An Address ON THE PRACTICE OF MEDICINE AS A FINE ART.

DELIVERED BEFORE THE MANCHESTER MEDICAL SOCIETY,

BY

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ABOUT fifty years ago Buckle wrote that wonderful book, *The History of Civilization in England*, and in studying the methods of Cullen, the great Scottish physician, he says:

Even now and notwithstanding the great steps which have been taken in morbid anatomy, in animal chemistry, and in the microscopic investigation both of the fluids and solids of the human frame, the treatment of disease is a question of art, far more than a question of science. What chiefly characterizes the most eminent physicians, and gives them their real superiority, is not so much the extent of their theoretical knowledge—though that, too, is often considerable—but it is that fine and delicate perception which they owe partly to experience and partly to a natural quickness in detecting analogies and differences which escape ordinary observers. The process which they follow is one of rapid and, in some degree, unconscious induction. And this is the reason why the greatest physiologists and chemists which the medical profession possesses are not, as a matter of course, the best curers of disease. If medicine were a science they would always be the best. But medicine, being still essentially an art, depends mainly upon qualities which each practitioner has to acquire for himself, and which no scientific theory can teach. The time for a general theory has not come, and probably many generations will have to elapse before it does come. To suppose, therefore, that a theory of disease should, as a matter of education, precede the treatment of disease is not only practically dangerous but logically false.

Now the first essential in an argument is to define one's terms. And for a definition of science I again turn to Buckle. In considering the study of moral philosophy he points out that it is not a science, because if studied by induction we arrive at one conclusion; if we proceed by deduction, we arrive at another:

This difference in the results is always a proof that the subject in which the difference exists is not yet capable of scientific treatment, and that some preliminary difficulties have to be removed before it can pass from the empirical stage into the scientific one. As soon as these difficulties are got rid of the results obtained by induction will correspond with those obtained by deduction; supposing, of course, that both lines of argument are fairly managed. In such case it will be of no importance whether we reason from particulars to generals, or from generals to particulars. Either plan will yield the same consequences, and this agreement between the consequences proves that our investigation is, properly speaking, scientific. Thus, for instance, in chemistry, if by reasoning deductively from general principles we could always predict what would happen when we united two or more elements, even supposing those elements were new to us; and if by reasoning inductively from each element we could arrive at the same conclusion, one process would corroborate the other, and by their mutual verification the science would be complete. In chemistry we cannot do this; therefore chemistry is not yet a science, although, since the introduction into it, by Dalton, of the ideas of weight and number, there is every prospect of its becoming one. On the other hand, astronomy is a science, because by employing the deductive weapon of mathematics, we can compute the motions and perturbations of bodies; and, by employing the induction weapon of observation, the telescope reveals to us the accuracy of our previous and, as it were, foregone inferences. The fact agrees with the idea; the particular event confirms the general principle; the principle explains the event; and their unanimity authorizes us to believe that we must be right, since, proceed as we may, the conclusion is the same; and the inductive plan of striking averages harmonizes with the deductive plan of reasoning from ideas.

For my definition of art I turn to Ruskin. In a lecture on the unity of art, delivered at Manchester and incorporated in his book entitled *The Two Paths*, he says:

It would be well if all students would keep clearly in their mind the real distinction between those words which we use so often, "Manufacture," "Art," and "Fine Art." "Manufacture" is, according to the etymology and right use of the word, "the making of anything by hands," directly or indirectly, with or without the help of instruments or machines. Anything proceeding from the hand of man is manufacture; but it must have proceeded from his hand only, acting mechanically, and uninfluenced at the moment by direct intelligence.

Then, secondly, Art is the operation of the hand and the intelligence of man together; there is an art of making machinery; there is an art of building ships; an art of making carriages; and so on. All these, properly called Arts, but not Fine Arts, are pursuits in which the hand of man and his head go together, working at the same instant.

Then Fine Art is that in which the hand, the head, and the heart of man go together. . . . Thoroughly perfect art is that which proceeds from the heart, which involves all the noble emotions; associates with these the head, yet as inferior to the heart; and the hand, yet as inferior to the heart and head; and thus brings out the whole man.

Another definition which has been given is that "Fine arts are such as give scope not merely to manual dexterity, but to genius; as music, painting, sculpture, architecture, etc."

Now, I wish for the purpose of this address to take the liberty of translating Ruskin's word "hand" into the phrase, "All therapeutic means at the disposal of the physician or surgeon"; and these may vary from the giving of a drug to the giving of advice, from massage to the surgical skill required to remove a brain tumour. And I contend that the practice of medicine is a fine art proceeding from the heart, involving the noble emotions of faith, hope, and charity; it has associated with it the head, using all available methods of diagnosis; and the hand, including not only surgery, but all other available therapeutic measures.

Let us first examine shortly what an interval of fifty years—the time of my own life—has done to prove or to uphold Buckle's statement that medicine is essentially an art and not a science. Certainly in the case of chemistry, as Buckle surmised, science has triumphed, and every advance in chemical knowledge shows clearly how much nearer it becomes to the perfect science in which, reasoning by induction or by deduction, both lead to the same conclusion. Electricity and all other branches of Natural Philosophy in the same way are more and more approaching the level of true sciences. And if this is the case in the inorganic world, there seems to be hope that it may be so in the organic world, until we approach the most difficult problem of all—the nature of life itself; and hope also that one day the questions of disease and its treatment may become more and more scientific, although they may never be entirely so.

I will give some illustrations, drawn from the progress of the last fifty years, to show how this hope is becoming fulfilled. The germ theory of disease is a big general proposition that certain definite germs are the cause of certain definite diseases. Arguing deductively, we say that the growth of the tubercle bacillus in the body will cause fever, night sweats, wasting, and other symptoms depending on the organ attacked. Or, arguing inductively from particulars to general, we find irregular fever, cough, hæmoptysis, wasting, night sweats, and certain physical signs in the lungs, and we say they are caused by the growth of the tubercle bacillus. Again, we put forward the general statement that absence of a functioning thyroid gland will cause the symptoms of myxoedema; and we expect deductively to find the slow mind and speech, the coarse and thickened skin, the falling of hair, and the waxy complexion, with malar flush. Or, finding a patient with these well-known signs, we say inductively that there is a functionless thyroid gland, and that the patient will be cured by the administration of thyroid extract by the mouth. Perhaps you will excuse a personal allusion. The discovery of the cause of the symptoms of the epidemic of arsenical poisoning some twelve years ago was first a piece of very rapid induction (for, as a matter of fact, induction of some simple kind always, consciously or unconsciously, precedes deduction). This was: Here are cases of herpes; I generalized and said, the only drug which causes herpes is arsenic; then by deduction I said these cases are arsenical poisoning. I then started the problem the other way round and took all particulars, the paralysis, the pigmentation of the skin, the herpes, etc., and, arguing inductively, arrived at the same conclusion that they were cases of arsenical poisoning. All these are examples of pure science as definite as are the problems of astronomy; and so I might go on giving a series, ever increasing in number, of other examples in all branches of medicine and surgery. And these achievements have been brought about by observation and experiment aided by the use of new methods and instruments of precision, utilizing to the

full the researches of the physicist, the chemist, and the biologist, without whom we can make no progress.

The pathologist works almost entirely on purely scientific lines, and so does the hygienist, who is to my mind doing the highest work of any department of medicine, fulfilling the old saying that prevention is better than cure. But when we are dealing with the diagnosis and treatment of actual living patients the question of pure science is worthy of some further consideration.

We proceed in our investigations by the methods of observation and experiment. Now, the physician is very strictly limited in the method of experiment, for he must never do anything which may be harmful or unnecessarily painful to his patient. Bacon, in his *Novum Organum*, has compared experiment with the torture of witnesses, but nothing even of a suggestion of torture is permissible in diagnosis or treatment. In observation we watch with attention phenomena as they occur; to experiment is not only to observe, but also to place the phenomena in particularly favourable circumstances as a preliminary to observation. Or, as Mill says, that in observation we find our instance in Nature; in experiment we make it by an artificial arrangement of circumstances. It is observation to examine a drop of blood to assist in making an accurate diagnosis in some form of anaemia; it is experiment if we first give our patient arsenic and afterwards examine a blood film to see the effect of the drug on the blood cells. It is observation to look at a fractured bone by means of the *x* rays; it is experiment to give a bismuth meal and watch by the fluorescent screen the behaviour of the bismuth in the alimentary canal.

To obtain our information about the condition of a patient we must, then, use every legitimate means, and, if necessary, intensify and add to the power of our special senses by the use of mechanical, physical, and chemical methods. Taste is not now, thanks to the use of Fehling's solution, any more used as a diagnostic method. Smell is aided when by chemical means we produce some diagnostic odour, say in the urine, previously hidden. Hearing is made more conveniently applicable by the use of the stethoscope; and although I have not much faith in various instruments on the market supposed to intensify sounds, yet I have often thought some proper microphonic method would be useful, and I should gladly welcome some form of instrument such as is known in physical laboratories by which certain sounds could be picked out to the exclusion of others. Touch is aided by probes, electrical and otherwise; by instruments of measurement, from tape measures to cyrtometers; by instruments for amplifying and recording movements, such as the sphygmograph and the polygraph, or for estimating blood pressure by the sphygmometer; and in the thermometer we can make accurate the extraordinary uncertainty of the hand in testing a temperature. It is, however, in aiding our vision that we have so many means at our disposal—the microscope, the spectroscope, the polariscope; the *x* rays, the electro-cardiograph, the galvanic and faradic batteries for testing the condition of the neurons and the muscles, and chemical analysis which enables us by visible reactions to judge of the presence of albumen, of sugar, or of free hydrochloric acid.

In the above rapid sketch of the uses of scientific methods in medicine I have not indicated more than a minute fraction of what I think medicine owes to science; moreover, I believe all future progress will be on scientific lines, and on scientific lines only. But this does not, to my mind, controvert the assertion that the practice of medicine is a fine art. And this is the parting of the ways in my argument.

The practice of medicine is, if you like, an art based on scientific methods. To use an analogy which is not perhaps perfect—there was much science underlying and as a preliminary to the painting of Raphael's Madonna, the science of geometry and perspective, of light, and of colours; there is much science underlying the production of a piece of music, but the four single notes of the opening bar of Beethoven's fifth symphony on which the whole work is largely based do not constitute the symphony itself.

I will first consider some of the imperfections and some of the fallacies of so-called scientific medicine. I do this with much misgiving lest it should be thought for a

moment either that scientific methods should not be used or that I wish otherwise than to improve these methods. I do so also as a warning to those who most illogically are prepared to base their diagnoses on some one or two observations made by the later scientific methods and are content with diagnoses supplied by a clinical laboratory expert or made by an *x*-ray examination. Any such diagnosis must be entirely subservient to an examination of the patient himself made by the trained special senses of the physician. All this may sound like a platitude, but, unfortunately, I feel that the warning is seriously needed at the present time. The student of medicine, and sometimes his teacher, often forget that the old methods of examining a patient, conducted by the more or less unaided senses, including that very necessary sense, common sense, are just as much scientific methods as examination by the most elaborate chemical or physical methods. They certainly have to be learnt by long years of practice, and have to be done personally by the physician; and I am afraid that of late years I have noticed amongst students a certain tendency to indolence, because, not being able to conduct elaborate scientific methods themselves, they rely for their diagnoses on various reports from experts, and they seem to forget that when they enter into practice many of the elaborate scientific methods are out of their reach. A short time ago I asked a student how he would diagnose a pleuritic effusion. He promptly answered, "By the *x* rays." I then said, "And supposing you were practising in the Isle of Skye, how would you diagnose it?" He said, "I do not know."

It is not necessary to be in the Isle of Skye to be placed in a position where elaborate methods are unavailing, and, moreover, a point very often forgotten, elaborate methods take time and are very costly, and for this very reason are often practically impossible. Now, in this society, medical politics are tacitly forbidden, but under a certain scheme of medical service time will be short and money for special examinations non-existent.

One drawback to the scientific method is that the physician in his practice is dealing with one patient at a time. The hygienist can, as I have already said, work more scientifically, because he is dealing with masses of population, and can more easily check his results. But when one deals with a single person one must remember that no one person is like another, and therefore true scientific method is impossible; the coefficient of error is too enormous. I suppose no two chemical balances and no two measures are exactly the same; but their differences are so slight as to be negligible. But two sphygmograms from different patients can never be scientifically compared; nor even two from the different wrists of the same patient; and, indeed, hardly two taken at different times from the same wrist. The same thing applies to readings of the sphygmomanometer, and equally, of course, to merely feeling a pulse with the finger. And when it is remembered that man differs from man not only in structure but in inheritance and in life-history, it must be conceded that an investigation of his condition can never be a matter of pure science, but merely of science in which there is much surmise. It is owing largely to differences in structure, to imperfect instruments, and to want of experience that *x*-ray examinations may be so fallacious. We could not now do without them, but I could give several cases to show that opinions given and diagnoses resting only on this method have been most harmful in practice. Similarly, I have known most serious mistakes made when entire dependence has been placed on diagnoses made only by bacteriologists, either owing to pure errors or to imperfect methods, or to imagining on the part of the bacteriologist that his results were so scientific as to be incapable of fallacy.

Another fallacy attached to the scientific method is really the logical fallacy of "induction by simple enumeration," and one which I will call the percentage fallacy. If a certain phenomenon is present in 999 instances out of 1,000, one can fairly safely call the phenomenon almost a necessary part of the instance. But if we are dealing with a phenomenon occurring in, say, only 80 per cent. or 90 per cent. of certain instances, reliance on this frequency may, and often does, lead to diagnostic errors, and to errors in treatment. It is unscientific and a stifling of further investigation to say that without syphilis there would be no general paralysis of the insane, even if we can prove

that syphilis has been an antecedent in 90 per cent. of the cases. And because a symptom is present in, say, 80 per cent. of the cases of a certain disease, we must be careful not to exclude that disease because in the particular patient we are examining that symptom is not present. Rarely there is no tachycardia in Graves's disease; and although diarrhoea is only present in about 66 per cent. of cases of enteric fever I have seen the diagnosis excluded because of the absence of diarrhoea. Supposing for the sake of argument that by Cammidge's method the presence of cancer of the pancreas can be excluded in 75 per cent. of the cases tested, let us by all means use this information for what it is worth, but do not let it blind us to the fact that our particular patient may quite well be one of the 25 per cent. in which the reaction is of no account.

A further enormous drawback to the full use of the scientific method I have already alluded to, namely, the strict limitation of the method of experiment in practical medicine. And here I would like to enter a protest against totally uncalled-for methods of examination which may be painful to the patient. I have known of a case of far advanced general paralysis of the insane in which a well-known neurologist insisted on a lumbar puncture and an examination of the cerebro-spinal fluid before he would make his diagnosis.

Von Pirquet's reaction for diagnosing tuberculosis is to me worse than useless; it has so many fallacies that it cannot even be used as confirmatory evidence of phthisis. Once a strong medical man who had recently had an influenzal tracheitis, accompanied, as this often is, by slight haemoptysis, came to ask me where he had better go to live, as he was selling his practice immediately, because having obtained a positive von Pirquet's reaction he found he had phthisis. I told him what I thought of such a method of diagnosis, went into his recent history, his symptoms, and physical signs, gave him a more accurate diagnosis, sent him away for a short holiday, whence he returned to his work as well and strong as ever. Calmette's reaction is even worse, as it may be positively dangerous to the patient, and ought, I think, to be totally condemned as a diagnostic method.

Both in diagnosis and in treatment, one skilled clinician of long experience may be, and often is, much more accurate when not using elaborate scientific methods than is another who is using them. I have known hospital sisters of great experience, using no instrument but the unaided eye, diagnose accurately a malignant endocarditis, a typhus fever, and an acute miliary tuberculosis, when the physician in charge could not satisfy himself of the ailment. The scientific physician in treating a case of pneumonia may pin his faith down entirely to the use of a vaccine, and perhaps deprecate the use of other methods; the more artistic physician may use the vaccine but will most certainly not rely on it only, but on many other means, some of which may seem trifles to the scientist. This want of faith may be due to the present imperfections of the vaccine treatment, which may or may not disappear with the increase of knowledge, but the necessity of the existence of the artist will still remain. In the use of drugs, again, the artist will shine. Take the case of opium; there is an art in knowing when to give it and when to refrain; it is said to be dangerous in cases of bronchitis and in Bright's disease, yet it is just in these two conditions in which an artist can use it with such enormous benefit; and it requires an artist to say which of the numerous hypnotics to the exclusion of all others should be given to any particular patient suffering from insomnia. All this, if you will, is in the nature of intuition or of mere empiricism; that is so, but it is not without enormous advantages to the patient, and it emphasizes the absolute necessity of learning medicine by the bedside as well as in the lecture-room or the laboratory. Medicine must be learnt from the experienced clinician rather than from the skilled scientist, and if I had my way I would allow no one to practise medicine unless he had first resided for at least twelve months in a general hospital.

I will give a few further examples of art in medical practice, and I will first allude to the art of cross-examination, which can only be learnt by personal experience. This art, in which I include the art of the detective, is an absolute necessity in the investigation of disease, and it is

because some men do not possess it that they are never a success in medical practice, and without it many blunders are made. I learnt such power as I possess in this direction from repeatedly, some years ago, listening to the cross-examinations of the late Mr. Sidney Smelt, the then coroner for the city of Manchester. It was an education to see him handling either an unwilling witness or an ignorant, stupid witness, or a too voluble witness; the patience he displayed, the way he sorted out the wheat from the chaff, the immediate use he made of a phrase dropped by accident, of the peculiar accentuation of a sentence or even a change of emotion shown by the voice or in the face: and having thus been put upon the track of some new truth, the way in which he followed it up until the truth was revealed was a pure delight to me. Of course, in cross-examination, knowledge of what you are looking for, or of what may be revealed, is necessary, and therefore it cannot be used to the full by the young student, who must learn, as I had to do, by watching the process used by those of greater experience. Scouting without knowledge is useless for obtaining information. If I were in a jungle, I might observe certain spoor, the footprints, the way vegetation was disturbed, the kind of animal droppings, but this would give me no information; it is only the trained shikari who can tell you that a tiger has passed that way.

Again, in the practice of medicine the art of the lawyer is frequently employed. The other day I was asked by one of the laymen on the infirmary board if in considering a certain question at the medical board the presence of a lawyer would be of any assistance to us. I declined the offer with thanks, telling him that we were giving legal advice to lawyers every day in the week. I cannot give instances, but you all know how in both diagnosis of cause of illness and also in the treatment it may be quite necessary that legal matters have to be gone into often with considerable detail.

And the doctor as priest; the man to whom confessions are made when they can be made to no one else, the man who has to blame, and often blame severely, the wrong-doer, and to give praise where praise is due, to encourage the patient in his efforts to reform, to sympathize properly (and what mischief the improper sympathy of the layman does is untold); and this sympathy must not only be for the sufferer in his bodily pain or his mental distress, but also for the relations and friends. And the physician must be able to make his patient believe in him, for without this faith his ministrations may be as nothing. And, finally, the power of instilling hope, even if there is but little left, for by this faith and hope many have reached the end with a feeling of pleasure, and known nothing of the agony of death.

But these things are not science; they are art, and they are fine art, for they require the hand, the head, and the heart.

A COMMON FORM OF HEART DISEASE (AURICULAR FIBRILLATION).

BY

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In the first place I desire to thank you for the honour you have conferred upon me in inviting me to read a paper before the Worcestershire and Herefordshire Branch of the Association.

Your invitation came to me when I happened to be reading the Schorstein lectures by Dr. James Mackenzie on auricular fibrillation,¹ and it occurred to me that this would be a good subject to bring before you, as I have been working at it for the past two or three years. Moreover, it is a subject of which every medical man must have some considerable knowledge and experience.

Auricular fibrillation is a clinical entity. We have all of us met it, although we may not have been aware of the fact. We are in the same position as Monsieur Jourdain in *Le Bourgeois Gentilhomme* when he discovered during his lessons in philosophy that he had been talking prose all his life without knowing it. Auricular fibrillation, for all practical purposes, is a paralysis of the auricles; yet it is not quite a paralysis, the auricles are in a state of