The Harveian Oration

ON

THE DEBT OF MEDICINE TO THE EXPERIMENTAL METHOD OF HARVEY.

DELIVERED BEFORE THE ROYAL COLLEGE OF PHYSICIANS
ON OCTOBER 18TH, 1926,

BY

SIR JOHN ROSE BRADFORD, K.C.M.G., P.R.C.P.,
M.D., D.Sc., F.R.S.

SIR HUMPHRY ROLLESTON and Fellows of the College: I have, as a first duty, to tender to you, Sir, my thanks for entrusting to me the honourable office of delivering, in accordance with the wishes of Harvey, the annual oration commemorating the benefactors of this College. It is a striking tribute to the greatness of Harvey that notwithstanding the lapse of time and notwithstanding the great benefactions received by this College from other pious founders from remote to very recent times, this oration has become more and more a tribute to the unparalleled services rendered by William Harvey, not merely to this College, but to mankind. This result is, no doubt, in part dependent upon the greatness and brilliance of Harvey's discoveries as to the existence of the circulation, and the unravelling of some of the mystery surrounding the heart beat, but it is due still more to the great conceptions he formed as to the proper methods to follow in attempting to understand the facts of Nature. It is probable that it is no exaggeration to say that the most valuable benefaction this College has ever received is Harvey's exhortation to the Fellows and Members of the College to search and study out the secrets of Nature by way of experiment, coupled with the example afforded by his own life's work.

Harvey, like most great men, was of so modest a character that he had little idea of the great place he himself would occupy in the history of science, and this exhortation of his, together with his desire that the Fellows of his beloved College should continue in mutual love and affection amongst themselves, was doubtless only an expression of his wishes for the successful development of this College, and thus indirectly of medicine as a whole.

Successive Harveian orators during the last 200 years have striven to honour his memory and his services in a series of orations dealing with different aspects of his life and work, or with the progress of scientific knowledge since his day, and the influence of this on mankind and on civilization. Scholars have brought before us the state of medical knowledge in his time, and have also vividly portrayed the mode of his life and the conditions under which he worked. We know, thanks to their labours, what manner of man he was, and what was the influence on him of the disturbed time in which he lived. These are matters of perennial interest, and his example of philosophic calm under conditions of great national anxiety cannot fail to be an example to all for all time.

Harvey's life work illustrates, in a remarkable manner, the many-sided character of medicine when considered as a whole, and how the men who promote its advance are often men of wide attainments in many branches of knowledge. His greatness is shown by the fact that, notwithstanding the conditions prevalent in his time, he was able to so free himself from the traditions of the past and the influence of authority and dogma as to lay the foundations of modern science by his insistence on the fundamental importance of experiment and of accuracy of observation. Different sciences afford different opportunities for the use of experiment as a means of discovery, but the founder of the science of physiology had, in his field of study, unlimited scope for this method of advance, as prior to Harvey but little had really been effected by experiment, although it had been used by many, even in ancient times. We reverence Harvey, not only for the foundation of physiology as a science and for showing how its progress is

to be effected, but, as Fellows of this College of Physicians, for making plain the intimate relationship which must exist between medicine, both as a science and as an art, and physiology. Harvey combined in himself the physician and the physiologist. This, with the lapse of time and the progress of knowledge, has become increasingly difficult, and this difficulty is one common, to a greater or less extent, to all branches of knowledge, but more especially when a field of work includes a science and an art. There is some danger of a catastrophe akin to that of the Tower of Babel involving the more narrow specialists of these later days, owing to the rapid development of the new languages required to describe the results achieved in some departments of knowledge.

Although the great developments of knowledge make it more and more difficult for any one individual to have anything approaching complete and detailed information even in one branch, and still more so in more than one, yet there should be, on the other hand, less difficulty in acquiring a fuller grasp of fundamental principles. After all, the magnitude of any scientific discovery is to be measured rather by its influence on the genesis of these fundamental principles, often formulated as so-called laws, than on the mere solution of the particular problem investigated. The so-called physical sciences have had in the past an advantage in this respect over the biological, in that the mathematician, the astronomer, the physicist, and the chemist have had more in common than the morphologist, the physiologist, and the pathologist. But the progress of knowledge is gradually effecting a change in this respect, and it is becoming more and more easy for the followers of the different sides of biology to meet on common ground. It is not only becoming increasingly difficult to draw a hard and fast line between the problems investigated by the physiologist and those of the pathologist, but even the morphologist, whether zoological or botanical, is now pursuing with zeal, purely physiological methods in order to further his science, even on its morphological side. Harvey, as a physiologist, would have rejoiced at these developments if he had known that the colour of an animal and even its form and mass, to say nothing of its sex, were under the influence of, and capable of being modified by, the functional activity of certain glands.

Harvey was, as just stated, a physiologist and a physician who practised his profession, and was daily face to face with urgent and distressing problems, as we all are. We all agree that our improved methods of coping with these problems are ultimately dependent upon his great discovery; and it is a trite observation that medicine must be founded on physiology, and that the study of healthy processes must precede that of morbid ones.

But the debt of the art of medicine to the science of physiology is greater than this, and the value of physiology to medicine is not to be measured merely by the direct practical utility, great as it may be and has been, of individual physiological discoveries, even when these are, like Harvey's, of a truly epoch-making character. The art of medicine must of necessity be reared as a superstructure on the science of medicine unless it is to be indistinguishable from mere empiricism; and the science of medicine, like other sciences, can only be advanced by observation and by experiment. When we reflect on what has been done in the past by the observation of our predecessors, from the most remote to recent times, we cannot fail to record our admiration of the great intellectual qualities that these observers must have possessed to have enabled them to achieve the results obtained. The knowledge of the natural history of disease is a subject of vast importance to the art of medicine, and one that can only be acquired by observation. Nevertheless, as the late Lord Moulton has well said, when we are reduced to observation, science crawls. If we are to treat a disease successfully we must know a great deal more about it than its natural history—that is, its course when it is allowed to pursue its career unchecked. Experiment as a method of furthering the progress of any given science is capable of achieving great results, and—again to quote Lord Moulton—"As soon as you can bring experiment to bear upon a subject you are free, but as long as you can merely observe, your progress is slow. The reason is that experiment is like

cross-examination: you can put the question you want, and Nature always answers it."

Experiment, however, is not equally applicable to all sciences, and from the circumstances it is obvious that in medicine its scope must necessarily be limited. This is, doubtless, one of the reasons why the progress of the science of medicine has been slower than we could wish, but another is to be found in the profound complexity of the problems that have to be solved. This complexity is not only obvious in the consideration of broad general questions, such as the nature and causes of disease, or of individual diseases, but applies often to our daily work of the mere recognition of the nature of the malady in an individual patient. We are often baffled by the complexity of this, and nothing is more common in the experience of practitioners of our art than to find suddenly that what seems simple and obvious is by no means so. The complexity of the problems daily before us is apt to react on the mind of the physician and produce, even in this rather materialistic age, a tendency to regard the phenomena of life, and especially of disease, in an individual, as scarcely subject to the same rigid laws of cause and effect that we all recognize as binding in the physical world. If this belief, even in a modified and attenuated form, is once entertained, it causes a looseness of thought that is most inimical to the real progress of knowledge in medicine; and it is here that the study of physiology and the work of Harvey have rendered the greatest service to medicine. Harvey, by experimental work, by clear thinking, and by accurate observation, laid the foundations of physiology as a science, and those who have followed in his path have continually added to his foundations, so that now no one questions its right to rank as a science, and no one disputes the fundamental proposition that in physiology, as in physics, like causes produce like results. If the excitation of the vagus nerve causes stoppage of the beat of the heart in one case, and acceleration in another, or possibly in the same animal at another time, it is not, as Magendie thought, due to the variability of vital phenomena as such, but is due, as Bernard, the great exponent of "determinisme," taught, to the conditions not being really the same. It is perhaps superfluous to mention such an attitude of mind as regards physiology, but possibly, even now, we are not altogether free from such a way of looking at things in human medicine, and Magendie's way of thinking has, perhaps, some followers. It is, of course, much more simple to render the conditions the same in physiological observations than in medicine, although even in physiology it is often difficult, as the discordant results of many competent observers show. In human medicine the difficulties are infinitely greater, and hence we should regard the results already achieved as a most remarkable monument to the acumen of clinicians, who must, in order to have obtained the results we are familiar with, have been possessed of exceptionally sound scientific methods of observation.

The history of the progress of our knowledge of medicine affords countless instances of the extreme accuracy of observation possessed by the foremost clinicians of the day, but although their observations were often excellent and their descriptions leave little to be added to, their explanations of the observed facts were not of the same value. Variations observed in the course of maladies were often explained in an artificial and non-scientific manner, owing to the want of appreciation of this law that like causes produce like results, and that if the results are different in apparently similar cases, the real explanation is, not some variability on the part of Nature, but that in reality, although the cases seem similar, there is some hitherto undiscovered difference. One type of thinking closes the door to further work and real progress; the other is one of the most fertile causes of discovery. Shrewd clinicians observed, long ago, that some cases of typhoid fever lasted four weeks, others were said to abort in two weeks; hence it was inevitable that a certain vagueness and uncertainty of belief should be entertained with regard to the action of the virus of this disease; and it is difficult not to think that such beliefs react on the mind and influence profoundly our outlook on problems that beset us, and on our methods

of coping with them. The progress of inquiry shows that the differences in the duration of the febrile period of typhoid fever are, at any rate sometimes, due to the differences in the organisms causing the disease, and we now talk of the enteric group, including both typhoid and paratyphoid bacilli. It is quite true that this increase of knowledge does not settle the problem, as there still remains the question of the relation and origin of the paratyphoid group of organisms; but this progress of knowledge has had the effect of demonstrating that the duration of so-called typhoid fever is not a matter, so to speak, of chance, but depends—or may depend—on the specific nature of the organism causing the illness. The fact that one organism causes an illness of four weeks' duration, and another quite distinct but allied one causes an illness of two weeks' duration, is a very different matter, from the point of view of scientific medicine, from a statement that the illness of typhoid fever may last either four weeks or two weeks. The attitude of mind produced by one belief is calculated to cause a certain bewilderment of feeling, and one both of hopelessness and helplessness of arriving at any definite attainment; the other encourages the inquirer to hope that long-continued patient work may reveal, at any rate in part, the mystery, and show that the phenomena of disease, complex as they are, are nevertheless amenable to the same laws and conditions as those governing the biological and physical worlds. Further, the apparent variability of disease becomes, instead of an impediment, a spur to further inquiry and a stimulus to research, and such inquiries are often rewarded by discoveries that add a definiteness to our knowledge that is of the greatest possible value. It would not have been possible for this attitude of mind to be developed in relation to the problems of medicine unless the study of physiology, and of vital processes generally, had been placed on the surest foundations by Harvey by the use of the experimental method, as it is this method, in its many developments, that has enabled us to see that in living things like causes produce like results, and that the exceptions are apparent only, not real.

Further, in the past there has been much theorizing, both in physiology and in medicine, often on very insufficient data. The experimental method has afforded a very certain and conclusive test of the inaccuracy of much work of this nature, and such results, important as they are, are, nevertheless, of a negative character, and have sometimes led to the belief that there is some antagonism between experimental inquiry on the one hand, and the result of observational methods on the other. Doubtless much loose thinking and many erroneous deductions may occur as the result of the direct and immediate transference of experimental animal results to the explanation of the phenomena of human disease, but this, again, arises from neglect of the fundamental principle that the conditions in the two cases are not alike, and also that we constantly think the phenomena of disease to be much simpler than they really are, and are perpetually hankering after cut-and-dried explanations of what are, in reality, exceedingly complex phenomena. Far be it from me to minimize the direct and immediate gains to medical knowledge and to medical practice that have resulted from individual discoveries in experimental physiology, as, for example, the notable discoveries as to the functions of the thyroid and its secretion, and many others. Harveian orators have dealt with these in such an able fashion that it is superfluous to dwell on them, and I would urge to-day that perhaps of even greater importance is what I may call the general effect on medicine of the development of physiology as an exact science, and the influence that this will have, in the course of time, in making the science of medicine as exact and as worthy to be deemed a science as any other branch of natural knowledge. Clinical observation, when carried out by a great mind and in strict accordance with scientific method, has achieved most striking results—results which are entitled to our highest admiration when, as already stated, we consider the extreme difficulty of the problems and the scanty means at our command to solve them. Often, however, the results, great as they are and accurate as they are, lack the precision of detail that sometimes the experimental method is capable of supplying, and then

the experimental method is responsible for the completion of that which has been truly founded by clinical observation.

A striking illustration of this is afforded by the history of the discovery of what is known, shortly, as localization of function in the cerebral cortex. Here the fundamental work was done by that great clinician and profound thinker Hughlings Jackson; and when we reflect that by the study of a very limited number of cases of hemiplegia and of a series of cases of fits, and by the careful correlation of the phenomena observed during life with the lesions found after death, he was able to arrive at the conclusion that voluntary movements were represented in a certain area of the cortex, we have a magnificent example of the accurate and far-reaching deductions that may be drawn by a master mind. Nevertheless, even in such a case as this, the subsequent experimental work was able to give a precision and supply a mass of detail that greatly enhanced the original fundamental discovery. So much is this the case that many came to think that cerebral localization had been discovered solely by the experimental method, whereas the fundamental facts were really the outcome of clinical observation.

The greatness of Jackson's conceptions is well seen in the fact that he recognized that cortical localization was concerned with the representation of movements, and not of muscles, and, indeed, with a special kind of movement—namely, voluntary movement. In the earlier days of experimental work on this subject some confusion was introduced by certain observers falling into the error of looking for so-called centres for the representation of muscles; and it was only later that inhibition of antagonistic muscles, as well as contraction of other muscles, was also recognized as a matter of cortical representation. Anyone who has actually seen the movements called forth by the electrical excitation of the cerebral cortex will experience quite different mental impressions from those he will have felt by watching an epileptiform seizure, perhaps confirmed subsequently by seeing the position and nature of the lesion responsible for it. In the one case there is a definiteness and certainty that in the other is apt to be replaced, at the best, by a feeling of high probability. And yet, if we are to succeed in our endeavours to cope with the facts of nature, we must aim at possessing certain, and not probable, knowledge. It may be that this is often, in our work at present, unattainable, but it is the great thing to aim for in all our work. Perhaps some of our mistakes in the practice of our art arise from not clearly differentiating between what we really know and what is only probable; and sometimes, perhaps, we speak with an apparent certainty about matters that are not, in our present state of knowledge, really as certain as we would like them to be. The touchstone of the experimental method of Harvey is a useful corrective whenever and wherever it may be applied to questions of this nature. The modern physiology of the central nervous system, investigated by the experimental method, affords a striking example of the extraordinary precision of knowledge that may be obtained by the use of this method, and, thanks to it, a flood of light has been thrown on the complex problems of the mechanisms involved in posture, equilibration, co-ordination of movement, and perhaps on the still more complex mechanisms associated with the highest functions of the brain. Nevertheless, in an oration associated with the name of Harvey it may perhaps be possible to illustrate the principles of the profound influence exerted by experimental physiology on medicine by seeking an example in that branch of physiology so closely associated with Harvey—namely, the beat of the heart.

I am not so much concerned to-day with giving a record of individually important discoveries that have been made from time to time whereby our knowledge has been increased and our power of recognizing, and treating with some success, individual diseases, has been extended, but rather with the nature of the conceptions we form of the phenomena of heart disease as we see them clinically in the light of the exact modern physiology of the heart, as revealed by the use of the experimental method of Harvey.

The beat of the heart, from the earliest times to the

present, has been looked upon as emblematic of life, by poets, philosophers, and thinkers, and has always had an element of mystery and awe attached to it. No physician will deny that in disease there is much of apparent mystery in what may be called the natural history, or, if you will, the clinical course, of heart disease; further, that the clinical picture presented by individual cases, even where relatively simple lesions are present, is one with much individual variety, that the phenomena are complex and difficult to analyse, and hence there has been much uncertainty of opinion, both in diagnosis and in treatment. Such differences of opinion existed, not only with reference to questions of interpretation or explanation, but even in reference to what we should now regard as mere questions of fact. There must be some present here to-day who will remember a controversy as to the actual time of occurrence of certain well known murmurs associated with a common valvular disease. When the methods available for clinical observation led to such remarkable differences of opinion among physicians of recognized skill and ability, it is not strange that many keen and able minds should have felt that the problems of medicine were so difficult and so complex that it was beyond their power to really solve them with the means at their disposal; and this attitude of mind is one very inimical to the progress of our science and our art. The method of Harvey of seeking the secrets of nature by way of experiment shed a flood of light on the problems that had baffled those who confined their methods to observation. Not only was this the case, but what is of supreme importance to recognize is the fact that many of the men who in recent times remodelled our conception of the nature of cardiac disease by the fundamental character of their discoveries of the nature of the cardiac beat were men who devoted their lives to the investigation of the facts of nature with no other object in view than that of adding to knowledge for its own sake. Great as their discoveries were, they had no idea of their practical bearing on heart disease, and some of them, indeed, were men who had no knowledge of heart disease as such, and never undertook their work with any idea of adding to our knowledge of heart disease. Further, the discoveries of the pure clinicians, remarkable as they were for the acuteness of their observation, were largely due to making use of the methods and apparatus of the laboratory in the investigation of clinical problems. The polygraph and the electro-cardiogram gave certain and accurate information where previously other methods of observation had led only to the dissemination of views, explanations, and theories of varying degrees of probability. It is one thing to be able to actually demonstrate beyond cavil that the auricle of the human heart in a living patient is in the state known to us as that of fibrillation, and another to talk of lack or failure of compensation, with no clear mental picture of the meaning of the words used. Anyone who has seen a fibrillating auricle in the living animal in the laboratory has a mental picture that is not easily forgotten—a picture of a singularly concrete and definite character, with an element of reality about it that cannot fail to be a useful reminder to him when he stands at the bedside of a patient suffering from the effects of this condition. It is, of course, quite true that the acute clinical observers of the past had recognized the clinical picture produced by this condition, and had described quite accurately some of its manifestations. We can but admire their acumen, but they were necessarily unable, owing to the lack of physiological knowledge and the want of accurate methods, to determine the true cause of the symptoms and to correlate the lesion with its results. Hence the inevitable temptation, to some minds, to speculate and theorize vaguely instead of confining themselves to mere observation, and hence the further elaboration of more and more speculation and ultimately much confusion as to what was speculation and what was firmly established by observation. This has often been most harmful to the real progress of medicine, owing to the development of loose methods of reasoning, and the frequent impossibility of bringing the matter in dispute to a decision by the crucial method of experiment.

The history of the development of our knowledge of fibrillation is singularly interesting as affording a striking example of the experimental method having substituted

a clear, definite, and easily proved condition for what was previously a most vague, indefinite, and even intangible conception. But the history of the discovery of fibrillation is even more interesting and important from another point of view, and teaches a lesson of supreme importance to those who are always insistent on investigation being directed to some immediate practical aim. Fibrillation was discovered by a physiologist who was attempting to ascertain where the beat of the heart had its origin; in other words, whether the heart contained a so-called centre from which the beat arose. In the course of his inquiries he found that on stabbing the heart with a fine needle a condition of what we now call "fibrillation" ensued. Sometimes this followed a single puncture, sometimes many were required. Later it was found that faradization of the exposed heart with a current of moderate intensity produced the same result. Sometimes the condition of fibrillation was transitory, in other cases it was permanent; the heart did not recover, and death ensued.

It is unnecessary to-day to go into all the details of the many subsequent investigations on this interesting subject; all that it is necessary to point out here to-day is that this condition of fibrillation was a discovery of experiment produced by such crude and simple methods as repeatedly stabbing or faradizing the exposed heart. I can well remember that it was a common method, some forty years ago, to kill an animal at the end of a physiological experiment by faradizing its heart, and so producing heart delirium or fibrillation. No one who saw fibrillation would have suspected that it was a common condition in human heart disease, and that almost every hospital ward had cases suffering from it, either temporarily or even for long periods of time. It might, indeed, have been argued very plausibly that it was inconceivable that such crude experiments could have any possible use or bearing on vital processes, and still less on human heart disease; and yet we see that the exact opposite is the case, and that, crude as the experiment was and remote as was the connexion between the normal dog's heart and human mitral stenosis, and although the mind of the investigator had no thought of heart disease in his inquiry, nevertheless this crude and apparently idle experiment was of the utmost significance for the intelligent understanding of one of the commonest forms of heart disease.

Another lesson may also be learned from consideration of these results, and that is the reciprocal influence of physiology on pathology, and of pathology on physiology, and how extremely artificial is the boundary between these two departments of knowledge, and how, if we are to have an adequate conception of even relatively simple vital processes, it is necessary to study them with the broadest possible outlook. A pure and rigid physiologist, studying only the normal activity of the heart, would scarcely be prepared to admit that the circulation, in a highly organized mammal, could be maintained when the rhythmical activity of the heart was so disorganized that auricular fibrillation was present. Nevertheless, clinical medicine proves that, even in man, such is the case. Surely such facts as these are of the utmost value, not only in our immediate daily work and in our attempts to treat disease and relieve suffering, but still more so in our appreciation of the nature, the complexity, and, more especially, the adaptability of the activities of living matter. Marvellous as are the various methods by which a circulation sufficient for the needs of the normal animal is effected—and this is well shown by the study of comparative anatomy and comparative physiology—the marvels of the relative efficiency of the disordered circulations of heart diseases are not any less. Certainly the study of these conditions not only widens our conceptions, but is also a useful incentive to our therapeutic efforts when a more limited and narrow belief might lead us to be less hopeful as to the possibility of procuring relief or cure in these conditions.

The history of the study of the phenomena of fibrillation has been cited as an illustration of the very close relationship that exists between experimental science and the daily work of the physician, especially if we wish to found our art on a secure basis of science, and if we desire to eschew empiricism where this is possible. The same illus-

tration may be found equally well in the history of the development of our knowledge as to the nature of the normal beat of the heart and of its disordered action in disease. Here also we see the extraordinarily close connexion between pure physiology on the one hand, and the interpretation of the clinical symptoms of heart disease on the other. It may seem, at first, strange to assert that our present conceptions of the fundamental facts in the pathology of heart disease owe their origin to researches conducted by a physiologist on the nature of the muscular contractions in the umbrella of the jelly-fish, but it is really an unexaggerated and plain statement of fact. Romanes, in the seventies, carried out a series of very beautiful but very simple experiments on the nature of the rhythmical contractions of the umbrella of jelly-fish, being especially interested in the question whether the wave of contraction was transmitted via the nerve plexus—that is, intermingles with the sheet of muscle in these organisms—or whether it was of purely muscular origin. He had but little apparatus beyond a pair of scissors, and, for some purposes, a simple recording lever and a smoked surface. Romanes, in the course of his work, discovered the fundamental fact that what is known as a block was a phenomenon dependent purely upon muscular changes, and due sometimes to a mere narrowing by section of the width of the muscular bridge connecting the two portions of a contracting muscular sheet. In other words, a block might be produced by local alterations in the conducting properties of the muscle itself. Further, he showed that delay in conduction could also be effected by such alterations in the muscular bridge produced by narrowing of the connexion, and that delay in transmission of a muscular wave from one part to another was not in itself any evidence of the intervention of the nervous elements. The importance to us, as physicians, of Romanes's work was that it was the origin of the slightly later and epoch-making work of Gaskell on the nature of cardiac action. We owe to Gaskell a very large part of our modern conceptions of the physiology and pathology of the heart muscle, and the bulk of Gaskell's experimental work, like that of Romanes, is beautiful in its extreme simplicity. Gaskell developed and amplified in the heart of fishes, of amphibia, and of reptiles, and especially in the heart of the tortoise, the fundamental experiments of Romanes; and, like Romanes, some of his most striking results were obtained by the use of experiment, with little more apparatus than a pair of sharp scissors. His fundamental experiment of producing a block by section of the muscle of the auricle, so as to leave only a narrow bridge connecting the sinus portion of the auricle with the ventricular portion, was performed on the heart of a skate on the deck of a yacht whilst cruising off the West Coast of Scotland, not far from where Romanes worked in his seaside laboratory on his jelly-fish.

Gaskell was the protagonist of the doctrine of looking upon the heart as a muscle and of unravelling the properties of cardiac muscle, and of correlating them with those of other muscular tissue, voluntary and involuntary. Further, he correlated structure and function, as shown by the different physiological properties of auricular and ventricular muscle associated with their different structure, and he emphasized, as also did Roy, the great part played by the auricle in the physiology of the heart beat, and its intimate association with the nervous system.

Another of Gaskell's great conceptions was that of the independent rhythm of the separate cavities of the heart and the way in which, under certain abnormal conditions, the prepotency of the sinus end of the heart might be interfered with, and an independent rhythm initiated in some other portion of the heart, as, for example, the ventricles. We owe to him the conception of the heart as consisting of a number of muscular cavities each with an independent rhythm, but the whole dominated by a prepotency at the sinus end. Nevertheless, under certain conditions this might be disturbed by the manifestation of the independent rhythm of some other portion of the heart, more especially the ventricle.

Gaskell's work on the nervous system was of such a striking character, and led to such far-reaching conclusions, that sometimes we are apt to forget that his earlier work on cardiac muscle was certainly of equal importance, and

has had a very great influence on the progress of modern pathological knowledge, especially by clarifying our ideas as to the nature and mode of origin of the cardiac rhythm. We also owe to him and to Roy the conception that the main function of the auricle is to act as a regulator of cardiac activity. As has already been pointed out, all these fundamental facts were discovered by experiment, with very simple apparatus. But in another branch of physiology to which medicine is also greatly indebted the experimental methods necessary in order to elucidate the phenomena were of a more complex character. Forty years ago electro-physiology was looked upon by many as a recondite branch of investigation which might perhaps be of interest, but which could not possibly be of any practical use. Physiologists, like Burdon-Sanderson, were engaged in the investigation of the electrical phenomena associated with the activity of vital processes, simply and solely because it was the most delicate physical method available for the investigation of such phenomena. It is a striking comment on such views as these to reflect that the study of the electro-cardiogram in the human subject and in animals was the actual means by which the condition of fibrillation was verified to exist in the human subject. Furthermore, the electro-cardiogram gives us the most definite and accurate knowledge of the real state of the cardiac muscle, as shown by the nature of its activity.

One of the most striking everyday illustrations of this may perhaps be quoted. Every practitioner is aware that in mitral stenosis under some conditions the presystolic murmur is absent and in others it is present. I know of no more striking demonstration than to be able to show, by means of the electro-cardiogram, that in the one case the auricle is contracting rhythmically, and in the other it is not. The electro-cardiogram removes all doubts and difficulties as to the interpretation of this phenomenon, and gives an element of certainty that is not obtainable by any other method. It has thus led to a precision in the interpretation of some clinical phenomena that leaves little to be desired. On the other hand, it requires an elaborate installation and a considerable amount of technical and special knowledge, and thus is ill adapted for ordinary routine observation.

Although we owe to the electro-cardiogram the complete demonstration of the existence of fibrillation in the human heart, we can recognize the presence of fibrillation by other and simpler methods. It is a common thing in the history of science that the interpretation of facts may require difficult and elaborate experimentation, whilst their recognition can be effected by the use of simpler methods. The electro-cardiogram may be useful for both purposes, but it is perhaps more especially valuable as an instrument of research when attempts are being made to ascertain the fundamental causes of certain clinical phenomena.

Another striking illustration of the profound influence of a simple experimental method of inquiry in advancing our knowledge of the nature of the heart beat, and indirectly of all vital activities, is afforded by Ringer's work on the effect of minute quantities of calcium, potassium, etc., on the heart. His discovery that the activity of a living tissue was dependent upon the presence of calcium was a great discovery, and one the full effects of which are only now beginning to be realized. He, further, had the very remarkable foresight to anticipate, in some respects, the modern doctrine of ions. Ringer's work is perhaps especially noteworthy inasmuch as it was the work of a man trained as a clinical physician, who did not embark on experimental inquiries until he had reached middle life, and then devoted himself to the investigation of what might be regarded as abstract problems of pure physiology. These observations, like those of Gaskell, were what may be called fundamental, inasmuch as they altered our conceptions of the nature of vital processes, in that they replaced what was vague and indefinite by concrete physical and chemical facts. These observers certainly fulfilled Harvey's exhortation to the letter, by searching and studying out the secrets of nature by way of experiment.

I have attempted, in this oration, to show that the pursuit of knowledge by Harvey's experimental method is not to be regarded only as a means for the acquisition of results of great value in our daily work, but also that it

is of supreme importance in laying the foundations of our conceptions as to the nature of the processes we see at work in disease, and that, thanks largely to it, we may claim that medicine has a scientific status as a branch of natural knowledge.

Hence in medicine, as in biology generally, it is true that like causes produce like results, and the successful investigator must realize the profound truth of the lines:

And Nature, the old nurse, took
The child upon her knee,
Saying, "Here is a story book
Thy father has written for thee.

"Come wander with me," she said,
"Into regions yet untrod,
And read what is still unread
In the manuscripts of God."

And whenever the way seemed long,
Or his heart began to fail,
She would sing a more wonderful song
Or tell a more marvellous tale.

British Medical Association.

PROCEEDINGS OF SECTIONS AT THE ANNUAL MEETING, NOTTINGHAM, 1926.

SECTION OF OBSTETRICS AND GYNAECOLOGY.

COMYNS BERKELEY, M.D., M.CHIR., F.R.C.P., President.

DISCUSSION ON CAUSES AND TREATMENT OF UTERINE HAEMORRHAGE WITHOUT GROSS PHYSICAL SIGNS.

OPENING PAPERS.

I.—BECKWITH WHITEHOUSE, M.S.LOND., F.R.C.S.,
Professor of Obstetrics and Gynaecology, University of Birmingham; Honorary Gynaecological Surgeon to the General Hospital; and Surgeon to the Maternity Hospital, Birmingham.

(With Special Plate.)

INTRODUCTION.

FROM the earliest times uterine haemorrhage has been the object of superstition, speculation, and theory. For almost five thousand years its discussion has occupied a prominent place both in medical and lay literature, and it appears perhaps somewhat strange in the twentieth century that so little is really known and accepted about the fundamental facts that control the function of menstruation. To review the past, with the strange myths and beliefs contained in the writings of the Sacred Law, the Ebers papyrus, Pliny, Hippocrates, Aristotle, and the Zend Avesta, is a fascinating study which, although foreign to the present discussion, serves to throw light upon some of the views and customs which even in the world of to-day are by no means obsolete. In modern times, with the sweeping away of superstition and the development of scientific investigation, our difficulties, however, have not lessened. Rather have they assumed a different aspect. During the last century a mass of heterogeneous material has been accumulating, and it is ever more and more difficult to sift the grain from the chaff. Conflicting statements and theories, many based upon insufficient data, have appeared which complicate the points at issue. Within the last few years, however, signs are apparent that at last we are nearing the edge of this morass and approaching more solid ground upon which to build a firmer edifice, based upon the truths of physiological and pathological research.

THE MENSTRUAL FUNCTION.

Any discussion upon the causation and treatment of pathological uterine haemorrhage must depend in the first place upon the views that are held with regard to the menstrual function. If we do not know the essential