BRITISH MEDICAL JOURNAL

LONDON SATURDAY DECEMBER 23 1950

HARVEY AND THE SCIENTIFIC METHOD*

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No compliment could be more acceptable to a physician than an invitation to deliver a lecture adorned by the name of Harvey. Yet the task and responsibility which the honour conveys are less enviable, and increase in difficulty as the years advance. Two hundred and thirty Harveian orators at the Royal College of Physicians and nearly a hundred Harveian lecturers of this distinguished society have probed deeply and well-nigh exhausted all that might be said of Harvey, his life and times, his contemporaries and forebears, his philosophy, and his contributions to medicine and science. So little would seem to remain that some orators and lecturers, having faced their difficulties, have solved them by avoiding them and have unashamedly deleted Harvey from the theme of their discourses, whilst not a few have sought refuge in the reflection of the French writer that "everything has been said before, but since nobody seems to listen we can say it all over again." As my tale unfolds you may well believe that I am not wholly guiltless of this exprobration.

Yet new facts concerning Harvey are still being revealed, and in the past year Harveian literature has been splendidly enriched by Mr. Geoffrey Keynes in his *Linacre and Thomas Vicary Lectures* dealing with the personality and portraiture of Harvey. The physician must feel deeply the passing of the initiative in this field of medical scholarship to the surgeon; if he is wise he will not be adduced by Mr. Keynes's assertion that "as a surgeon I belong to the less learned half of the medical profession"; rather will he sense that this is a meretricious concession not to the superior learning of the physician but to his overweening conceit.

But there is an aspect of history more significant than the study of persons and portraits, a study which is too apt to reflect the personality, not of the subject, but of the biographer and the artist. As Sir Arthur Helps has written:

"To us, my friend, the times that are gone by
Are a mysterious book, sealed with seven seals:
That which you call the spirit of ages past
Is but, in truth, the spirit of some few authors
In which those ages are beheld reflected,
With what distortion strange heaven only knows.
History!
Facts dramatized say rather—action—plot—
Sentiment, everything the writer's own,
As it best fits the web-work of his story,
With here and there a solitary fact
Of consequence."

We can, however, study the mind of the man and its working in his writings. It is this which conveys to us a due sense of his greatness; it is this which helps us to assess his contribution to that irrepressible insurgence of the human spirit which reveals itself in the advancing frontiers of knowledge. It explains why we place Harvey amongst the greatest physicians of all times, not because he *discovered* that the blood circulates but because he *demonstrated* beyond all reasonable doubt that the blood circulates. It is with the Harveian method that I propose to concern myself in this lecture, and with why after he had demonstrated "the passage of the blood from the veins into the arteries and of the manner in which it is transmitted and distributed by the action of the heart" he was constrained to write: "But what remains to be said upon the quantity and source of the blood which thus passes, is of a character so novel and unheard-of that I not only fear injury to myself from the envy of a few, but I tremble lest I have mankind at large for my enemies, so much doth wont and custom become a second nature. Doctrine once sown strikes deep its root, and respect for antiquity influences all men. Still the die is cast, and my trust is in my love of truth, and the candour of cultivated minds."

In this passage Harvey hints at the bases, the reasons for human beliefs. Translated into modern terminology these resolve themselves into four groups—the methods of tenacity, authority, intuition, and science or reflective inquiry.

Method of Tenacity

Tenacity, obstinacy, perseveration, call it what you will, is the spring of many of the doctrines we hold to be true. The pain of a new idea, said Walter Bagehot, is one of the greatest pains to human nature. "It is as people say, so upsetting; it makes you think that after all your favourite notions may be wrong, your firmest beliefs ill founded; it is certain that till now there was no place allotted in your mind to the new and startling inhabitant; and now that it has conquered an entrance, you do not at once see which of your old ideas it will not turn out, with which of them it can be reconciled, and with which it is at essential enmity."

And Wilfred Trotter (1932), in his "Commemoration of Great Men"—and I commend not merely this but all that he wrote to your attention—said: "The mind delights in a static environment, and if there is any change to be itself the source of it. Change from without, interfering as it must with the sovereignty of the individual, seems in its very essence to be repulsive and an object

*The Harveian Lecture delivered before the Harveian Society of London on March 22.

of fear. A little self-examination tells us pretty easily how deeply rooted in the mind is the fear of the new, and how simple it is when fear is afoot to block the path of the new idea by unbelief and call it scepticism, and by misunderstanding and call it suspended judgment."

A glance at our medical textbooks establishes that habit or inertia makes it easier for us to continue to believe a proposition simply because we have always believed it. Fables about the value of drugs and other forms of therapy, and of such diagnostic measures as percussion, persist because we close our minds to all contradictory evidence. History may not repeat itself but the writers of textbooks do, and their reiteration, be it true or false, strengthens our beliefs. Moreover, the interests of ourselves, our country, our religion, our business or profession—all these and a host of others—will determine how far we insulate ourselves from contrary views and opinions. And since we have different interests we shall cling to different beliefs. Thus the method of tenacity is incapable of deciding between conflicting views.

Method of Authority

Secondly, as a basis of belief there is the method of authority, the appeal to an accepted source which in the highest degree is regarded as infallible. Two forms of appeal to authority must be clearly distinguished. One is both reasonable and inevitable. In the practice of medicine, for example, no one man can have the time, even if he had the ability, to find out all knowledge for himself. He accepts the carbohydrate values of certain foodstuffs given by competent authorities; he does not question that specific patterns in an electrocardiogram indicate a posterior cardiac infarction, or that herpes zoster is a virus infection. But he retains the right to question the authorities if his own experiences cause him to doubt their dicta; there are other authorities to whom he can appeal; and, indeed, if he is prepared to do so he can acquire the competence and experience himself with which to test the validity of the pronouncements of authority.

The second form of appeal to authority is fundamentally different. It "invokes an external source, which it invests with infallibility and finality to give sanction to beliefs." This is the basis of many of our religious beliefs and our political affinities even to-day, and in the history of science it has played a sinister and obstructive part. Even Vesalius in 1543 was content to teach the Galenic doctrine of the ebb and flow of blood, which, as I shall later describe, demanded a perforated ventricular septum. He wrote: "The septum of the ventricle, composed as I have said of the thickest substance of the heart, abounds on both sides with little pits impressed in it. Of these pits, none, so far at least as can be perceived by the senses, penetrate through from the right into the left ventricle, so that we are driven to wonder at the handiwork of the Almighty, by means of which the blood sweats from the right into the left ventricle through passages which escape human vision."

You will observe that he permits himself to express not doubt at the teaching of Galen but wonder at the handiwork of the Almighty, who could create a ventricular septum which allowed the passage of blood though no perforations are visible; but perhaps his true belief is hinted at in a later writing, when he confessed that he "accommodated his statements to the dogmas of Galen not because he thought that these were in all consonant with truth but because in such a new great work he

hesitated to lay down his own opinions, and did not dare to swerve a nail's breadth from the doctrines of the Prince of Medicine."

The influence of authority is perhaps most strikingly exemplified in the sentence of the Inquisition on Galileo in 1633 for maintaining that the earth goes round the sun. Recollect that this was five years after Harvey had published *De Motu Cordis* and you will better appreciate the courage and magnitude of Harvey's contribution. For in his time the methods of tenacity and authority dominated anatomy and medicine. The anatomist saw only what his mind had been attuned to see by the prior dicta of his teachers; he selected, perhaps unconsciously, what confirmed accepted doctrine. The purpose of dissection in Harvey's time was not to ascertain new facts but to demonstrate the truth of Galenic teaching; the assistant dissected the body, and the lecturer, seated apart, recited the appropriate passages from the works of Galen.

There were a few anatomists, however, prepared to dispute the authority of Galen. Of these, Matheus Realdus Columbus, a man of dubious integrity, who succeeded Vesalius at Padua in 1544, was the most outspoken, for in his *De Re Anatomica Libri XV*, published in 1559 after his death (a fact which might explain his frankness), occurs this indictment of his fellow anatomists: "But for these it is enough that Galen said so. What? To think that some folk in our time swear to the dogmas of Galen about anatomy so that they dare to assert that Galen ought to be taken as gospel, and that there is nothing in his writings which is not true! It is wonderful how men are carried away by this doctrine; and the princes of the anatomy offer it to the rabble. Yet no one sees how much this is to be blamed. Who indeed is there who never offends? But of this enough and more than enough."

Any chapter in the history of human thought will reveal that there can be no uniformity or stability of belief for *Homo sapiens*—the species with the inquiring, the inquisitive mind—which is based on authority, for authorities differ.

Method of Intuition

Is there a better guarantee of stable beliefs than those we have thus far discussed? Some have chosen and urged the method of intuition, the acceptance of truths which when uttered are seen to be and are accepted as self-evident; and they point in support to the utterances of the prophets and poets. But intuitions must be tested by experience, for intuitions are only self-evident within an accepted framework.

It was at one time "self-evident" that the earth was flat, and that the sun moved round the earth; to some it is self-evident that polygamy is a sin; and most people would accept such intuitions as that every effect must have a cause, and that the axioms of Euclidian geometry are true. For most propositions self-evidence has been claimed, yet it is not difficult to establish that early training, inherent desires, and current knowledge play a large part in the formulation of our intuitions, and the certainty and authority with which we utter them is no guarantee of their validity.

Method of Science or Reflective Inquiry

It is the fourth method of arriving at truth, the method of science or reflective inquiry, which is the essence of my theme. Scientific method has been so often described that it may seem unnecessary to elaborate its content and its form, but there are certain aspects of it which in

relation to Harvey's work need elaboration if his contribution to the stream of human thought is to be properly assessed. It is customary to describe three main stages in arriving at a scientific truth. First, there is the observation of significant facts; secondly, a hypothesis is formulated which, if true, would account for the facts; and thirdly, from the hypothesis are deduced consequences whose validity can be tested by observation or experiment.

This is, I suggest, an over-simplification. The first stage of most discoveries is the chance observation, which in not a few instances has been associated with the false assumption. From the time that King Hiero asked Archimedes to test his suspicion that his artificers were alloying gold with silver in his crown, and Archimedes, by chance, noticed that in his bath he displaced water equal to the volume of his body and realized at once that, weight for weight, the lighter alloy would displace more water than the heavier gold, to Fleming's observation on *Penicillium notatum*, the path of the history of science is strewn with the fruits of accidental observations.

But we must not forget with Pasteur that "chance only visits the mind that is prepared." The chance observation lights the spark only in the mind of the receptive, and he responds by a tentative, a very tentative, hypothesis or hypotheses which lead him to seek *significant* facts, by observation or experiment. The operative word is "significant." There must be critical discrimination in the selection of apposite data; there must be lack of bias, and face value must not mislead. It is a mark of scientific ability to be able to distinguish the significant facts and to know where and how they may be found. In other words, "observation of significant facts" is not uncontrolled observation, but observation with a purpose. Meandering aimlessly and unguided in the realm of nature is rarely a profitable pursuit. When the significant facts are assembled possible explanations are formulated; an attempt is made to produce order out of chaos; for science is interested in systems and classes, in generalizations.

It is here that formal logic plays its part. But formal logic is not scientific method. It is an instrument which demonstrates to us that if certain propositions are true others also are true, but logic does not establish the truth of our initial premises. Formal logic aids us in devising ways of formulating hypotheses explicitly and accurately. If alternative hypotheses are available logic helps us in the next stage of our scientific method; it helps us to develop the consequences of these alternative hypotheses so that when compared with other observable phenomena we can test which hypothesis is in harmony with the observed facts.

To summarize, the stages in the method of science as exemplified by its history are: (i) the chance observation; (ii) the immediate reactive hypothesis; (iii) the search for significant facts which confirm or refute the initial hypothesis or (iv) lead to further hypotheses (induction), which can (v) be tested by observation and experiment (deduction).

It is perhaps necessary to re-emphasize, for those who hold the Baconian view that the mere orderly arrangement of data would make the right hypothesis obvious, that at each stage of scientific inquiry ability (it may be intuition) often amounting to genius is indispensable. As Butterfield (1949) has observed: "Of all forms of mental activity the most difficult to induce, even in the minds of the young who may be presumed not to have lost their flexibility, is the art of handling the same bundle of data as before, but placing them in a new system of relations with one another,

by giving them a different framework, all of which virtually means putting on a different kind of thinking-cap for the moment." But any logically trained mind, once the sequence is pointed out, must of necessity accept the conclusion or question the accuracy of the observations or the validity of the logical processes which led to the hypothesis.

The Galenic Doctrine

Let us within this scheme of belief consider the contribution which Harvey made to the knowledge of the heart and blood vessels. Until his day, and indeed for many years after in places where the methods of tenacity and authority held sway, the universal teaching was the Galenic doctrine. This regarded the heart as a source of heat, an innate God-given property persisting from the beginning to the end of life. Food particles passed from the gut by the portal vein to the liver, where a crude blood of base nutritive quality (said to be endowed with "natural spirit") was manufactured and, passing to the right side of the heart, was sent by flow and ebb to irrigate the tissues and organs. Diastole of heart and veins occurred at the same time. The "natural spirit" flowed (a) through the venae cavae to all the tissues and (b) through the artery-like vein (what we now call the pulmonary artery) to the lungs, for their nutrition; and (c) through invisible pores of the ventricular septum to the left side of the heart. The left heart received also air direct from the trachea and bronchi and through skin and tissues, which not only cooled the innate heat of the heart but by mixing with the blood seeping through from the right heart gave rise to the "vital spirit" which was conveyed again by ebb and flow, with synchronous diastole of heart and arteries, to the tissues, endowing them with their "vital" functions.

In the process of forming "vital spirit," attributed to a fermentative activity, fuliginous vapours arose which were carried to the lungs and tissues for excretion. The brain had its special function: from the arterial blood which is received it distilled off the "animal spirit" (the breath of the soul), which was carried along nerves for movement and sensation.

Galen's Critics

The Galenic view had its critics even before Harvey's day. That air passed direct from lungs to heart had been doubted by Leonardo da Vinci, who found it impossible to pump air into the heart through the trachea and bronchi. Vesalius, as we earlier saw, had been unable to demonstrate the septal pores. Michael Servetus had published in 1553 an earlier written work, *Restitutio Christianismi*, in which he inferred that the passage of blood from right ventricle to left "does not, as is generally believed, take place through the median wall septum of the heart, but by a signal artifice the subtle blood is driven by a long passage through the lungs." He then repeats the Galenic view that "it is mixed with the inspired air, and by expiration is cleansed from its fumes. And so at length it is drawn in a complete mixture, by the left ventricle through the diastole, stuff fit to become the vital spirit." His reasons for accepting the lesser or pulmonary circulation merit recall. He writes: "This view is confirmed by the conspicuous size of the artery-like vein which would not have been made so large and so stout, and would not discharge from the heart itself such a power of very pure blood into the lungs, for the mere purpose of nourishing these organs. Nor would the heart serve the lungs in this manner, especially since at an earlier date in the embryo on account of

the little membranes of the heart, the lungs themselves are up to the hour of birth nourished from other sources, as Galen teaches."

Columbus, too, had in 1559, in the work to which I earlier referred, rejected the view that blood passed from right to left ventricle through the septum, and he enunciated the pulmonary circuit. But he was a proved plagiarist in that he falsely claimed to have discovered the stapes which from Fallopius we know to have been earlier described by John Phillip Ingrassias, of Palermo; and he may well have seen a copy of the *Restitutio* of Servetus and claimed the discovery as his own.

It was Andreas Caesalpinus, professor of medicine at Pisa, great both as scholar and as disputant, who came closest to Harveian views in his opposition to Galen, and he incorporated his conclusions in two books. In the first, *Quaestiones Peripateticae*, published in 1571, he states, from a study of the valves of the heart and their directional opening and closing, that the heart in systole propels blood into the aorta and pulmonary artery, and in diastole receives blood from the venae cavae and pulmonary vein. And again, in his *Quaestiones Medicae* in 1593, he appears to have recognized that blood flows from arteries to veins, and along the veins to the heart. He noted also that veins when ligated swell on the far side—"a fact well known by experience to those who let blood"—and he recognized that this was in opposition to Galenic teaching. But, though Caesalpinus clearly formulated the pulmonary circulation and groped after a systemic circulation, he was making assertions unsupported by careful observation or experiment; and his views did not influence the teachings of his contemporaries.

Harvey in Padua

Whatever contributions da Vinci, Vesalius, Servetus, Columbus, Caesalpinus, and even Fabricius may have made to the idea of the circulation of the blood, they are insignificant when compared with Harvey's. He stands alone by virtue of his *methods* rather than by his *discovery*, great as were to be the repercussions of that discovery on the growth of scientific medicine.

But, given that he was a man of genius, how was his mind prepared for the rich harvest it was later to yield? The field was ploughed and the seeds first sown in the University of Padua, which as late as 1670 was described as "the Imperial University for Physic of all others in the world." It played an impressive part in freeing Harvey's mind, for of all European universities it was the least shackled in the fetters of authoritarianism. It had fallen under the rule of Venice in 1404, and Venice was at that time and for long after the most anticlerical State in Europe. Experimental and quantitative methods of investigation were there early introduced into the physical and biological sciences, and it is not surprising that in a university so highly secularized as Padua doubts were early harboured about the part which final causes play in natural philosophy. The question "How?" assumed greater importance than "Why?"; and the answer to "How?" in a university which sheltered Copernicus and Galileo at important periods of their lives had naturally a large mechanical bias. Thus it was that Harvey, in contrast to his predecessors and indeed contemporaries, professed both to learn and to teach anatomy, not from "books but from dissections; not from the positions of philosophers but from the fabric of nature"; and for him the heart "is in a piece of machinery in which, though one wheel gives movement to another, yet all the wheels seem to move simultaneously."

That Harvey should turn to the heart for his major interest can occasion no surprise. The pulse and breathing were problems of movement in living organisms suggested by the kinetics of matter which he had heard discussed at Padua. Moreover, with Aristotle, he regarded (as he writes in his introduction to *De Motu Cordis*) "the heart of animals as the foundation of their life, the sovereign of everything within them, the sun of their microcosm, that upon which all growth depends, from which all power proceeds." I suspect that this panegyric was influenced, however, by the need for an appropriate analogy with which to dedicate his work to his patient and monarch, that "most illustrious and indomitable prince, Charles, King of Great Britain, France, and Ireland, Defender of the Faith."

Whilst Harvey was in residence in Padua, Fabricius, his master and friend, was perfecting his knowledge of the valves of the veins which he was to record in 1603 in the small treatise *De Venarum Ostiolis*. This must have excited Harvey's interest, though there is nothing to show that he appreciated at that time the significance of the direction of the orifices of these valves or that he rejected Fabricius's interpretation of their function on the basis of Galenic doctrine—namely, that they were there to slow the flow and check the accumulation of large quantities of blood at the periphery.

Harvey's predecessors had known that when veins were cut or ligated blood flowed out of them in the direction of the heart, but so attached were they to Galenic doctrine that they explained it as "blood behaving irregularly when tortured by such experiments, rushing off in the wrong direction like a fluttering, frightened hen." Osler quotes Boyle as saying that in the only conversation he ever had with him Harvey acknowledged that a study of the valves of the veins had led him to the discovery of the circulation of the blood. Yet I suspect that a chance observation was the spark which set alight the conflagration which consumed Galenic doctrine, an observation to which no author seems hitherto to have drawn attention.

In order to demonstrate that the arteries were filled with blood and not air, as Erasistratus had maintained in naming them, Galen had carried out a simple experiment. "If you include a portion of the artery between two ligatures," he wrote, "and slit it open lengthways, you will find nothing but blood"; and, as Harvey comments, "thus he proves that the arteries contain blood only." But Harvey went further: "We too may be permitted to proceed by a like train of reasoning: if we find the same blood in the arteries as in the veins, after having tied them in the same way, as I have myself repeatedly ascertained, both in the dead body and in living animals, we may fairly conclude that the arteries contain the same blood as the veins, and nothing but the same blood."

Is it not likely that this observation started a train of reasoning in Harvey's mind? He had been tutored in the Galenic teaching that the veins are filled with a crude blood containing "natural spirit" and the arteries with a lighter blood replete with "vital spirit"; yet Harvey believed that the blood he saw in the arteries and veins was the same—an inaccurate observation when critically assessed, but surely the most probable source of the inference that, since the blood is the same, there must be continuity or communication between arteries and veins. With this in mind Harvey then sought for significant facts, but before so doing he disposed of certain hypotheses by revealing their inconsistencies with known facts; indeed, the introduction to *De Motu Cordis* is a model critical review of past work. There is no doubt that in Harvey's mind the

idea of the circulation was formulated early, for in his notes to his Lumleian Lectures at the College of Physicians in April, 1616, he writes of it in a curious mixture of Latin and English:

"constat per fabricam cordis sanguinem per pulmones in Aortam perpetuo transferri, as by two clacks of a water bellows to rayse water constat per ligaturam transitum sanguinis ab arteriis ad venas unde perpetuum sanguinis motum in circulo fieri pulsu cordis." ("It is proved through the structure of the heart that blood is constantly transferred through the lungs into the aorta, as by two clacks of a water bellows to raise water. It is proved by ligatures that blood passes from arteries to veins whereby the heart's beat produces a perpetual motion of blood in a circle.")

Mechanics of the Heart

With the bare idea of a circulation in his mind he sought for significant facts, but he sought them in relation to a significant question, which merely to ask, let alone to answer, was a remarkable innovation, for in the true spirit of science he isolated from a mass of irrelevant considerations the one which was amenable to scientific treatment—namely, the mechanics of the heart. His predecessors had asked, "Why?" about the heart and concerned themselves with "heat, spirit, and perfection," which, as Harvey rightly said, must be "decided upon other grounds." Harvey asked, "How?" and only occasionally did he deviate from this question.

The first task of the scientist is to find how he may make his most significant observations, and what material is best suited for his task—a point illustrated in recent years, for example, by the use of the giant axon of the squid in experiments on nerve conduction, and by T. H. Morgan in selecting the fruit-fly, *Drosophila*, for his genetic studies because of its rapid reproduction and its clearly defined chromosomes. Now, Harvey writes:

"When I first gave my mind to vivisections, as a means to discovering the motions and uses of the heart, and sought to discover these from actual inspection, and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Fracastorius, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that; the diastole the same; and then everything was reversed, the motions occurring, as it seemed, variously and confusedly together. My mind was therefore greatly unsettled, nor did I know what I should myself conclude, nor what believe from others. I was not surprised that Andreas Laurentius should have written that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle."

But Harvey was not defeated, and he found his suitable material in cold-blooded animals with slowly beating hearts and in the dying warm-blooded animal when the heart begins to flag.

Harvey's Test of his Views

Between the crude formulation of his hypothesis and the publication of his *De Motu Cordis* Harvey was engaged in collecting data from human anatomy (for he held that design indicated function), from comparative anatomy, from embryology, from animal experiments, and from clinical observations which would test his views. Only a few examples can be quoted, but especially significant are his observations on the structure of the valves of the veins and on the arrangement of the circular and longitudinal fibres of the heart; on his condemnation of those who "confine their researches to the human body alone, and

that when it is dead," and who "upon the ground of one particular proposition should proceed to draw general conclusions." So he turned to comparative anatomy and embryology, and thus he argues from the fishes with a single ventricle and no lungs, through amphibians with lungs and a single ventricle, and the human foetus with its unidirectional foramen ovale and ductus arteriosus, leading to the probability that in warm-blooded animals the passage from vena cava to aorta is accomplished through the substance of the lungs.

His experiments included the application of ligatures in the living animal, perfusions, and experimental observations on man, including those illustrated in *De Motu Cordis* which revealed with certainty the centripetal one-way flow of blood in the veins and the absolute bar afforded by the valves to a peripheral venous reflux. His clinical observations were accurate (for example, he knew of the insensitivity of the heart muscle to pricking and pinching), and they were apposite to his thesis. He denied that the arterial wall itself originated the pulse, because false aneurysms pulsed though they had no arterial covering, and because blood spurts in regular jets out of even the calcified artery, whose rigid wall prevents pulsation. He calls in evidence of the circulation the general spread in the body of "contagions, poisons from wounds, bites of serpents and rabid animals, lues venerea, and the like" even though "the part first infected remains sound." Indeed, so clear are his observations and so cogent his reasoning that I would insist on every medical student having *De Motu Cordis* as a set book for his preclinical studies.

Quantitative Considerations

But there is yet another form of argument which Harvey introduces, as Singer (1928) emphasizes, for the first time in biological literature; that is, the *quantitative* considerations which would be expected to flow from the Galenical theory.

"Let us assume," writes Harvey, "whether arbitrarily or from experiment, the quantity of blood which the left ventricle of the heart will contain when distended to be, say, two ounces, three ounces, or one ounce and a half—in the dead body I have found it to hold upwards of two ounces. Let us assume further, how much less the heart will hold in the contracted than in the dilated state; and how much blood it will project into the aorta upon each contraction—and all the world allows that with the systole something is always projected, a necessary consequence demonstrated in the third chapter, and obvious from the structure of the valves; and let us suppose as approaching the truth that the fourth, or fifth, or sixth, or even the eighth part of its charge is thrown into the artery at each contraction; this would give either half an ounce, or three drachms, or one drachm of blood as propelled by the heart at each pulse into the aorta; which quantity, by reason of the valves at the root of the vessel, can by no means return into the ventricle. Now in the course of half an hour, the heart will have made more than one thousand beats, in some as many as two, three, and even four thousand. Multiplying the number of drachms propelled by the number of pulses, we shall have either one thousand half ounces, or one thousand times three drachms, or a like proportional quantity of blood, according to the amount which we assume as propelled with each stroke of the heart, sent from this organ into the artery; a larger quantity in every case than is contained in the whole body! In the same way, in the sheep or dog, say that but a single scruple of blood passes with each stroke of the heart, in one half-hour we should have one thousand scruples, or about three pounds and a half of blood injected into the aorta; but the body of neither animal contains above four pound's of blood, a fact which I have myself ascertained in the case of the sheep.

"Upon this supposition, therefore, assumed merely as a ground for reasoning, we see the whole mass of blood passing

through the heart, from the veins to the arteries, and in like manner through the lungs.

"But let it be said that this does not take place in half an hour, but in an hour, or even in a day; any way it is still manifest that *more blood passes through the heart in consequence of its action, than can either be supplied by the whole of the ingesta, or than can be contained in the veins at the same moment.*"

And so by advocacy, cogent and unassailable, we too must conclude with Harvey that:

"Since all things, both argument and ocular demonstration, show that the blood passes through the lungs and heart by the force of the ventricles, and is sent for distribution to all parts of the body, where it makes its way into the veins and porosities of the flesh, and then flows by the veins from the circumference on every side to the centre, from the lesser to the greater veins, and is by them finally discharged into the vena cava and right auricle of the heart, and this in such a quantity or in such a flux and reflux thither by the arteries, hither by the veins, as cannot possibly be supplied by the ingesta, and is much greater than can be required for mere purposes of nutrition; it is absolutely necessary to conclude that the blood in the animal body is impelled in a circle, and is in a state of ceaseless motion; that this is the act or function which the heart performs by means of its pulse; and that it is the sole and only end of the motion and contraction of the heart."

Passages from Arteries to Veins

Yet there was a missing link in Harvey's demonstration—the channels of communication between arteries and veins, which were to be revealed by Malpighi in 1661. With the aid of his microscope he was to see the capillaries of the frog's lung. But in retrospect it seems strange to us that *Harvey did not himself perform a simple yet crucial perfusion experiment to show that the passages from arteries to veins are direct and continuous.* He was not unacquainted with the technique of perfusion, and had used it to demonstrate that water injected into the pulmonary artery passes into the left ventricle. In a letter to Paul Marquand Slegel, dated "London, this 26th of March, 1651," Harvey, then over 70 and racked with gout, relates yet another experiment devised to demonstrate the pulmonary circulation.

"Having tied the pulmonary artery, the pulmonary veins, and the aorta, in the body of a man who had been hanged, and then opened the left ventricle of the heart, we passed a tube through the vena cava into the right ventricle of the heart, and having at the same time attached an ox's bladder to the tube, in the same way as a clyster-bag is usually made, we filled it nearly full of warm water and forcibly injected the fluid into the heart, so that the greater part of a pound of water was thrown into the right auricle and ventricle. The result was that the right ventricle and auricle were enormously distended, but not a drop of water or of blood made its escape through the orifice in the left ventricle. The ligatures having been undone, the same tube was passed into the pulmonary artery and, a tight ligature having been put round it to prevent any reflux into the right ventricle, the water in the bladder was now pushed towards the lungs, upon which a torrent of the fluid, mixed with a quantity of blood, immediately gushed forth from the perforation in the left ventricle; so that a quantity of water, equal to that which was pressed from the bladder into the lungs at each effort, instantly escaped by the perforation mentioned. You may try this experiment as often as you please: the result you will still find to be as I have stated it."

The last sentence makes it clear that Harvey was well aware that consistent uniform results must ensue if the experiment is valid for scientific induction.

There are errors, many errors, in both observation and interpretation in *De Motu Cordis*, and as a speculator on ultimate causes Harvey was not far removed from his

physiological predecessors for two thousand years (see Curtis, 1915). These do not detract from Michael Foster's (1901) appraisal of this work "His wonderful book," wrote Foster in his *History of Physiology*, "or rather tract, for it is little more, is one sustained and condensed argument, but an argument founded not on general principles and analogies but on the results of repeated 'frequent appeals to vivisection' and ocular inspection. He makes good one position, and having done that advances on to another, and so marches victoriously from position to position until the whole truth is put clearly before the reader, and all that remains is to drive the truth home by further striking illustrations."

But I would suggest that Harvey's book did more than achieve its *scientific* purpose. It attacked and laid bare the shallowness of the conventional mind; it pressed for the acceptance of rational truth even if the apparent foundations of revelation and authority be undermined in the process. If man is a reasoning being, it taught, he must pursue his findings wheresoever they lead him in order, as Harvey himself avowed, that "what is true may be confirmed, and what is false set right by dissection, multiplied experience and accurate observation." If I am to describe the mind of Harvey I can do no better than borrow the words of one of Harvey's patients, Francis Bacon, of whom Harvey had cynically remarked that "he wrote philosophy like a Lord Chancellor," and who, in his *Proemium*, thus flatteringly describes himself—"A mind nimble and versatile enough to catch the resemblance of things, which is the chief point, and at the same time, steady enough to fix and distinguish their subtle differences; . . . endowed by nature with the desire to seek, the patience to doubt, fondness to meditate, slowness to assert, readiness to reconsider, carefulness to dispose and set in order; and neither affecting what is new nor admiring what is old, and hating every kind of imposture."

Conclusion

In the turmoil in which we who spend most of our days and nights in practising and teaching medicine and in trying to fashion its future there is a great need for the refreshing, calming, and cultural diversion of the literature and scholarship of our calling. The time which we can devote to reading, research, and meditation becomes less as the complexity of our lives increases. Yet there is nothing, even in the demands of our many-sided vocation, that compels us to abate our interest in our ancient masters, and especially in the workings of their minds. I have taken advantage of the opportunity which this occasion offers to express opinions and formulate judgments upon a historical and philosophical theme. I pray my critics will in justice remember, what I shall certainly not forget, that these are necessarily the work not of an expert but of an amateur

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