasions for less ceremony.

In both Colleges the purpose of the anatomies was to demonstrate and teach knowledge already acquired and opinions based on authority. They were not occasions for the presentation of new facts, for both Colleges were conservative in outlook and rightly suspicious of novelties. It seems altogether unlikely, therefore, that it was during the course of any formal anatomy at the College of Physicians that Harvey demonstrated for the first time his new theories on the movement of the blood. All that he could have shown on such an occasion would have been the structure of the valves in the veins. We know from Boyle that he began by thinking about this. In the *Prelectiones* he observes that "a rod inserted into the vena cava goes as far as the loins," and we may well speculate as to when he inserted a probe into a more distal vein to find its passage impeded by a valve as he describes in *De motu cordis*. When he had taken that step, it is possible that he may have seen the important experimental proof which could be provided by the ligatures of varying tightness around the arm. This, coupled with the question of the amount of blood extruded from the heart at each beat, would have given him the whole basis for his *De motu cordis*.

"Prelectiones" Harvey's Public Face

If Harvey did not demonstrate the animal experiments by which he proved the circulation during a formal public anatomy, then he must have done so either to his friends and in his own house, and this may well have been the first step, or to the College on some different occasion. It is strange that none of his friends seems to have left any record of attending his demonstrations before 1628, and we are left to guess who did in fact see them. Dr. Argent certainly, for Harvey says so. Possibly Dr. Fludd, who referred to Harvey's discovery in print in 1629 and who is mentioned by Harvey both in the Lectures and in *De motu locali animalium*. Among the more elderly members of the College, Dr. Henry Atkins is a possibility, for he was President four times during the relevant period and at an earlier date had supported Harvey's application for the office of physician to St. Bartholomew's Hospital. Sir William Paddy and Thomas Moundeford are also possible, for they too were Presidents during the right period. But those who are more likely to have been receptive of Harvey's new ideas are his own contemporaries, men like Simeon Fox and Sir Matthew Lister, who were present when Harvey took his doctorate in Padua. Remembering that Aubrey says of Harvey that he was not "stiffe, starcht and retired, as other formall Doctors are," and that Scarborough makes a similar point in stressing his willingness to teach those who wanted to learn, it may well have been that it was among the younger men who were Candidates during those years that he found his audience. Alexander Read and Francis Prujean could have seen the demonstrations, but neither they nor anyone else so far known has admitted to it. Instead, there is a complete blank in the records of the College of Physicians until January 1648, when Baldwin Hamey the younger described the circulation of the blood in the course of the formal anatomy he gave to the College.

And so we arrive at the conclusion that the *Prelectiones*, in spite of the additions to the text, represent Harvey's public face only. They do not constitute his private experimental notebook, and they do not reveal what he demonstrated to the members of the College, nor when, nor where.

Harvey's Answers to Criticism after 1628

When he wrote *De motu cordis* in 1628 Harvey had by no means realized the full implications of his theory, nor had he performed all the experiments by which it could be proved. Some time after this he showed the King the demonstration on the jugular vein of the stag, but this was no advance on his previous proofs. During the next twenty years more experimental proof was evolved, largely in answer to attacks made on him, and these experiments were designed to answer particular points. Riolan's specific criticisms were answered in two Letters addressed to him and published in 1649. The First Letter contains the description of the experiment by which Harvey disposed of the idea that the blood could get from the veins into the arteries by regurgitation, anastomoses, or any other kind of special channel. The Second Letter contains, among other things, the proof of the circulation in the portal vein, a circulation which Harvey had only inferred in *De motu cordis*. And finally, in 1651, came the experiment to prove that there are no porosities in the intraventricular septum. It is described in the Letter written to Slegel in that year, and there Harvey says that he had recently performed it in the presence of several of his colleagues and makes it quite clear that it was performed as a direct answer to Riolan's continued attacks, for the proof of the circulation of the blood was never based on the hypothesis that the septum was a solid wall.

The last remarks which Harvey made on the circulation occur in the Letter to Dr. R. Morison of Paris written in April 1652. "The heart has three kinds of motion, namely the systole in which the organ contracts and expels the blood contained in its cavities, and next, a movement, the opposite of the former one in which the fibres of the heart appropriated to motion are relaxed. Now these two motions inhere in the substance of the heart itself, just as they do in all other muscles. The remaining motion is the diastole, in which the heart is distended by the blood impelled from the auricles into the ventricles; and the ventricles thus replete and distended, are stimulated to contraction, and this motion always precedes the systole which follows immediately afterwards." The clarity of the description is in striking contrast to the bewilderment with which he had first contemplated the heart's action and to which he had confessed twenty-four years previously, saying: "I was almost tempted to think . . . that the motion of the heart was only to be comprehended by God." Of the manner of the heart's action and of its necessary and immediate consequence, the circulation of the blood, he had in the intervening years given to the world the clearest possible description.

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