MEDICINE AND OLD ETHICKS

(WITH SPECIAL REFERENCE TO ELECTRONIC REACTIONS).*

BY

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I HOPE that you will not be disappointed with me this evening, in that I am not reading you a paper upon some academic medical subject, nor giving you my experiences in any matters of diagnosis and treatment. There are some larger aspects of our work, and of our relations to the community, upon which it is sometimes useful to reflect. Medical men are citizens as well as doctors, and the medical profession has greater privileges and deeper obligations than are represented by the daily round of visits to sick persons and the discussion in the consulting room. I propose to deal with one of these larger aspects of our professional life and conduct to-day. Not in a spirit of dogmatism, as one qualified to dictate to others, but in a spirit of inquiry. I am hopeful none the less that such a spirit may lead to some concerted view of what our attitude should be towards a question that has been in my own mind and in the minds of many others for some time past, and which has now become of such moment that I believe we are not acting in our own or in the public interest if we leave it longer unconsidered. If to be a law unto himself can scarcely be defended as a rule of conduct by the individual in his private life, it can certainly not be defended when that individual is a member of an honourable profession occupying the position in the public esteem and confidence which we enjoy as medical men. I spoke of our privileges and our obligations. We cannot divorce these two things; they are inseparable. True, there is a section of the public that looks askance at us, does not trust us, thinks of us as a close corporation existing only for our own ends. There are even those who regard the "regular" practitioner as a narrow-minded bigot who obstructs, rather than furthers, those advances in medical science and practice which tend to increase the public welfare. By them we are often accused of jealousy and bias—jealousy of the unqualified practitioner who claims to possess knowledge and skill whereby he can assuage human ills, and bias towards discoveries which emanate from outside the profession or, if emanating from within, are the product of the brains of certain of our colleagues whose methods we dislike or whose honesty we doubt. One of the most distinguished of modern writers, who will be recognized without difficulty by this very incident, recently lectured to the students of my hospital upon the subject "The disadvantages of being qualified." He made it quite clear that, in his opinion, it would be better for suffering humanity if no one was qualified at all. But in spite of these things it remains a fact that the great majority of men and women do trust us with the conduct of their health, do believe that we know more about it than those who have had no training in the schools, and know more about it even than they do themselves. This is our privilege, that they place themselves confidently and without question in our hands, that they rely upon us to heal them, and those dear to them, to save them from death if this be possible, and, in any case, to free them from pain which is the "worst of ills." In return for this great privilege, we have obligations no less great. These obligations, as it seems to me, are of two kinds. We are under the obligation of employing on our patient's behalf every piece of knowledge that can be of service in the study of pathology and every measure that can assist treatment. And we are under the obligation of being honest.

1. To most of us, seeing that we belong to no particular method or school in our profession, it is the glory of medicine that it strives to read in "Nature's infinite book of secrecy" whatever it can and by whatever branch of

science is available. We, like Bacon, have taken all knowledge as our province, both when we seek to elucidate the complex problems of disease and when we strive to adjust the patient's disordered functions by our therapeutic endeavours. Medicine is the child, not of one, but of all the sciences. The lessons of past ages have taught us to attach ourselves to no system and to no sect. If we are governed in our actions by any one principle, I suppose that principle is experience. True it is, as the Father of Medicine has said, that experience is fallacious and judgement difficult; "but this is a part of human frailty, and we have no better guide." "It is my bounden duty," said one of the wisest of the physicians of the last generation, "and I desire nothing more than to increase my knowledge on all hands and thereby to attain the ancient aspiration of our hospital to bring health or ease to my poor patients. I like the tale which Plutarch tells of one that cavilled upon a time with Capitaine Iphicrates, and by way of reproach and minding to prove that he was of no reckoning, demanded what he was. For (quoth he) you are not a man at armes, nor archer, nor targetter. I am not indeed, I confesse (quoth Iphicrates), but I am he who commands all these, and employ them as occasion serveth. In like manner the physician says, I am ready to accept help from any source, from physiology and pharmacology, but also from mechanics, optics and similar sciences, from electricity and chemistry, nay very gratefully from cookery, upholstery, ironmongery, and indeed from any source." As I have already observed, I think there is no doubt that in respect of our desire to help our patients most of us are of the same mind as Samuel Gee.

2. The second part of our obligation, and one which is expected no less of us, is that we shall be honest. "My patients like to be humbugged," said a friend to me, and it is, alas! a fact that the temptation to humbug the sick man too often comes from his own inherent desire to be dealt with other than truthfully. But for the sake of our own methods, as well as for the hope of advancing the science and the art of medicine, we dare not yield to the temptation. And as to what kind of honesty is required of us, that gifted and clear-sighted member of our profession, Sir Thomas Browne, reminds us in eloquent words that there is only one kind. "Live by old ethicks," he says, "and the classical rules of honesty. Put no new names or notions upon authentic virtues and vices. Think not that morality is ambulatory; that vices in one age are not vices in another; or that virtues which are under the everlasting seal of right reason may be stamped by opinion."

I am quite prepared to be charged with sententiousness this evening, and some will think the charge well merited. But I feel strongly that an occasional reflection given to this double duty imposed upon our profession is not amiss. And I can scarcely deal with any question of our duty in any other spirit than one of seriousness.

From time to time, both of the aspects of our duty to our patients which I have referred to are in danger of being lost sight of. The cause is not infrequently the introduction of some new fashion or theory into medical practice, which tempts us to the belief that at last science has taken a leap and we must hasten to avail ourselves of it. Some of us are urged to exploit the new idea so that we may obtain a pardonable advantage over our competitors; some of us are lured to it by enthusiasm bred in us by those responsible for its inception. Many of us follow our leader because we fear to be left behind. Inasmuch as most of these ideas have some demonstrable truth in them, follow some recognized principle which has been clearly established, and are serviceable once we recognize their limitations, the initial excesses we commit in regard to them are excusable, and more or less harmless. Occasionally, however, the idea is introduced without the stamp of scientific control, and makes claims which are of such magnitude that it is absolutely subversive of most of our orthodox tenets and most of our regular practice. We are to-day faced with such an idea and with such claims, and I am interested to learn what you think about it and what our attitude should be towards it. I refer

*An address delivered before the Manchester Medical Society on February 28th, 1924.

to the system of diagnosis and treatment introduced by Dr. Albert Abrams of San Francisco, and generally known as the Electronic Reactions of Abrams. If you will allow me to approach the subject in this way, I propose to make my first references to the matter follow my own personal experience.

Prior to twelve months ago I knew practically nothing about the electronic reactions of Abrams. A few patients had spoken about a method of diagnosis and treatment by the use of a wonderful "box," in the way patients not seldom do speak of such things, and I had scarcely listened, as one scarcely does listen at such times. On two occasions I remember doctors telling me that patients of theirs whom, after consultation, I had regarded as suffering from incurable diseases, had decided to try "electronic treatment." I had not read Sir James Barr's articles in the English medical journals, or, if I had, they did not leave anything definite in my mind; and certainly they did not interest me as they did when I read them quite recently, after certain other things had happened. I am quite ready to plead guilty to this almost complete ignorance of a subject which had evidently attracted a good deal of attention in America, though I would offer in extenuation the fact that so many medical novelties arise like mushrooms in that country, and die before we have time to interest ourselves in them, that one of more permanence is quite likely to go overlooked for a while. But the receipt of a letter from a surgical colleague, whom I had been advising on account of his own health, and whose judgement I had every reason to respect, brought the subject once more before me. I propose reading this and another letter from the same writer, because they illustrate some important points. The first letter reads as follows:

"A friend of mine has just returned from America, glowing with zeal for the *Electronic treatment*, and gave me a demonstration which astounded me. To be able to diagnose, by means of a drop of blood, cases that puzzle the wisest, is disconcerting—especially as it can be done without seeing the patient. Anyhow I wanted to know whether you would think me ridiculous if I went over to Chicago this summer for a few weeks to study the method? My own blood showed the 'usual' congenital syphilitic reaction, which appears to be universal; also a positive *B. coli* reaction and streptococcus infection of most of my air sinuses. Not a trace of tubercle. Although it looks like black magic, I can see that it is based upon sound scientific principles. Although I had never seen the apparatus before, I was able to determine the presence of an osteoma in a woman's femur, and say definitely that it was on the bone of the right side. This was done, not from her blood, but from a letter written by her, which my friend put into the apparatus! 'Yes' or 'No' over the telephone will be quite enough to decide me. This thing is said to be on its way over, and if it can do all it appears to do, it will knock 75 per cent. of my surgery on the head."

I appear to have answered this letter by saying that the receipt of it was the first intimation I had had of the existence of any mental complication in my colleague's own case. I merely state this fact to show that the sudden presentation of the electronic claims and terminology produced a kind of shock to my mind, as I suppose it does to everybody not acquainted with them. However, my friend quite properly disregarded my disparaging remarks, and went to the States to see things for himself. Here is his second letter, written from Chicago:

"You see, my 'cerebral complications' have, after all, brought me to this—the dirtiest, noisiest, most depressing place I have ever yet come across. But the Electrons of which I told you are worth it. I am only at the very threshold, of course, and unfortunately I have to leave in a fortnight for home; but already it is quite evident that this much-criticized method will be of the utmost interest (and possibly value) to me in my work, conjoined with the orthodox methods. When I told you that congenital syphilis had been found in me I was merely repeating my friend's diagnosis. This was only partly correct. I had my blood examined here, more for curiosity than for any other reason. A syphilitic strain has again been proved—but *bovine*; and I am told that this is almost universal in persons who have been vaccinated with unreliable lymph. In my case this may have been so. I am just beginning to try my hand at getting reactions from known pathological specimens. And I was encouraged yesterday by spotting carcinoma of the uterus and correctly estimating its 'potential.' To-day I was less successful, but, of course, I need practice. But I won't waste your time over my vagaries, hoping only you will forgive my playing truant in this fashion. I came here sorely against my will. This work is not at all to my taste, but I feel more certain than ever that it is going to become a force to be reckoned with. In order to familiarize myself with the technique,

I am undergoing treatment here as well as instruction. If it does good, so much the better. Dr. ——— here is very young, but wonderfully well educated, and a born teacher."

This letter arrived whilst I was on my summer holiday, but I think it was more on account of some specially urgent matters that kept me very busy on my return, than on account of this fact, that I deferred giving the matter any particular attention. Nor did I do much more than acknowledge a number of letters which I received about this time from doctors, some of them old pupils, asking me about the method. Most of these letters asked the same questions, and I will quote one because it is typical of most of them and is couched in very simple terms: "If the thing is humbug, why isn't it being shown up, and if there is something in it why is it not being investigated?" Then an incident of quite another kind happened, and I found myself suddenly faced with the real and living thing. It looked as though, since I would not go to "E.R.A.," "E.R.A." insisted upon coming to me. One day I received a request for an appointment from a lady who excused herself from putting her doctor into communication with me on the ground that she had, without his knowledge, done something that she could not tell him about until after her visit to me.

"Owing to an article I read" (I quote her letter), "and also on the advice of a friend, I wrote to a doctor at X, who diagnoses from a blood test which I sent to him, but as his report ignored the pain in the hip which I told him about, and told me I was suffering from a dreadful disease, I want to go through a proper examination to find if this is correct."

I gave the appointment and a very frightened woman came to keep it. She was suffering from osteo-arthritis of the left hip-joint, following an old injury. I found no other signs of disease during my examination. She left the electronic practitioner's letter on my table, being very glad to get rid of it. It reads as follows:

"Dear Miss F.—I have examined the specimen of your blood, and find a very heavy basic infection, and cancer of the left breast; and there are signs that this growth is inclined to spread. I am sure I can help you if you care to come down for treatment. You would have to stay in the town, and come to my clinic daily; the probable time needed for your treatment would be from four to six weeks. My fee is 30 guineas a month, payable in advance. Should you decide to come for treatment, will you please let me know, and I will make room for you as soon as possible. Yours sincerely, ———."

Miss F. was, however, only left for one week to endure the mental agony of this very serious diagnosis—an agony which was not at all assuaged by the doctor's sanguine hopes of help from six weeks' electronic treatment. For after this interval she received a second letter, as follows:

"Dear Miss F.—There appears to have been an unforgivable mistake. I can only say that I regret it deeply. You have got the wrong report—one concerning someone else, and I can only ask you to forgive me for not reading your letter over before signing it, and thereby causing you so much worry and trouble. You have a big basic infection, and a streptococcus infection of the post-nasal sinuses, to a certain extent the teeth, and some bones, especially of the lower part of the spinal column. I am telling you this so that you may know that there is nothing dreadful at all, only painful. With many apologies, I remain, Yours sincerely, ———."

This personal experience of the actual working of the Abrams school in our midst induced me not only to pay more heed to the things I heard in connexion with it, but also to acquaint myself with the methods, to read the literature, and to attend certain demonstrations. I then discovered that the men who possess the Abrams machines in this country are fairly numerous (I speak throughout this paper of registered and qualified medical men only). One or two of the machines are being used solely and entirely for purposes of scientific investigation, with an endeavour to establish definite data, quite apart from the diagnosis and treatment of patients. I am in close touch with a small group of men who are using the instruments in this way; the group includes a trained physicist and a wireless expert, and all the work is done in properly equipped laboratories. A few of the machines have been purchased or hired by practitioners who, either because they fear the criticism of their colleagues, or because the zeal which originally led them to undertake the venture has waned, keep the apparatus locked up in a cupboard, and speak of it only to their most intimate friends in whispered confidence.

If I exclude these two groups of men, there remain two important classes of workers with the Abrams machine.

1. The first of these groups, containing certainly half a dozen, but perhaps more men, consists of those who are carrying out their own investigations upon the method in conjunction with their routine clinical work, but who are not substituting the apparatus for the orthodox methods of practice. Amongst these is my surgical friend whose letters I have read, and who has his apparatus installed at his hospital, much to the concern (he now tells me) of certain of his colleagues. Although his early enthusiasm has been damped, he still believes that great possibilities lie hidden in the acknowledged complexities of the machine. In this group I also put another colleague to whom I am much indebted for facilities in the matter of demonstrations to me personally. He is a psychiatrist, and he tells me that he finds the apparatus of considerable help in his treatment of patients by suggestion—a significant admission.

But in this same group, and deserving special mention, I place a worker who has done much more than anyone else in this country to pioneer "E.R.A." investigations, and to clear away a great deal of the confusing verbiage and wild ideas surrounding the system. This worker happens—unfortunately as I think—to be a homoeopathist, a fact which will doubtless prejudice many, and lead them to assume that his efforts in the direction of elucidating physical data are not to be relied upon because of a presumed desire in his mind to link up his findings with his homoeopathic doctrine, and thus extract scientific support for this doctrine. Personally, I do not find it difficult to interest myself in his physical experiments and conclusions and to leave any applications he may make of these to homoeopathy alone. I refer to Dr. W. E. Boyd of Glasgow, who has published two reports of researches carried out under the auspices of the Beit Research Fund of the British Homoeopathic Association. A careful study of these reports convinces me that Dr. Boyd has advanced very considerably our knowledge of a few of the many factors concerned in the complex "reactions" crudely described, and certainly too hastily interpreted, by Abrams. Boyd has evidently tried to find facts, realizing that until these are established, and are capable of repeated demonstration, we are not likely to distinguish anything of value in the mist of meaningless terms and unwarranted assumptions that at present surrounds the subject. This worker has already simplified the unnecessarily complicated electrical contrivances of Abrams, and has reduced the apparatus to a form which can be described in proper technical language. This simplified apparatus Boyd calls an "emanometer."

Although it must be for physicists to criticize his work, one cannot but be struck by the scientific spirit in which he approaches the subject and the care taken to avoid false conclusions. Boyd considers that his emanometer is accurate so far as methods of recording go, but that the interpretation of the phenomena is subject to a large degree of error. He appears to have proved definitely that the coil which Abrams called a "resistance" was in reality acting as an "inductance," and that Abrams's "unknown" energy was of a well recognized character; in other words, Abrams had empirically constructed a form of wireless receiver. The inductance, acting together with the human subject as a selective receiver, picks up minute ether waves. When these waves are tuned by variation of the inductance and a variable condenser, they produce small areas, dull to percussion, on the abdomen of the subject. Boyd considers that this percussion dullness is due to local muscular contraction, and, from my own experience, I am inclined to agree with him. At the moment when—as I was informed in the course of my own percussion of the subject—a "reaction is coming through," I was nearly always conscious of an increased tenseness of the rectus abdominis beneath the pleximeter finger. Indeed, the increased resistance due to hypertonicity of the muscle was a more obvious change to my tactile sense than was the reputed change in resonance to my auditory sense. Some of Boyd's practical conclusions illustrate the cautious and temperate nature of his attitude towards the whole affair.

"Even if one allows," he says, "any definite significance to individual waves, the fact remains that the emanometer gives an analytical picture of the abnormal electrical activity at the time of the test and not a picture of pathological conditions. The name of a single disease based on the emanometer (test) is therefore in most cases an impossibility." Again, "taken by themselves these waves cannot be said to be significant of disease. They are significant of electrical activity . . . what we are detecting is not the presence of a toxin or infection but the registration of an increased normal energy. In other words, disease considered from the viewpoint of the emanometer is normal energy become abnormally active. It is the actual patient we investigate, not the infective or causative agent."

From these considerations, which seem to me to follow quite reasonably from his ascertained facts, Boyd propounds the following thesis:

" . . . a healthy person has a normal condition of electrical activity which produces numerous resultant waves of no great intensity. The introduction into the system from without of a substance or infection of which the electronic period is identical with the period of any particular substance, cellular or otherwise, would tend to enhance the normal activity until it became abnormal, resulting in an increase of intensity as revealed at test, and an increased variation on the condenser. In other words, normal patients [? persons—T.H.] are susceptible to infections which are electrically of such a periodicity as to tend to enhance the activity of one or more of the normal electrical activities of that particular patient [? person—T.H.]."

When I compare these moderate and reasoned statements with the immoderate and hopelessly irrational jargon of the earlier disciples of the Abrams school, I find it now as easy to form a definite concept of what may possibly take place as I found it difficult before. There is no longer any insult to my mind, but a hypothesis that I can readily assimilate.

This question of the existence of a reasonable hypothesis is not so irrelevant as it may at first sight appear. We have a right to demand some show of reasonableness in regard to new claims made by any method. And the present claims of the "E.R.A." teem with unreasonableness. Let me give one instance. From a drop of blood taken from the tip of a patient's finger a diagnosis is made of cancer of the left breast. By what conception can we allow that localization of a disease process to one of two symmetrically situated glands, histologically and functionally identical, is possible, when the only material submitted to examination is a drop of a fluid which bathes both glands alike, and is taken from a remote and neutral point on the surface of the body? Speaking for myself, my mind boggles at the effort of finding such a conception.

2. The second, and by far the largest, group consists of those who have made the Abrams instrument the mainstay of their daily practice. These are the electronists *par excellence*, a busy and a thriving brotherhood. Imbued with all the enthusiasm of the master, from whom, or from whose American associates most of them have learned the method direct, they scorn the sceptics' criticisms and find the day too well filled by satisfying the clamour of an increasing throng of patients, to spare time for scientific investigation or control of the method, or to render the discovery popular and accredited by giving demonstrations or reading papers before societies which exist for such a purpose. Into this group comes the doctor whose diagnosis of carcinoma mammae sinistrae was so unfortunately posted to the wrong patient. How many men there are in this group it is not possible to say, but they are probably fairly numerous, and certainly they are increasing rapidly. It is doubtful whether the increase is determined more by a desire not to be behindhand in exploiting a method which makes such a strong appeal to the credulous public and therefore increases practice, or by an honest conviction that the method constitutes a real advance in medical knowledge.

But the position raises an ethical question of vital importance to the profession. How far is a practitioner justified in using a new method of diagnosis and treatment, and charging his patients fees for it, if the criteria upon which the method is based are entirely new, and are as yet not established? In connexion with this crucial question I want to remind you that the layman is always attracted by any method which claims to substitute mechanical exactness for human judgement based upon a set of data collected, however carefully, yet by orthodox means. If,

therefore, the patient is not warned that the new method also has serious fallacies, the limits of which are not yet known; and if he is led to believe that it is an improvement upon the older system, he is almost certain to prefer it, and choose the doctor who is known to follow it. But I think the fundamental consideration lies in another direction. The patient believes the *criteria* of health and disease underlying the new system, both in regard to diagnosis and treatment, are identical with those underlying the older method. When he is told that he has a cancer, as diagnosed by Abrams's instrument, he thinks in terms of a growth having very serious and even lethal possibilities; he does not think of a mere "reaction" the meaning of which remains to be determined by proper investigation. And when he is told, six weeks later, that he is cured, he thinks the cancer no longer exists, because that is what "cure" means to him. He does not know that the sole evidence of cure is the mere absence of the "reaction" formerly yielded by means of the instrument. And if the practitioner explains the matter to him, telling him that this "reaction" is specific for cancer, he assumes that this fundamental fact has been established beyond reasonable doubt.

The electronist is often told that the onus rests upon him to prove that his method leads to accuracy of results; by which the critic means that the electronist's results must be shown to be identical with results obtained by present clinical and pathological methods. This requirement is entirely justified if the criteria of both systems are the same, but not otherwise. It is open to the electronist to explain discordant results by saying that his criteria of disease processes are quite different from the old criteria. If he does so, however, the onus rests upon him to state his criteria very definitely, and to advance cogent reasons why these new criteria should be made the current basis both of our nomenclature and of our concepts of disease. Let me illustrate. By the criteria of combined clinical, histological, and immunological investigation, we find that a certain small percentage of the general population are the subjects of congenital syphilis. By the Abrams criteria there are very few persons living who are not the subjects of congenital syphilis. If the Abrams conclusion is correct, it is clear that we must change our concept of congenital syphilis, and we must admit that literally millions of persons are infected by *Spirochaeta pallida* who show none of the criteria at present known to us, and whose blood gives a negative complement-fixation reaction with syphilitic antigen. But when John Jones is told with complete confidence that he is syphilitic by his electronist doctor, the idea in his mind is quite a different one from the idea which led Abrams to say that syphilis is the primary cause of nearly all human illness, and that most diseases can be traced back to this origin. As a matter of interest I may mention that in this country I find patients are not usually told that their blood yields this almost universal syphilitic reaction, and I have wondered why. Is it that such information would prejudice the popularity of the "E.R.A." method, or is it that the exponents of the system have decided not to follow Abrams in his interpretation of this particular reaction? I think we have a right to know which reason obtains; for if the former, then electronists are taking a big responsibility in diagnosing syphilis, and not giving the patients the chance of proper treatment; and if the latter, then the specificity of this reaction is no longer regarded as reliable, and the whole method should be put back into the laboratory for further consideration.

This brings me to one of the chief if not the chief consideration I wish to put before you this evening. Should the "E.R.A." have left the physical and clinical laboratories at all? Is not the whole thing much too premature? Would not further and more scientifically controlled experiments determine either that it is not calculated to assist pathology and therapeutics, or that help from its use is only to be expected in quite a different direction from the one at present pursued in practice?

If we may assume for a moment that such a step be taken, and that my second group of Abrams's disciples decide to forgo for the time being the dubious benefits

to their patients and to themselves of a too eager exploitation of "E.R.A." methods—as to which, not being very optimistic in these matters, I am not very hopeful—what are the lines along which research might be profitably conducted?

1. There are first of all certain questions that concern the pure physicist. Does the patient, and do his blood and secretions, give out any form of energy which can be detected and measured by known physical means? If so, can this energy be demonstrated by some objective device such as a photograph or a graphic record, so that we are able to eliminate the subjective element in determining the pitch of a percussion note or the lag of an ebonite rod? Can the human medium be eliminated, or, alternatively, how many facts can be ascertained without the complication introduced by the inclusion of the medium in the circuit?

2. Then there are questions that include both physicist and clinician. What relation does any tuned and recorded electronic energy bear to known pathological conditions? What tuned and recorded energy, emanating from patient or secretion or tissue, bears no relation to known pathological conditions, and what light, if any, do such records throw upon our present criteria of disease processes? It may not improbably transpire that, although "reactions" can definitely be established, these possess no values that can be related to known pathological states.

Electronic practitioners as such protest, and with reason, that they should not be called upon to explain the physical laws underlying their instruments so long as these instruments give consistent and helpful results. The protest is one that must appeal to all of us who are empiricists in the proper sense of that word. But the question is, do their instruments give consistent and helpful results? I have no knowledge that they do. I have a considerable bulk of evidence that they do not. I am of course supposing that consistency and helpfulness are to be judged of by the old criteria, and not by new criteria concerning which we have not been instructed. Not seldom, when an electronist is told of a wrong diagnosis, he disclaims the necessary efficiency, and says that So-and-so, who is more proficient, would not have made this mistake. But is the patient informed that he is not to rely upon the diagnosis on account of the fact that the electronist who made it is a beginner? The patient regards his doctor's "box" as being the same as any other doctor's box—an instrument of precision and incapable of error. He is not told of any personal equation being involved, nor of a long educational process, nor of the almost innumerable fallacies that are known to complicate the readings at the seance. "Surely the box cannot lie?" a patient said to me recently, when I tried to cheer him by telling him I could not verify the diagnosis of his carcinoma of his stomach diagnosed by the "E.R.A." methods. If it be advanced by the electronist in this connexion that similar difficulties and fallacies attach to most instruments used in our work, I should reply by reminding him that for no other instrument are anything like the same revolutionary claims made. Surely the confidence necessary to write to a nervous patient and tell her she has cancer of the breast, without examining her, demands a degree of proved consistency in the working of the diagnostic system which no other set of apparatus at present in use by us need ever be required to possess!

Following my plan of giving you in the main my own experiences, I will cite an instance of a gross error committed by a practitioner working with the "E.R.A." methods. I will do this by reading another letter in my possession from the doctor who so alarmed my patient. It was written to the anxious wife of a very sick man who sought the help of the "E.R.A." diagnostic method on account of the regular practitioner's grave view of the case.

"Dear Mrs. G.,—I have examined your husband's blood and found a very heavy basic infection of the *Bacillus coli* from the large intestine, and a streptococcus infection of the sinuses behind the nose. There is nothing in this which we cannot get rid of, and the getting rid of these things should benefit your husband's health considerably. My fee for treatment, which would have to be for one hour daily for a month, is 30 guineas, payable in advance, and for this examination and report, two guineas. If he wishes to have treatment, will you let me know at once, and I will try to find a place for him.—Yours sincerely, —"

Three weeks after the receipt of this epistle the patient died as the result of a large sarcoma involving the axis vertebra.

[Sir Thomas Horder went on to say that he had not had much opportunity of testing the "E.R.A." system of diagnosis in the other obvious direction—submitting specimens of blood from well marked cases of disease to expert electronists for report. A friend who was testing the method, and who was from his belief in its possibilities likely to get the best out of it, had, however, requested him to submit specimens, and he had sent him six. The results were very disappointing: only one diagnosis out of the six conformed to the diagnoses of the patients' diseases as registered at the hospital. This was not more than the law of chance might explain, when one considered that the range of gross and certain diseases was not large, and that either carcinoma or tuberculosis or syphilis was very likely to have at least one representative amongst six selected cases of unequivocal disease. But his friend was one of those who disclaimed proficiency, and did not wish this effort to be considered in the light of a scientific investigation into the merits of the method. But Sir Thomas Horder thought that his friend could not possibly object to his saying that, having watched him at work, and knowing that he was bent upon hammering out the truth of the position, his failure must necessarily make a bigger impression on the mind than did the alleged success of certain others who could not be credited with so little bias. This particular worker informed Sir Thomas Horder that he found a number of confusing difficulties and fallacies in carrying out the diagnostic tests, which made it clear that the conclusions were to be accepted with great caution. But Sir Thomas was not aware that any statement had yet appeared in the medical press dealing with these fallacies, though they must have been met with by other workers previously.]

I may say here (Sir Thomas Horder continued) that I have purposely confined my remarks to the diagnostic part of the "E.R.A." problem, partly because time does not admit of my discussing the therapeutic side of the question, and partly because the whole of the evidence in favour of successful treatment turns upon the question of diagnosis.

In conclusion, I do earnestly plead for an attitude of forbearance amongst the members of the profession in regard to this matter. For the sake of patient and practitioner alike I think this is indicated. The patient, as I have said, is suspicious of our conservatism, and inclines towards novelty and a short cut. But he is a sick man, he is ignorant, and he must be protected against himself. The doctor is keen to outstep his fellows and welcomes a new method if it promises more dramatic results than are yielded by the old. But he has great traditions to maintain, and he dare not exploit a pseudo-science. I am sure our medical societies would welcome any discussion or paper that tend to settle some of the vexed questions I have mooted; and I feel equally confident that our medical journals would welcome contributions that aim at the same end. Let those who are qualified to do so, both physicists and clinicians, continue to explore the whole field in a spirit of impartiality and with strictly scientific methods, publishing their results from time to time. Whilst these matters are proceeding I suggest that it is not ethically sound to substitute the "E.R.A." for the present form of medical practice, since this substitution misleads the public into thinking that, whereas the criteria of disease studied by the two methods are the same, the "E.R.A." method represents an improvement of acknowledged and established value.

DR. LEPINE, professor of nervous and mental diseases in the Lyons faculty of medicine, has been nominated dean of the faculty for the next three years.

THE fourth number of the *Bibliography of Industrial Hygiene* has now been published by the International Labour Office at Geneva at the price of 1 franc (Swiss). It contains a list of 409 publications on industrial health issued in all countries since the appearance of the last number of the *Bibliography*. A note of seventy new cinematograph films dealing with labour conditions and medical subjects is appended.

England and Wales.

VOLUNTARY HOSPITALS COMMITTEES.

Somerset.

THE second report of the Somerset Voluntary Hospitals Committee is lacking in evidence of actual work undertaken or of advice tendered for the improvement of the hospital system of the county. The report restates, however, the difficulties of the financial position of voluntary hospitals, and the reasons for these difficulties; an appeal is made for co-ordination, a word far more blessed nowadays than Mesopotamia, and a short summary, accompanied by tables, is given of the work and finances of the hospitals in Somerset. It is noted that at a conference of representatives of all voluntary hospitals in Bristol, Somerset, and Wilts, held in November, 1923, it was resolved to constitute a joint hospitals committee—a first step towards co-ordination. The report contains many pessimistic statements, such as "The very foundations of the voluntary hospital system have crumbled away"; "The attempt to preserve the old conditions . . . is rendering the position of the consultants alarmingly insecure"; "The isolation of London . . . prevented even the recognition of community of interest"; "The unwillingness of the cities to decentralize, and of the voluntary hospitals to co-operate with public bodies." It is satisfactory, however, to note that conferences between representatives of local hospitals are to be held this year at Taunton, Bridgwater, and Bath, under the auspices of the Committee.

Lancashire.

The report of the chairman of the Lancashire Voluntary Hospitals Committee displays laudable activity on the part of the Committee. The Voluntary Hospitals Commission announced that of the balance of the Government grant of £500,000 remaining in the hands of the Commission, the sum of £2,500 was available for the area, and after careful consideration the Committee decided that the best method of distribution would be to take five of the voluntary hospitals in the area which had large accumulated deficits on the ordinary maintenance account, and to invite them to raise £500 each on the understanding that an equivalent sum would be granted out of the £2,500. The plan was approved by the Commission, and the hospitals of Blackpool, Bolton, Rochdale, Warrington, and Wigan have benefited by it. In addition to this, the Lancashire Committee considered the question of legacy duty on bequests to hospitals, and forwarded a memorandum on the subject to various authorities. The Committee proposed that the duty should be collected by the Treasury as heretofore, but should be transferred to the Voluntary Hospitals Commission for distribution amongst necessitous hospitals. In this way it is hoped to meet objections that have been raised to the recommendation in paragraph 55 of the final report of Lord Cave's Committee. Under the Committee's scheme it is stated that exemption of legacies to hospitals from legacy duty would not relieve residuary legatees of the burden of providing the duty upon the testator's hospital bequests; and applications of a similar kind from other voluntary philanthropic organizations could be resisted, since by the setting up of the Voluntary Hospitals Commission hospitals have already been put in a position different from that of other charitable organizations. The plan of earmarking a particular source of income for a specific purpose has already been adopted—as, for example, in the appropriation of revenue from customs and excise duties for the purposes of higher education, and of income from taxation of motor vehicles for the construction and maintenance of roads. The Lancashire Committee has, so far, received no reply adverse to the proposal contained in its memorandum.

SAFEGUARDS AGAINST IMPROPER DETENTION IN MENTAL HOSPITALS.

At the meeting of the London County Council on March 11th the Mental Hospitals Committee presented a report on the safeguards which exist to prevent detention of sane persons in the Council's mental hospitals.