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MEDICAL FACTS AND FALLACIES*

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In the first Aphorism of Hippocrates it is written that experience is fallacious or, to be more accurate, there appear the Greek words *ἡ δὲ πείρα σφαλερή*. The exact meaning of these words is open to discussion, as they have been variously rendered into several tongues. The accepted Latin translation is "experimentum periculosum," and Sprengell, whose English translation appeared in 1708, evidently thought that the word experimentum should be literally translated as experiment: "to make experiments dangerous," he says. We may concede that the Father of Medicine would have disapproved of experiments on patients on other grounds than their danger, but experiment in the modern sense of the word was unknown in the fifth century before Christ; it is, I think, very doubtful whether Sprengell's rendering gives the true meaning of the Greek words. The French, taking Littre's translation as authority, say "l'expérience trompeuse"; but "expérience," like experimentum, may mean either experience or experiment. Nothing but the context can give the clue to the true meaning, and in this isolated sentence there is no context to help us.

The most recent translation of the Aphorisms which I have consulted—Jones's translation in the Loeb Classical Library—gives the rendering "experience treacherous (or deceptive)." Not entirely satisfied with this, I referred to Liddle and Scott, who give as the first meaning of *σφαλερὸς*, "likely to make one stumble." This seems to me as good a rendering as any, whether *πείρα* means experience or experiment, for neither of necessity leads us to the truth. Yet we must confess that when experience leads us astray or experiment makes us stumble the cause is our faulty interpretation of facts which would be clear if we had eyes to see. It is well, perhaps, to recognize that experience is not always an infallible guide, and that facts, apparently confirmed by experiment and observation, will not always stand the test of time. In diagnosis and treatment experience is unavailing unless that common sense, which has been said to be the least common of all the senses, is also brought to bear upon the problem. We frequently find that the facts which are accepted without question by one generation become notorious fallacies in the next. Bright's disease occurs to me as an example. Thirty years ago it seemed established that the various types of so-called Bright's disease were primarily diseases of the kidneys. To-day we are being led to the belief that the renal lesions are but a small part of the pathological changes which, in reality, involve a number of other organs, tissues, and functions.

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Empiricism and Rationalism

Modern medicine is built on a structure whose foundation is experiment. But the modern practitioner is still apt to base the practice of his art upon the philosophy of empiricism. Nor is this entirely a bad outlook, for unyielding rationalism is as harmful to medicine as intransigent empiricism. The young doctor, until his mind has been ripened by the rain and sunshine of experience, is usually a stubborn rationalist; he will believe nothing which cannot be fully explained by those exact sciences to which he has devoted so many years of study. The passing of time brings a change. Science and rationalism give place to art and empiricism, until finally there is a danger that the science be lost and only the art remain. This does not of necessity make bad doctors; but the best philosophy for the medical man is a blend of empiricism and rationalism. A balanced judgement is bred from a healthy scepticism, large-minded enough to be tolerant of things that cannot be clearly understood, combined with a desire for proof and an appreciation of the meaning of scientific evidence.

Doctors, as a class, are far too willing to accept as proof evidence of the most exiguous character. This is partly the fault of our training, for it is given to few to pass through the chastening fire of work in the research laboratory. It would sharpen our critical faculties if we could all spend a year entirely devoted to research. The field of pure knowledge might not be greatly enlarged, but we should emerge wiser men and better doctors.

The Importance of Doubt

The strict standards which the research worker must exact of himself have never been better expressed than in the famous passage from Claude Bernard's classical work on experimental medicine. He emphasizes the importance of doubt and scepticism even after a theory has been amply confirmed by experimental proof. He says:

"... dans tout raisonnement expérimentale, il y a deux cas possibles: ou bien l'hypothèse de l'expérimentateur sera infirmée, ou bien elle sera confirmée par l'expérience. Quand l'expérience infirme l'idée préconçue, l'expérimentateur doit rejeter ou modifier son idée. Mais lors même que l'expérience confirme pleinement l'idée préconçue, l'expérimentateur doit encore douter; car, comme il s'agit d'une vérité inconsciente, sa raison lui demande encore une contre-épreuve."

These words should be written in letters of gold not only over the laboratory bench but over the consulting room table. For, though it falls to the lot of few to be research workers, it is necessary for all of us to seek the truth and to distinguish fact from fallacy. The road to the truth is no easy one. It is certainly no modern

"by-pass" along which we can race and pay no heed to the posts that point the way. Rather is it a mountain path which must be climbed perilously on foot; a way that is often blocked and over which we must many times retrace our steps before we find ourselves, if we ever do, at the summit. It is easy to rest half-way; still easier to wander along the low-lying paths of the valleys. But if we wish for the clear and distant view which only the topmost pinnacle can give us we must work patiently upwards until the last obstacle has been surmounted, and then, indeed, the world will be at our feet.

But a search can hardly be successful unless we know clearly what it is for which we are looking. Our search for facts will be barren if we start without a clear conception of their nature. Now a fact is a theory which has been proved, and proof is a matter of evidence. The alleged criminal is assumed innocent unless the evidence of his guilt is overwhelming. The slightest doubt and the verdict must be "Not guilty." Medicine would do well to take a leaf from the notebook of the law. We are, I fear, too prone to find our prisoner guilty on evidence which no judge or jury would accept. Before we admit a fact as proven the evidence must be conclusive, and even when proof seems certain we must, as Claude Bernard has told us, still be prepared to doubt. For, as we have seen, what is proven fact to one generation may be patent fallacy to the next. We must never be so wedded to our facts that divorce is impossible.

Use and Abuse of Drugs

Nowhere in the whole realm of medicine is the conflict of fact and fallacy, and of empiricism and rationalism, more clearly shown than in the field of therapeutics, particularly in relation to treatment by drugs. Here is well illustrated the virtue of empiricism, for many of our most effective drugs were first used as traditional remedies by the country folk of many nations before the medical profession had anything to do with them. Digitalis is a notable example. In its early days it was a true old wives' cure for dropsy. Withering in 1776, having learnt from an old Shropshire woman of the virtues of the foxglove, tried it in his practice, and was convinced of its value. But he no more knew how it cured than he knew that dropsy was not a disease but a symptom of many diseases. Half a century passed by before Bright drew attention to the association of renal disease with albuminous urine, and so paved the way for a distinction between renal and cardiac dropsy. And we can afford to smile at Withering's disappointment at the failure of his foxglove leaves to cure a case of ovarian cyst—then regarded as a form of dropsy. The final demonstration of the specific effect of digitalin and certain allied alkaloids upon the disorder of cardiac rhythm which we now know as auricular fibrillation belongs to the present century. But tradition dies hard; and we still find preparations of digitalis being used, if not for ovarian cyst, for conditions for which there is little evidence that it is beneficial.

Our textbooks of pharmacology are full of the ascertained actions of drugs upon experimental animals, in doses which could never be employed in medical practice. We should be cautious in assuming that a patient will react in the same way as an animal, or that a small dose will have the same effect as a large one. The fact that digitalis is a proven remedy for one relatively common disorder of the heart's rhythm has led to the unjustified assumption that it is a universal cardiac "tonic." It is widely used in pneumonia, but the evidence that it confers benefit upon sufferers from this disease is far from conclusive. In one large-scale experiment, adequately controlled, the mortality was strikingly higher in a group of digitalised pneumonic patients than in a similar group

of cases from whom the drug had been withheld. I feel that group experiments of this sort in other acute diseases for which digitalis is commonly given might well give very comparable results. The rationalistic view of digitalis therapy at the present time is simply that it controls the ventricular rate, and thus restores circulatory efficiency, in auricular fibrillation alone. In other cardiac conditions its uses are doubtful, and outside the province of cardiology its benefits are quite unproved.

Even in its limited sphere, however, we must beware of a common error. It does not, as is often supposed, "cure" fibrillation; nor should we, on theoretical grounds, expect it to have any direct effect upon the mechanism of a circus rhythm. Its action is quite distinct from that of quinidine, and, if fibrillation ceases during the administration of digitalis, the result is to be credited to nature rather than to the treatment.

A further instructive illustration of the use and abuse of drugs is furnished by quinine. Known apparently since the earliest times to the natives of Peru as a cure for ague, the cinchona bark was introduced into Europe by the Jesuits in 1632. Its universal application in the treatment of all fevers soon made it apparent that a certain type of intermittent fever was specially susceptible to its action; and thus it was that Sydenham was able to distinguish what we now know as malaria from other types of febrile disease. But even at the present day we find quinine being widely prescribed for all manner of infective disorders as diverse as influenza and malignant endocarditis. Yet surely the truth is that (excluding its uses in obstetric practice) its place in the *Pharmacopoeia* is justified solely by its specific action on the parasite of malarial fever.

Medical Credulity

We often marvel at the credulity of our patients, but the truth is that we are almost unbelievably credulous ourselves. This weakness is quite naturally exploited by the manufacturers of drugs and therapeutic products who have enough acquaintance with psychology to know that we shall be impressed by the presentation of a remedy in pseudo-scientific language. I am afraid that most of them—there are a few distinguished exceptions—are not over-scrupulous as to the matter or the manner of their advertisements. But, just as our job is to treat our patients so theirs is to sell their wares, and we cannot blame them if they take advantage of our simplicity. "The ultimate proof," says the skilful advertiser, "lies in clinical trial." There follows a spate of testimonials from doctors of varying degrees of eminence (judging from their academic qualifications) all joining in a monotonous paean of praise based, as a rule, upon a single case in which the remedy has been tried. The failures are not mentioned, but it would be equally logical to attribute the failures to the action of the drug as the successes.

Credulity is not, however, the prerogative of our present age; nor is it, I think, likely to grow less with the passing of the years. Many instances could be quoted from history, and it is interesting to speculate whether the credulities of the twentieth century will seem as fantastic to our descendants as to us seem those of our ancestors. As an example let me quote to you a passage from the life of Elias Ashmole, the seventeenth century doctor, scientist, and antiquary. "I took," he says, "early in the morning, a good dose of Elixir and hung three spiders about my neck, and they drove my ague away—Deo gratias." A century later we find Horace Walpole writing to his old school friend, Thomas Gray, the poet and author of the famous *Elegy*, in these words. "... Madame de Bouxols, Marshall Berwick's daughter, assured me that there was nothing so good for the gout,

as to preserve the parings of my nails in a bottle close-stopped." And this may serve to remind us that colchicum is still an empirical remedy the value of which few would deny, although it has as much scientific basis as the simpler expedient recommended to Walpole.

Judgement in Using Remedies

But in all seriousness it is clearly of the greatest importance that medical men should develop judgement in the use of remedies. This is not easy, as our experience is derived from clinical use; and clinical experiment, in spite of what the advertisers tell us, is the one type of experiment which is most likely to lead us astray. Partly this is due to the fact that most clinicians are but poorly trained in experimental method. Partly to the wish being father to the thought; for we never use a remedy without a hope that it will cure. But mainly, it seems to me, because such experiments must be largely uncontrolled and because we fail to give a proper value to a negative result.

Many of you will remember the failure of Martin Arrowsmith's experiment in Sinclair Lewis's brilliant novel. Sent to test a newly discovered plague serum on an island stricken with an epidemic, he starts to inject alternate patients so as to have an equal number of controls. Finding that the latter are obviously dying from being denied the serum which is saving the lives of those selected for treatment, he sacrifices his experiment to his humanity and deliberately spoils the fruits of years of patient research. All of us who try a clinical experiment of this nature may find ourselves between the same Scylla and Charybdis. Untreated controls are essential for scientific proof, but such controls may die for want of treatment. Clinical experiment involves the heart as well as the head of the experimenter, and his plan of work must be made to fit the practice of his art.

The Negative Result

But consider for a moment the value of a negative result. In expressing the results of our trial of a new remedy we are apt to say that we have tried it in a number of cases and in a few it has been successful. If the successes are only few we should rather say that in most cases it has been a failure; for experiment should be democratic and the rule of the majority hold sway. A remedy which has only occasional successes to its credit is probably winning an unmerited reputation; for genuinely successful agents yield successes which approximate to the 100 per cent. level. How often do we meet with a hyperglycaemia which fails to respond to insulin, a case of pernicious anaemia which cannot be controlled by liver therapy, or a myxoedematous patient whose metabolism will not be restored by thyroid gland? These are all proved remedies; their results are therapeutic facts and their failures are negligible. There are other therapeutic products of this class, but a moment's reflection must convince us that their number is small; and, further, that most of them are biological preparations rather than drugs in the strict sense of the word.

It is probably a safe generalization to state that there are no drugs that cure except in the domain of those diseases—mostly tropical—which are caused by protozoa. The exceptions to this rule are those substances which cure by what is now called "substitution therapy," and the disease which they cure can be classed among the deficiency diseases. Obvious examples are the use of iron in hypochromic anaemia and iodine in certain forms of non-toxic goitre. If we narrow the field to include only the microbic diseases we can, without fear of contradiction, assert that drugs with specific action are as yet unknown. Yet the faith of our patients in a bottle of

medicine is still widespread, and the tradition that therapeutics consists in finding the right medicine for the disease dies hard. In America the faith in medicine has long given place to faith in the hypodermic syringe, and the doctor is pestered by his patients for what they tersely call "a shot in the arm." There, indeed, the pendulum has swung too far, and the new idol is worshipped to excess. Even in this country we have not entirely escaped, and we find drugs being given by the hypodermic route which would be more effective if given by the mouth. It is now well known that an adequate dose of iron cannot be given by any other method than by the mouth, yet the number of proprietary preparations of iron for injection therapy which are still on the market bears witness to the fact that the lesson of the last few years has not yet borne fruit.

Vaccine Therapy

The subject of hypodermic injection suggests that it may not be out of place to say something here on the subject of vaccine therapy. On this subject I would venture to remark that there seems to me to be no sound evidence to show that vaccines have any place in the treatment of acute or chronic infective disease. In my own practice the failures have been sufficiently numerous to justify the conclusion that they have exercised little effect on the progress of the disease; and the few successes could be attributed to the *vis medicatrix naturae* with as much justice as to the effect of the injections. I feel sure that any impartial observer who studies his results with vaccines and gives at least equal value to the successes as to the failures must conclude that the case for vaccine treatment has yet to be proved. We may go even further; for, inasmuch as the whole theory of the use of vaccines is that they are specific, there is less excuse for failure than there is with drug treatment or substitution therapy. And if salvarsan and its derivatives can, when properly used, yield as small a number of failures as they do there is little excuse for vaccine treatment if it falls short of this standard. But could anyone be found to maintain that the results of treatment with vaccines could be compared for success with the results of salvarsan treatment in syphilis?

The role of vaccines in prophylaxis is quite another matter. Here the percentage of success is high—so high that no unprejudiced observer could doubt their value in their proper sphere—for their application is by no means universal, even among the bacterial diseases whose cause is known. But, within its limits, the use of vaccines in preventive medicine may be claimed as one of the most clearly proven facts of our science.

Right Attitude Towards Remedies

At this point I am conscious that my hearers may be murmuring to themselves: "But you cannot apply research standards to medical practice. Are we to use no drugs or remedies which cannot show 100 per cent. successes?" If such thoughts are passing through your minds let me say at once that I have no wish to urge you to restrict your therapeutic practice to the class of remedies whose action is of proven benefit; but rather that you should use all remedies with discretion and with scepticism.

The occasional success of a remedy is not necessarily due to its lack of specific action, for often we employ them upon groups of cases in which the precise diagnosis is not clear. If we may return to digitalis for a moment it will be obvious that its use for all varieties of dropsy will yield but a very moderate measure of success. But if we confine its use to cases of auricular fibrillation only, its failures become insignificant. There are still many

chronic diseases whose aetiology is obscure. Many of these are probably not diseases, *sui generis*, but groups of diseases with many symptoms in common. We may here remind ourselves that until recent times jaundice was regarded as a disease, whereas now we know that it is but a symptom arising from a great number of morbid conditions. And I think it is safe to conjecture that the large group of rheumatic diseases will, sooner or later, undergo a similar process of simplification, with the discovery of causes which are at present unsuspected. Just as we now know that so-called sciatica is the result of quite a number of different pathological processes so, ultimately, we may find that the various forms of arthritis will be classified into simple aetiological categories instead of being described mainly from the nature of their clinical manifestations.

The recent introduction of gold for the treatment of rheumatism will serve as a good example of what our attitude should be towards a new remedy. If it was specific for all forms of rheumatism it would not take us long to discover the truth; but the results so far reported, while not entirely discouraging, have certainly not been comparable with the results of liver therapy in pernicious anaemia. It is, however, quite possible that there is one small and unrecognized group of rheumatic disorders for which gold salts are in fact specific; but until we have the means to recognize these cases we can only continue to use the method without discrimination in the hope that the swings will give a return for what is wasted on the roundabouts. A remedy such as this may indeed serve to make clear for us the existence of a special type of rheumatism just as quinine was the means of differentiating malaria from the other types of fever. There is thus a place in medicine for the empirical use of new remedies if only we will remember that our proven remedies do, in fact, yield results approximating to the 100 per cent. mark, and if also we will always give at least an equal value to a negative as to a positive result.

Fact in Diagnosis

There we must leave the consideration of fact in therapeutics and turn for a moment to a survey of the problem of fact in diagnosis. Some of you may join with me in wondering whether there really is such a thing as fact in diagnosis. Certainly all will agree that the number of patients who leave our consulting rooms with a diagnosis both dogmatic and true is exceedingly small. In this age of elaboration it is hardly unfair to say that clinical diagnosis, unsupported by evidence from the laboratory or x-ray room, can never be absolutely certain; for absolute certainty can only be achieved if there is no means of proving the contrary. How fortunate, compared with us, were our fathers! Faced with a case of obscure pyrexia they could make a confident and dogmatic diagnosis of typhoid fever, secure in the knowledge that nothing but necropsy could prove them wrong; and if, indeed, the illness exceeded in its course the allotted span of typhoid, a change of front could be made with a minimum of loss to dignity and reputation. But who to-day would dare to say that a man had typhoid unless the serum had first been tested for its agglutinating power? And who could for long maintain a diagnosis of tuberculosis with the bacteriologist and radiologist ranged firmly on the other side?

It seems that fact in diagnosis—that is, a certainty as high as can be attained by the human mind—requires in nearly every case the collaboration of some other expert besides the clinician. This is not to say that the advice of such expert must be sought in every case. Fortunately in a majority of cases certainty of diagnosis is of academic rather than practical importance. Our patients are relatively uninterested in diagnosis. What

they wish us to tell them is how to get well, and they will accept almost any name we choose to give for their disease, with a strong partiality for one derived from either of the dead languages. Our responsibility is to be sure that we are prescribing the proper treatment. If uncertainty of diagnosis leads to uncertainty of treatment the diagnostic field must be dug deeper. But when precision of diagnosis cannot yield fruit in the form of more suitable treatment we may safely leave the field untilled. A golden rule to follow when there is doubt in diagnosis—even after full investigation—is to choose the alternative which offers the greatest scope for treatment.

If it is seldom our fortune to achieve certainty in diagnosis it is still true that all diagnosis must be built upon a basis of many facts. In a simple case of pneumonia—which I choose because it is a disease where purely clinical methods yield practical certainty—our diagnosis is the result of deductions made from observed facts. The subjective ones are provided by the patient. But, objectively, it is a fact that the patient has pyrexia, that his chest is dull, that his breath sounds are tubular, and that his sputum is rusty. Here again our professional practice deserves comparison with a branch of the law. Criminal investigation, in its technique, has much in common with medical diagnosis. Both must remain anchored to facts, though we may cruise with discretion through a sea of theories. The art of the physician, as of the detective, is to make sound deductions; and this is only possible if he is possessed not only of experience but also of a sufficiency of technical knowledge.

Deductions from Observed Facts

The deductions which we make from our facts yield results which vary in proportion to the number of such facts available. In the case of a patient with pneumonia, which I have used as an example, the facts are sufficient for a deduction which has but a small margin of error. Faced with a paucity of facts—as when pyrexia is the only objective sign of disease—we are in the position of a detective who has but a single clue to the perpetrator of a crime—say, the knife with which the murder was committed. Unless some further fact can be discovered there is nothing for it but to wait in the hope that the criminal may give himself away. In medicine we refer to this as “expectant treatment,” and I have no doubt that Scotland Yard has some equally well-sounding phrase with which to calm the consciences of their baffled agents. But the desired event will only sometimes happen, and, more usually, the aid of science is invoked and the knife is scrutinized for finger-prints or the patient's blood examined for evidence of the infecting organism.

Just as the clinician may be misled by false deductions from observed facts (or from failure to observe them) so may his laboratory colleagues mislead him by falsely interpreting the additional facts which they may bring to light. The biochemist, the pathologist, the radiologist, none of them are infallible. If comparisons may be made between them I would suggest that their accuracy is in the order in which they have been named. This implies no criticism and no disparagement. The biochemist is rarely asked to give us more than simple facts, and, subject only to faults of technique, there can be no argument about these. The content of the blood in urea, sugar, calcium, or other substance is pure fact, and the blame is ours if we misuse it. The pathologist has a wider margin of error although much of his work, too, is purely technical. But the nature of an abnormal blood cell, the precise class in which an obscure organism should be placed, or the presence or absence of malignant changes in a histological section, are matters which call for judgement and experience. A false inter-

pretation may mislead the clinician as much as a correct one may help him.

If the radiologist comes at the bottom of the list it is merely because his is the youngest science. We have only recently emerged from the days when a skiagram was little more than a diverting and interesting episode in the investigation of a clinical problem. The progress of radiology in the last fifteen years is one of the most astounding features of modern medicine. But its accuracy depends as much on the interpretation of what is seen as on the technical perfection of the radiograph. Outside hospital practice the opportunities for studying radiographs are few. As a result, the average practitioner requires of the radiologist not only fact but theory. An accurate description of an abnormal shadow is not enough; he needs to know what that shadow portends. The opinion of the radiologist is therefore often given a weight which he himself would deprecate, since he recognizes that the x -ray appearance can never completely override the results of clinical observation. But there is an element of magic about x -ray work which deceives our patients into expecting diagnostic miracles and ourselves into placing too high a value upon its results.

In the sphere of diagnosis the pathologist has the advantage over the biochemist and the radiologist, for he has at his disposal a number of tests which may be regarded for all practical purposes as pathognomonic. The chemical laboratory provides us with many facts, but few of them are in themselves diagnostic; while in radiology the interpretative faculty must be so widely exercised as to extend the potential fallacy of human error throughout its domain. When the pathologist sends us the result of a sero-diagnostic test, whether it be a Wassermann reaction or an agglutination, he is sending us the nearest thing to a ready-made diagnosis that is to be had in medicine. The discovery of a specific organism, whether it be a tubercle bacillus in the sputum or a meningococcus in the cerebro-spinal fluid, is more than a simple fact upon which a diagnosis can be made; it is the completed structure of the diagnosis itself.

Reviewing the whole field of fact in diagnosis, I think we can safely assert that conclusive proof is rarely achieved by clinical methods alone. A degree of probability, amounting to practical certainty, may be reached by those who have the gift of accurate observation and sound deduction. But pathognomonic signs are rare in purely clinical diagnosis; such as there are are mainly in the province of the pathologist. And while chemistry and radiology provide invaluable contributions to the diagnostic total, the one gives us facts few of which are in the strict sense diagnostic, while the other supplies information the truth or falsity of which depends largely upon the skill with which we interpret it.

Sources of Error in History Taking

Having considered the more philosophical aspects of fact and fallacy in medicine, I propose to devote my concluding remarks to a brief survey of some of the more obvious fallacies which beset us in our daily work. And, first, I would ask you to consider for a moment the histories supplied to us by our patients. We have all been brought up to believe that the first step in clinical diagnosis is an accurate history, and that examination should only be carried out after this has been obtained. If we follow this rule we must remember that the uneducated patient has but poor powers of observation, analysis, and expression; and that the educated may have such command of language that he may be reluctant to spoil a good story by too strict an adherence to the truth. This was recognized by Pye-Smith, who, in a clinical lecture to students at Guy's Hospital in 1898, advised that examination should precede the taking of

the history. The history was, he said, "comparatively unimportant. Your natural tendency is to lay too much stress on the history." He elaborated his point in the following words:

"Again and again, without intention or wish to deceive, the history you get from hospital patients is so confused, so inaccurate, so involved, lapses of memory are so frequent, and they so mix up what they know with what their friends have told them and what the doctor has told them and what they suppose, that it needs the skill of an Old Bailey lawyer to get at the facts of the case. On the whole, you will economize time by examining the patient first and listening to his narrative afterwards."

While I do not suggest that we should take this advice too literally it does, I think, illustrate a fallacy into which we are all, from time to time, liable to fall. We depend, of necessity, upon our patients for the subjective phenomena of disease; and if, as is frequent, objective findings are few, we are apt to base a clinical diagnosis on the patient's narrative alone. We must be prepared for inaccuracies in this, arising not so much from a deliberate desire to mislead as from inability to express in words the complex visceral sensations resulting from their disorder. A further source of error in history taking is the use of words which have one meaning for the patient and another for the doctor. Patients, I find, will often tell us that they "vomit," but will deny "being sick"; or they may do neither of these things, but yet "bring up their food." To most of us there is no special distinction between being sick and vomiting; but patients of the hospital class use words and phrases in senses which no dictionary would authorize. "Nausea," "palpitation," and "giddiness" are further examples of words which have an almost technical significance for doctors, but which are loosely used by patients to describe a great variety of curious internal sensations. Care should be exercised in interpreting symptoms, unless they are described in words whose precise meaning leaves no room for doubt.

Pallor and Anaemia

Even a perfect history is not an infallible diagnostic guide. There is no time for me to do more than indicate a few of the more common fallacies which confront me weekly in the out-patient clinic. The first to which I should like to direct your attention is concerned with anaemia—the most commonly diagnosed but the least frequently recognized of all diseases. Time after time patients are referred to hospital for "anaemia," and investigation shows that their red cells are normal in size, shape, and number, and that they contain a normal content of haemoglobin. The explanation of this simple fallacy is the failure to recognize that while all anaemic people are pale by no means all pale people are anaemic. My experience has been that most patients referred for anaemia have no anaemia; and, conversely, that those who have anaemia are sent up with some other tentative diagnosis. They may be short of breath and are suspected of heart disease; their ankles swell and kidney trouble is feared—especially if, as may well be the case, there is also a trace of albumin; or they complain of undue fatigue, and reach the hospital with a request for an x -ray of their chest.

Many of these cases may be diagnosed clinically with a fair degree of accuracy if attention is paid to the colour of the conjunctivae and mucous membranes rather than to the skin. A snow-white skin may be nourished by a blood rich in red corpuscles and haemoglobin; and such a skin may indeed be paler than that of a patient with a very definite degree of anaemia. This fallacy will seem obvious to many of you, but it occurs with such depressing regularity that I have thought it worthy of comment.

Fallacies in Diagnosis of Chronic Dyspepsias

The hospital clinic of the general physician, consisting as it now does very largely of the waste material from the special departments, is crowded with the race of dyspeptics, and from these we might harvest a rich crop of common fallacies. The first of these I put forward with some diffidence, as I incur a risk of being accused of anti-surgical bias. I hope you will accept my assurance that this is not the case when I assert my belief that peptic ulcer is a medical and not a surgical disease. By this I do not mean that such ulcers should never be submitted to surgical treatment; I am simply commenting on the rather surprising fact that a large majority of all cases of suspected ulcer are referred by their doctors to the surgical rather than to the medical clinics. The relative merits of surgical and medical treatment are outside the scope of my present subject, but I hope I may secure your consent to the proposition that cases of chronic dyspepsia of whatever type should *in the first place* be referred to a medical department.

Many of these cases of suspected ulcer are, in reality, suffering from non-ulcerative conditions of the gastro-intestinal tract. A frequent mistake in the diagnosis of these patients is the assumption that pain related to the taking of meals must be due to peptic ulcer. Many such pains are, in fact, not due to either organic or functional disturbance of the stomach, but to a disorder of the large bowel commonly known as "spastic colon." The pain in such cases is situated below the umbilicus, a very uncommon location for the pain of gastric or duodenal ulcer, which is remarkably constant and usually high in the epigastrium. The importance of accurate localization of abdominal pain cannot be exaggerated, and it is not safe to assume that a pain described as "in the stomach" is either epigastric in situation or gastric in origin. The word "stomach" is one of those to which I have referred as having one meaning for patients and another for doctors. The former employ it usually as a polite synonym for "belly." There is only one way to find out the precise location of a pain in the abdomen, and that is to ask the patient to indicate it with his finger.

"Acidity," "hyperacidity," and "anacidity" (or hypochlorhydria) are often held responsible for painful dyspepsia, but, in truth, these disorders of secretion are not common, and do not, of themselves, give rise to symptoms. Many of my patients referred for hyperacidity have been found, on investigation, to have absent or deficient acid in their gastric juice. The reason for these popular mistakes is the assumption, unsupported by experimental evidence, that the mucous membrane of the stomach is sensitive to changes in the degree of acidity of the gastric juice. The pioneer work of Lennander, Hurst, Ross, Mackenzie, and Carlson has clearly shown that the whole of the alimentary tract, with the exception of the mouth, oesophagus, and anal canal, is insensitive to those stimuli which provoke painful responses in the skin. Heat and cold, acid and alkali, cutting and scratching are all quite painless to a healthy mucous membrane, unless, like the portions mentioned, they are lined with squamous cells. Anyone may satisfy himself of the truth of this contention by swallowing a mouthful of hot tea or ice-cream and noting the presence of oesophageal and the absence of gastric sensation; or if chemical stimulation is preferred a glass of neat brandy will serve as well.

The question whether a diseased mucous membrane may not acquire sensibility to these stimuli is one which is not yet finally solved, although it has been shown that ulcers are not necessarily sensitive to concentrations of acid greater than any that would be found in the

stomach under physiological conditions. The conclusion may, however, safely be made that disorders of motility are a more frequent cause of pain than disturbances of secretion. The latter, indeed, appear in many cases to be determined rather by abnormal motor activity than by any unusual behaviour of the secreting glands.

Recognition of the causes of visceral pain explains many curious anomalies of gastro-intestinal disease. We need no longer assume that a painful stomach is the seat of organic disease any more than is the painful colon of colospasm. Disturbances of tone—or of what Sherrington has more aptly termed "posture"—may result from a multitude of extrinsic causes, and our search for these must not be limited to the abdomen. A clearer appreciation of these facts would lead to a lessened use of some well-worn but fallacious abdominal diagnoses.

The application of radiology to gastro-intestinal disease gives rise to another widely held fallacy. It is often supposed that a film or films of the alimentary canal following the ingestion of a barium meal will be sufficient to detect organic lesions or functional disturbances. Actually such films are of relatively little value. Direct visualization of the viscera on the fluorescent screen is essential for accurate radiological diagnosis. Films have their place in confirming what has been seen upon the screen with greater definition or for making a permanent record; by themselves they prove little, and they may often fail to disclose abnormalities which have been clearly seen by the direct method. Screen examination must be left to the expert, and it is better not to attempt gastro-intestinal work in hospitals and institutions where the apparatus and personnel are unequal to the task; otherwise mistakes will be made and much valuable material wasted.

The Rheumatic Diseases

The rheumatic diseases offer numerous examples of fallacies of observation and deduction. May I ask you to bear with me while I make a few observations upon that common and disabling complaint sciatica? The first fallacy is to regard this complaint as a disease instead of as a symptom. Primary neuritis of the sciatic nerve, which is really not a neuritis at all, but a perineural fibrositis, is comparatively rare. Pain referred to the distribution of the sciatic nerve is common enough, but a majority of such cases show no evidence of direct involvement of the nerve or of its fibrous sheath. Tenderness is absent, sensation and power are normal, and the ankle-jerk is unimpaired. The error is important because it determines treatment. It is little use to immobilize the limb in plaster if the cause of the pain is an osteo-arthritis of the lumbo-sacral spine, some affection of the hip or sacro-iliac joint, or, as may rarely be the case, pressure from a pelvic tumour. Carefully selected cases may respond rapidly to manipulation; all require treatment of the primary lesion; few get the complete investigation they deserve.

Incidental to the investigation will be the search for focal sepsis. Shall I be condemned as a heretic if I venture the view that the importance of focal sepsis in the rheumatic and other diseases has been sadly overrated? The frequent presence of gross focal sepsis in the absence of any systemic disease, and the failure to find it, after exhaustive search, in many conditions for whose cause it is held responsible, has long persuaded me that we have been following a false scent. The result has been the wholesale sacrifice of teeth and tonsils, and even of gall-bladders and appendices, with, I believe, subsequent ill-health due to the absence of these organs or to the results of their removal. But here again we must be cautious of allowing our scepticism to run riot. A genuine septic

focus should doubtless be treated—not necessarily by ablation of the organ concerned—but the evidence of its existence must be strong, and our promises as to the remote results of its treatment cautious.

Conclusion

It is not, however, my intention to weary you with a long catalogue of the mistakes which all of us make and which, if sown on fertile ground, are the seeds which grow into the tree of experience and, in the fullness of time, reward us with the fruit of knowledge and wisdom. My purpose in bringing to your notice these few practical illustrations of common fallacies has rather been to point, the moral and adorn the tale of the main purpose of my discourse. And this has been to urge that in our attitude to the methods we use in diagnosis and the measures we adopt in treatment we should beware equally of an excess of empiricism as of an exaggerated rationalism.

NON-SPIROCHAETAL INFECTIOUS JAUNDICE

BY

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An outbreak of infectious jaundice among the inmates of Stoke Park Colony during 1935 gave an opportunity for the study of this disease. In England the view most commonly accepted is that infectious jaundice occurs in two forms: (1) Weil's disease, a grave form which is spirochaetal in origin; and (2) epidemic catarrhal jaundice, mild in type, of which the causative organism is unknown. The ordinary sporadic form of catarrhal jaundice is regarded as a separate disease, due to an ascending cholangitis, in which exposure to cold and errors of diet play a large part as predisposing causes.

Miller¹ summarizes a more modern viewpoint when he writes:

"The old view that so-called catarrhal jaundice is due to an ascending infection of the ducts from gastro-duodenitis falls more and more into disfavour. The alternative hypothesis is that it is a hepatitis due to some unknown infection occurring sporadically or in epidemics. Viewed thus it is seen to have an incubation period of fourteen to twenty-eight days, an autumnal seasonal incidence, an age incidence from 5 to 10 years especially, and to have the power of conferring a lasting immunity from fresh attacks. The type of child with 'cyclical vomiting' seems particularly liable to this disease, and adults show much more immunity against it than children."

Hampson,² whose work Miller is discussing, points out that "epidemic catarrhal jaundice" is a bad term, and prefers "epidemic infective jaundice."

Hurst and Simpson³ consider that two separate conditions are commonly diagnosed as catarrhal jaundice. In the first there is gastro-duodenal catarrh with obstruction of the ducts, and the initial symptoms of gastritis are followed by jaundice. In the second there is a mild subacute hepatic necrosis in which jaundice is the first symptom, followed by symptoms of general toxæmia; the liver is tender and slightly enlarged, and the spleen is always palpable. In France catarrhal jaundice is regarded as a manifestation of infectious jaundice. Widal and Abrami⁴ state that "infectious jaundice occurs in three forms . . . catarrhal jaundice, benign infectious jaundice, and malignant jaundice."

Weil's disease, or malignant jaundice, is accepted as being due to infection by the spirochaete or *Leptospira icterohaemorrhagiae* of Inada and Ido. The spirochaete

That we should appreciate the value of proven facts and apply to them the highest standard of proof that we can attain. That scepticism is necessary in embracing new ideas, though it is not necessary for us to go so far as Montaigne, who wrote, "I feel we should say most times: There is no such thing." That we must never be impervious to new ideas, and if only we will say "There may be no such thing" we shall avoid the pitfalls into which credulity may lead us.

The Hippocratic aphorism which has served me as a text concludes with the observation that judgement is difficult. The late Lord Dewar once remarked that an optimist was one who made of every difficulty an opportunity; and a pessimist one who made of every opportunity a difficulty. Let us then be optimists, and strive to achieve that balanced judgement which alone can enable us to distinguish fact from fallacy and guide us into the path of wisdom.

may be demonstrated in urine or blood; the disease may be reproduced in guinea-pigs by injection of the same material, or agglutination reactions may give positive results even with dilutions of 1 in 1,000,000.

Some Views on the Causative Organism

The causative organism of "catarrhal" jaundice, sporadic or epidemic, has not been established. These epidemics are regarded by some writers as being due to a mild form of spirochaetal infection. Montford,⁵ describing an outbreak in North Leicestershire, refers to two epidemics, among others, in which a spirochaete was found, and states "the symptomatology in these and other British epidemics did not greatly vary, and it would seem that such epidemics are manifestations of the one disease, although a spirochaete or other organism could not be constantly demonstrated." Pettit⁶ mentions the many pathogenic spirochaetes which resemble *Spirochaeta icterohaemorrhagiae*, particularly *S. hebdomadis*, *S. pettiti*, *S. pseudo-icterogenes*, etc. He quotes Saesilo, who, working in Amsterdam, found that of nine cultures obtained from rats in the town seven showed typical *Spirochaeta icterohaemorrhagiae*; the other two gave serological reactions identical with those of another spirochaete which causes the spirochaetal fever of Déli. Pettit suggests from this that there may be a second spirochaetal disease in Europe in which the symptoms are less severe than in Weil's disease. This may explain the findings of Elliott and Beattie⁷ in the Selkirkshire epidemic, in which eight urines out of ten showed some form of spirochaete and only one a typical leptospira. In no case was animal inoculation successful in this outbreak.

Benign infectious jaundice occurs sometimes in association with infections of the coli-typhoid group. Boyd⁸ states "cholangitis may occasionally be a cause of transient jaundice. It is probable that the cases of epidemic catarrhal jaundice are of this nature, possibly due to the paratyphoid group of bacteria." Widal and Abrami mention many investigations into the cause of jaundice in which agglutination reactions, blood culture, or examination of the faeces revealed typhoid, paratyphoid, or *B. coli* infections. They give the following examples:

Sacquépée and Fras,⁹ Audibert,¹⁰ and many others obtained positive typhoid agglutinations in cases of jaundice. Netter and Ribadeau-Dumas,¹¹ Sacquépée and Fras, and Demanche¹² obtained positive paratyphoid agglutinations in similar cases. Typhoid bacilli in the stools of jaundiced patients have been demonstrated by Sacquépée and Fras, and Etienne.¹³ Positive blood cultures were obtained in cases of sporadic jaundice by Delachanal and Savy¹⁴ (typhoid), Lemierre¹⁵ (paratyphoid B), Widal, Lemierre, and Bénard¹⁶ (para. *B. coli*). Positive blood cultures of paratyphoid B were obtained in epidemic jaundice by Sarrailhé¹⁷ in the Dardanelles and by Cantacuzène¹⁸ in Rumania. Both obtained strong positive agglutination reactions, and both further confirmed the causative organism. Volunteers were given cultures of the paratyphoid B by the mouth; they were isolated, and all developed jaundice,