SOME ASPECTS OF SPECIALIZATION AND RESEARCH IN THE SERVICES.

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It is becoming more and more obvious that no one man can master more than a fraction of the accumulated knowledge, or have time to acquire the technical ability necessary for the efficient practice of more than one or two of the numerous subdivisions of applied medical science. Specialization or division of labour solves the problem to some extent. And the time seems near when the general practitioner will be the most important specialist of all, with the chief function of informing patients to which experts to go. To perform this duty adequately he will require a far wider knowledge than anyone who specializes on a single organ of the body. This may be feasible ashore in areas where the population can support a team of medical men, but in the Services the all-round medical man is still required, especially in ships and isolated districts with small populations.

Of late years the practice and study of medicine is developing more as a science than as an art. The pose of confident omnipotence formerly typical of the family doctor has largely been replaced by the diffidence of the student of nature, and the value and necessity of medical research are becoming recognized. As it has been my fortune to have been engaged about equally in general, special and laboratory practice in the Royal Navy, I presume to make a few remarks on these important subjects.

Sir Thomas Lewis [1] recently pointed out that medicine falls naturally into two main divisions—curative and progressive. The former is practically the craft of applied medicine, the latter is research. We can further divide both of these two main divisions into individual and collective medicine—general practice and public health. Public health can be divided into curative preventive medicine, or herd-treatment, and progressive preventive medicine, or herd-pathology. It must be clearly understood there are no watertight bulkheads between these departments of medicine. Anyone who administers a remedy to a patient practises progressive medicine in so far as he notes the sequence of events in a certain type of subject under a specific set of conditions. Lewis draws attention to the divergencies in the method of approach towards pure curative and pure progressive medicine. In curative medicine diagnosis is the main end-point whose object is to recognize the known in order to administer the correct established remedy, if any. The worker in progressive medicine has no interest in the known, except as a starting-point to reach out into the

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unknown. Secondly, the whole duty of the practitioner of curative and preventive medicine is to do the best for the individual, or the herd, with the knowledge and material at his disposal. To the scientific investigator the individual or community is of no importance or interest, unless as material to help the progress of scientific medicine. In so far as he is allowed any humanitarian interests at all, the researcher works largely for the future, and is glad if during his lifetime any of his work should prove useful in presenting applied medicine with new methods of diagnosis or treatment. Thirdly, there is the difference in the mental attitude of the curative and progressive worker. There is no doubt that from both the patient's and the doctor's point of view, telling the literal truth to patients is incompatible with sound medical therapeutics. If we always confessed to the truth that is in us, we should often have to say to our patient, "I do not know what is the matter, I do not believe the treatment I am ordering will do you any good, but hope it will not do you any harm, and make you more comfortable," and, paradoxical as it may seem, the more widely read a doctor is the more often he finds himself in this position. Pedantic honesty in a practitioner of curative medicine is a selfish luxury and bad practice, and may account for some of the bad reputation Service medicine used to have among certain ignorant or misinformed circles. Francis Bacon has said: "To pronounce disease incurable is to establish negligence and carelessness as it were by law, and screen ignorance from reproval." And, however much a practitioner realizes his ignorance, it is an essential part of therapeutics to deceive the patient as to the fact. More, the fact itself should never be an excuse for the neglect of patients, but a continual spur to find out what little is really known in order to place it at his patient's disposal, and to make certain he does not miss one of the many conditions in which he can help Nature, or that he does not destroy the only remedy which is helpful in every malady-confidence in the doctor. On the other side progressive medicine is science, and science is the definition of truth. As Lewis [1] says:—

"The very essence of daily observation and thought in scientific work is the continual effort to discriminate as closely as possible between what is true and what is less true or actually false. The standard of truth attained by constant and deliberate cultivation among scientific men in their work greatly transcends that in any other sphere of human thought. It is a standard that allows no statement to pass without full qualification, without full display of its limitations. It is a standard essential to progressive work, but one highly inconvenient and even obstructive in the practice of everyday life."

The qualities required for the pursuit of progressive and curative medicine are so divergent as to make it seem improbable they can be successfully blended in one individual. Nevertheless, the plasticity of the mind is such that it is possible, and I speak from experience, to enter a
ward, be convinced that a case is one of influenza, prescribe aspirin with certainty that it will be efficacious, and confidently assure the patient that he will be well in a week. Five minutes later the practising investigator is in his laboratory facing what appears to be the only interpretation of drastically controlled experiment, and becomes a flabby mass of indecision and doubt. After all, the difference in mental attitude is only due to a common psychological mechanism, a mixture of dissociation and repression which, unless we are critically introspective at the time, passes without notice, and which many happy men never discover at all.

I think many men can practise both the curative and progressive branches of medicine with success; in fact the long list of practitioners of applied medicine who have made notable contributions to progress indicates that this must be so. However, while certain types of medical research are more easy and fruitful if the investigator is actively practising administrative or curative medicine at the same time, yet the converse does not hold good, and the essentially critical attitude of the scientific investigator does interfere with his ability in the art of managing and treating patients, because it is bound to undermine his confidence in much of his daily work. In fact we all know the type of consultant, whose extensive knowledge so fills him with hesitancy and doubt that he is of little use to the general practitioner who hopes for a definite opinion. Most of us can remember the happy state of mind we were in at the time we qualified. Every statement in our textbook, or uttered by our teachers, was an unimpeachable fact. Our medical education largely consisted of memorizing a medley of facts, theories, opinions and legends, giving to them all an almost equal amount of credence. The relative probability of the truth or falsity of any statement was rarely indicated. To some extent this may have been unavoidable, seeing the mass of detail a medical student must of necessity absorb to practise the profession of applied medicine. One sometimes feels that the medical practitioner should endeavour to remain in this state of mind, by never reading anything but the last edition of his favourite textbook, and believing that every word in it is the literal truth. Many first-class practitioners do, I think, remain in a frame of mind approaching to this, and they and their patients are happier for it. And, as probably more than 90 per cent. of men never recant from the religious, political, or medical creed in which they were brought up, I presume the majority of practitioners are consciously honest, while unconsciously deceiving themselves and their patients.

In addition to the natural divisions of medicine into curative, progressive, individual, and collective, there is the more artificial subdivision into "specialisms." Keith [2] shows how this specialization is the result of the "inexorable law of evolution" which applies to medical practitioners as much as to any other group of organisms. As a result of the growth of medical knowledge, the profession has of necessity to split up into an ever-increasing number of specialists. The advantages of specialization are
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evident, but there are also disadvantages. The highly specialized doctor, just as any other highly specialized animal, is closely adapted to only one type of environment, in which he far outshines the more plastic general practitioner, who is, however, much more at home in all other types of medical environment. Under the present system of medical organization and education the practitioner generally specializes too early. In unconscious defence of his ignorance of things outside his own department he sometimes develops an air of superiority over the more generalized doctor, who has to be a wider-read and more adaptable individual or go under. Again, the breaking up of the medical profession into sub-herds, often of differing ideals and customs, has led to a certain amount of friction between the groups, which may hinder progress in some directions while advancing it in others by stimulating healthy rivalry. The following purposely overdrawn incident will show what I mean. When I was doing both ward and laboratory work, an eminent pathologist visited me in the latter environment. I had to excuse myself to go and see an urgent case. When he realized what I was going to do he said, “Good Lord! man, you do not tell me you actually have to go into the wards and open people’s bowels.” I shall never forget his tone of pity and reproach; I felt I had been thrust out of his herd of pathologists. I was a pariah—a clinician.

In certain subjects early specialization seems actually to induce pride in ignorance of general medicine and surgery. The following episode illustrates such a tendency, which should always be discouraged while the subject’s mind is still plastic enough to receive impressions. At one of the naval hospitals early in the war, a recently qualified youngster, who had started to specialize, joined up as a temporary surgeon lieutenant. He was rightly made specialist in his own subject, but in accordance with an excellent naval custom he was also expected to do some general duties and keep days on. Learning this, he “fell in” to see the Principal Medical Officer, to whom he explained that it was dangerous for him to keep a day on, as he might miss an “acute abdomen” or a case of infectious disease—because he was a “specialist.” Much to his colleagues’ disgust, he got away with it, and was relieved from days on. I may add that if I had been P.M.O. he would have kept day on and stop on, under his general practitioner colleagues, until he felt he could spot appendicitis or mumps with a degree of certainty required of the general practitioner. These little failings can be winked at ashore, where a much greater subdivision of labour is possible and desirable than at sea, but they need not be encouraged in the Services where, under the close conditions of living, they are less tolerable. In the Navy there is not the work for many whole-time specialists, because there are not enough special cases. Therefore we can avoid the narrowing effect of specialization while retaining much of its usefulness, by refusing to let medical officers specialize officially before attaining their “half-stripe” and sticking to the excellent custom of making specialist medical officers keep au fait with general medicine, and
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with administrative duties. Another excellent naval custom, which I hope will never be dropped, is that all Surgeon Commanders should serve a commission at sea before further promotion. Among certain naval medical specialists and bureaucrats I know this custom is unpopular, but for the good of the Service and senior three-stripe officers themselves, I hope the rule will continue to be rigorously enforced, especially with regard to those officers who specialize in administration. Many hospital doctors and administrators, who have been long ashore, forget how rapidly the Navy has changed and is changing, and many also seem to forget, or perhaps never knew, that they are the servants of a sea-going Royal Navy. The specialist the Royal Navy wants is a man with at least the average knowledge of general medicine and surgery with an extra training in some speciality in addition thereto, but not in lieu thereof. Moreover, if the Service is to get the best results from its medical experts, they must be familiar at first hand with the conditions of life and duty in ocean-going submarines and triple sixteen-inch gun turrets, as well as in the “Victory” and the Ark.

It is probably an inexorable psychological law that early and whole-time specialization narrows the mental outlook, however plastic the original mind. It is logical to believe that an individual who is continually thinking of one small section of medicine, and only sees patients suffering from one type of illness, cannot help attributing a greater relative importance to his own subject than it is entitled to. Analytical psychology shows how a man’s occupation, especially if it is a hobby as well as a livelihood, forms a mental complex which subconsciously influences his judgment and behaviour in completely irrelevant matters. A wise general practitioner is well aware of this, when he is loth to recommend a patient to consult Mr. So-and-So, because he fears that, whether his patient has housemaid’s knee, or neurasthenia, he will return without tonsils, appendix or great intestine, according to the surgical complex which subconsciously sways all Mr. So-and-So’s actions. The psycho-analyst himself, though he has done invaluable work in showing up the influence of the unconscious mind on the symptoms of patients and on the behaviour of the medical men who treat them, yet seems as prone to this defect as the ordinary man. What may be an example of this tendency in an eminent psychologist is Stoddart’s theory of sea-sickness. He says: “The sea is a well-known mother symbol which may be subconsciously associated with the respiratory rise and fall of the mother’s breast while the infant is taking food. The rise and fall of a boat during a sea voyage tends to remind certain people of this forgotten situation in an unconscious way, and sea-sickness with its rejection of food is a mode of repressing this infantile memory.” Candidly, as a naval surgeon and a dabbler in psychology who has given much thought to seasickness, I do not believe that Stoddart’s hypothesis rests on a sure enough foundation of fact to recommend that only recruits who were bottle-fed as infants should be accepted for the Royal Navy. However, Trotter, a
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brilliant psychologist as well as eminent surgeon, warns us that scepticism is not the hallmark of a scientific mind. That it is just as often an automatic mental defence against the painful irruption of new ideas. So perhaps I am trying to be funny at the expense of this analogy either because I am insufficiently educated in psychology or because some unconscious influence sways my judgment. Stoddart himself says that those unfamiliar with psycho-analytical investigations will reject this theory of sea-sickness. However, the illustration holds good even if the hypothesis is right, because I become the example of a mind refusing to believe the truth because some mental resistance prevents my making a logical judgment of the facts, subconsciously, using scepticism as a defence against the pain the acceptance of this novel theory would cause me.

Every specialism has its curative and progressive side. Nowadays the exponents of these two divisions tend to divide automatically into clinicians and laboratory workers. This is largely because most, but by no means all, recent progress in clinical medicine, has been secondary to laboratory experiments on animals and the training and time necessary to acquire the knowledge and technique of modern laboratory work has forced the majority of medical investigators to be whole-time professionals. As Fletcher points out, the day of the amateur is practically over, and, perhaps as a result, the community, used to getting its medical research for nothing, grudges the research worker a fair share of worldly honours and a wage comparable to that of the practising clinician who freely markets the results of his laboratory colleagues' labours. Of course, when man is civilized scientific investigation will become one of the best paid professions, but meanwhile the researcher has to console himself with doing the job he likes best, a blessing given to few in this world. And, although the lack of cash is a real hardship, yet as regards worldly honours I expect most scientific workers agree with Huxley that "The only order of nobility which becomes a philosopher is that rank which he holds in the estimation of his fellow-workers, who are the only competent judges in such matters."

The Services, contrary to the general view, offer one of the finest fields for any man with a craving for research, who yet is not sufficiently enthusiastic to slave for the best part of his life on £200 to £300 a year in a laboratory ashore. In the Service the would-be researcher will find leisure and opportunity for many types of investigation. In general the Service doctor should look on his research as a hobby and not expect a reward for it. He should remember that his Service duties as a practitioner of preventive and curative medicine always come first; he is neither asked nor paid to research. But in spite of this he will generally find plenty of encouragement from those in authority over him, both medical and executive, and even from his material, the ever-cheerful sailor—if he treats them the right way. Should he be lucky enough to produce results and get advancement thereby, he should regard this as a bonus on the advancement he has otherwise earned. It is noteworthy that in the Services
original work has been recognized and rewarded to an extent which I think is relatively greater than in the civilian profession—a point in which the organization of the fighting Services is ahead of civilian medical practice.

The man with a special bent for research will find plenty of scope for his activities, not only in well-equipped laboratories at the base hospitals ashore, but also in the spacious dispensaries of a modern ship of war, where a man good at devising makeshifts can, if he will, carry out academic laboratory and biological investigations at sea. But this is not the only work I include under research or progressive medicine. The Services offer an ideal field for medico-statistical investigations, for clinical, as opposed to laboratory, research, and for the study of epidemiological problems. Again, there is room for much research in "administration." Also there are still many interesting problems to solve as regards the food, clothing, work, and last, but not least, the psychology of the sailor. Thus progressive medical work in the Services is of a variety to suit anybody's fancy. Except when he is undertaking an investigation by order, the Service researcher is in an enviable position: he works to amuse himself only, he can choose his subject, do as much or little as he likes; it does not matter if, as often is the case, his work leads nowhere. He can publish or not, but if he thinks his results of Service interest, there are his own journals grateful for copy. In fact the Services are the happy hunting ground and one of the last strongholds of the amateur medical researcher. I use amateur in its literal sense as meaning one who does something for the love of doing it—not one who does something worse than a professional. I write this as an amateur hoping to encourage others to take up a most absorbing hobby. We amateurs spell research with a small r, we do not expect to make any discovery of general importance; we do not even mind if we discover things that have for years been general knowledge to those better informed. We are only amusing ourselves, but if something useful emerges from our pastime so much the better, but that is not the main object of all investigators. Although one may not wholly agree with the professor who at the end of a lifetime spent on some academic research said, "Thank God I have never done anything practical," yet, in these sordid times, one cannot but sympathize with his sentiment. There is, however, much investigational work which promises useful results to those who prefer to be practical. There are many problems of little outside general interest, the solution of which would be of value to the Service.

One could ask many conundrums of domestic interest to the Royal Navy, some of which should be answerable if anyone took the trouble to try to find out. There is plenty of progressive work which can be done at sea without any knowledge of out-of-the-way medical literature or special training in laboratory technique.

Now, although the majority of Service research workers are working for
their own amusement, if they publish serious results they should stick to the rules of the game, and make every endeavour to observe the highest standard of literal truth. I shall, therefore, discuss one or two common fallacies and tendencies which spoil much good work and should be avoided in scientific literature. There are two main methods of trying to find the causes of events: observational and experimental. There is really no hard line separating these methods. In the purely observational method, the human agent is passive. He waits for Nature to set the experiment, and records carefully what happens; when Nature has set enough experiments which appear to control each other, the observer makes his deductions. This is a very slow method of progress, as it may be centuries before Nature sets enough of the right experiments or the observer notices the salient points in them to give him the true interpretation he is seeking.

In the experimental method the observer is active; he sets his own experiments, which are small imitations of Nature, and numerous control experiments; he alters condition and environment to suit his fancy and may find out in days things that would have taken centuries of passive observation, or could never have been discovered at all, without the observer’s active interference. It is only within the last fifty or sixty years, with the advent of the bacteriological era, that experimental pathology has bulked so largely as a separate subject to applied medicine. The experimental method has only limited applications to human beings; but it has been found that the experimental method can be employed in studying disease in animals, and, in a way, it is surprising that the phenomena observed in experimental animals are as often as not found to hold good for man. Moreover, preliminary trials on animals and the experience gained in the technique of experimental science, are allowing a freer and more legitimate application of direct experiment on man himself. The application of the statistical methods, better record keeping, and Lister’s gift to surgery, have also given the observational method a fresh lease of life. Before this renaissance of medicine, the observational method had about exhausted itself, and medicine was to a large extent a subject like religion.

The unquestioned acceptance of the pronouncements of authority and of the printed word, however desirable in religion or law, is incompatible with progress in science. No statement of any man, however great and learned, is to be taken as true just because that man made it. Here we may note the use and importance of references. In discussing scientifically any controversial point it is no use saying that Professor Highbrow said “this is so” unless you say why he said it, or give a reference to the original work from which he came to his conclusion, so that any sceptic can judge the evidence himself. Of course, we have to use and quote authority on any subject when we have not sufficient training or education to judge for ourselves. For example, if I want to make a statement which brings in a point in astronomy I am careful to consult and quote a well-known living
authority on the subject, such as, say, Professor Jeans, who, in the opinion of his colleagues, is up to date in all recent astronomical work, and whose conclusion on the point in question is accepted by the majority of astronomers, who are the only group capable of judging the evidence. Provided I have done this, I do not worry if any bacteriologist or surgeon, however eminent, any politician, or journalist, however popular, contradicts me on that point in astronomy, any more than I would worry if the greatest of astronomers criticized my views on the biology of the diphtheria bacillus. On the other hand, I am severely shaken, and review my position very carefully when Dr. J. A. Arkwright does so, because he is one of the greatest living authorities on bacterial variation. In your own subject, where you are capable of weighing the evidence for yourself, you may find what in your opinion is a reliable observation by a practically unknown investigator. Such work you are perfectly at liberty to use in support of your own theories, even if it is contrary to the pronouncements of the leaders of your own subgroup of the scientific herd. These few remarks indicate the proper use of authority in scientific work.

Now for a few words as to how it comes about that tradition and authority may hinder progress in knowledge. There has always been a strong instinct in any human herd to worship their dead leaders and respect old herd customs, and the medical profession is no exception. Far be it from me, a naval officer, to decry this tendency in everyday life. Without some such instincts no gregarious species of animal would survive. But because hero-worship is such a deeply ingrained mental character, its influence on scientific work must be carefully watched. We honour the scientific pioneers of the past none the less if we carefully re-examine their work and theories in the light of modern knowledge. And all traditions and herd customs, however admirable, require constant overhauling to ensure that they fit the ever-changing environment. Some present-day writers suggest that there has been little progress in clinical medicine which is not due to the older traditional methods, and that the experimentalists are getting a bit above themselves. Crookshank [7], for example, rebukes Harley Street for “The verbal quackery which allows us to pretend that we are nearer the ultimate understanding of life, death and disease than was Hippocrates.” But I think such indictments are scarcely just, for the average modern investigator in ward, theatre, office, and laboratory, approaches his subject in a humbler spirit than the ancients, because he realizes more fully the complexity of biological mechanisms, and admits his complete ignorance of ultimate causes. Although it may savour of blasphemy to say so, what little I have read of the old masters of medicine does not lead me to believe that they were blessed with any greater intelligence than many who are alive to-day. Among those recently dead I would place Pasteur, Lister, Robert Koch, and Manson, on a higher plane of intelligence and attainment than Galen or Sydenham.

One might say that of recent years a large part of progressive clinical
medicine has been occupied with the attempt to re-edit our textbooks, and
discover what statements are proved, what are probably true and founded
on facts, and what are definitely false or mere pious opinions unbased on
any accepted facts. As an instance of the unreliability of the older text-
books, I will give the extraordinary statement which used to appear in
edition after edition of a popular book on surgery, namely, that a man
with a urethral stricture had a corkscrew-shaped stream when he urinated.
As a student, with an elementary knowledge of hydrostatics, I could not
believe it, and at the next opportunity I watched two or three stricture
patients urinate. Though the volume and initial velocity of the stream
was less than normal, it descended in the parabolic curve and continued in
the same vertical plane demanded by the first law of motion and the theory
of gravitation. This absurdity illustrates a thing the scientific investigator
must never let himself do. It might have been legitimate for the originator
of the corkscrew hypothesis to have published his opinion as a speculation,
and wait for the critic to slaughter him; but, seeing how easy this opinion
was to confirm or refute by observing the next case of stricture, or experi­
menting with a piece of bent tubing, he would have been wiser to have kept
his theory to himself till he could test it himself. Where he committed
the "unforgivable sin" against science was to publish a hypothesis as a
fact. Of course this is a very gross error, selected to illustrate a principle,
but many statements in medical literature which are presented as if they
were proven fact, have a low probability of being true. As the great
investigator Sherlock Holmes said to that rather unintelligent practitioner,
Dr. Watson: "It is a capital mistake to theorize before one has data.
Insensibly one begins to twist facts to suit theories instead of theories to
suit facts."

A class of work for which our standardized population, living under
identical conditions of nutrition and environment and disciplinary control,
forms admirable material, is the testing and comparing therapeutic or
prophylactic agents. This form of work, unless carefully controlled, is
especially liable to the great post hoc ergo propter hoc form of fallacy, which
permeates and vitiates so much attempted research work in medicine that
no excuse is necessary for bringing it up again. It is most interesting to
note that we are all perfectly aware that a subsequent event is just as likely
to be "in spite of" as "because of" a former event.

Trotter [4] has been so much impressed by the ubiquity of the post hoc
fallacy that he thinks it the expression of a "characteristic cerebral
function," a kind of conditioned reflex. If two events occur frequently
enough in fairly close sequence, the mind automatically makes the first the
cause of the second, without requiring a logical reason for doing so. There
is no doubt that it is a specific criterion of man to be uncomfortable if he
cannot supply a cause for any event. Therefore any "cause" becomes
better than none. The history of primitive magic, religion and medicine
proves this. The real cause not being self-evident, it is easy to substitute
a striking or exceptional recent event as a "cause," and thus ease the acute mental anguish of not knowing why, and hence the ubiquity of the post hoc fallacy. The absurd lengths to which the "post hoc" argument can be carried is best seen in the uneducated and mentally weak. I once asked a reservist his reason for objecting to vaccination. He answered that he was the only survivor out of three brothers, and the only one who had not been vaccinated in infancy. I asked him what his brothers died from. He said that one had been killed in a street accident, and the other had died from pneumonia some fifteen years after the vaccination, which their brother firmly believed was in some way responsible for both their deaths.

In therapeutics a host of alleged remedies have been recommended merely on the grounds that patients generally recovered after their administration. And, seeing that many such remedies may have been given as routine for generations and recommended by the highest herd authority, any inquirer who suggests that the patient may have recovered in spite of them is generally snubbed for questioning the experience of his betters. But if a man has always given castor oil to his appendix cases, what experience can he have of oilless appendicitis? Because the type and severity of a disease show marked deviations from the average at different times and places, there is practically only one reliable method of testing the value of therapeutic or prophylactic treatment which will eliminate the chances of falling into the post hoc trap. This is by taking all the admissions of the class to be treated as they turn up in hospital or sick bay, and treating alternately, one by the new and the next by the old procedure, until a sufficiently large number of observations has been collected to be of statistical significance. The severity of symptoms, the duration of illness, the mortality, and number of sequelae in the two groups are then carefully compared. One may note that the smaller the variation in the behaviour of test and control group, the larger will be the number of patients that must be treated before any difference in the two series can be fairly attributed to anything more than luck. There is no better example of the successful application of this method than Leonard Rogers' working out of the treatment of cholera.

For many years opium was the routine treatment of cholera, given, I believe, as much in the belief that it increased the patient's chance of recovery as that it made his passing more comfortable. First, by the above method of using alternate admissions as a control series, it was discovered that opium prejudiced a man's chance of recovery. The routine use of opium in the past must have killed thousands of cholera patients who would otherwise have recovered. Subsequently, by the use of the same principle Rogers perfected the hypertonic saline treatment of cholera.

Mackenzie [8] has said that "the principles of modern therapy are the same as those which have been in use from time immemorial—the giving of a remedy whose action we do not understand for a condition of whose nature we are ignorant." Like so many epigrams or aphorisms, this is true according to how literally we interpret it. The optimist would say
salvarsan cures syphilis, that we have a fair idea of how the remedy acts and of the nature of the disease. But the pessimist would be justified in replying we are not certain if syphilis is ultimately eradicated by salvarsan treatment, and that the scientific description of its action and of the pathogeny of syphilis is still far from complete. But even if the latter view is accepted salvarsan remains a valuable empiric remedy, because it has been proved according to the strictest of scientific standards that under its influence the early and observable lesions of syphilis will disappear more quickly than if left to themselves. It does not matter in practical medicine if treatment is empiric or "rational," provided there is a reasonable probability that any beneficial change in the patient, noticed after its exhibition, is due to the remedy and not in spite of it. The difficult question for the practitioner to decide is whether he should use the multitude of drugs or procedures which have been alleged to be remedies merely on the grounds that some patients got well after their use, but which have not been proved to do good, and may interfere with a patient's natural tendency to recover. The use of some traditional remedy which appears more or less innocuous is probably justified because of the beneficent effect any placebo may produce by suggestion. Nevertheless, it is certain that the application of drastic measures in the past, merely because they were the traditional practice, caused much unnecessary suffering and many deaths. But, with the patient and his friends clamouring for one to do something, and with the physician's natural desire to help, the attitude of masterly inactivity is hard to maintain. And I think, even to-day, the conscientious doctor is more prone to over- than under-treat his patients. I can illustrate this principle from the work of Alexander Bryson [9], a distinguished past Medical Director-General of the Navy, a man whose efforts deserve more recognition than they have obtained, a naval surgeon fifty years before his time, to whom the sea-going population owes not a little, if only for his work on the control of yellow fever in the Royal Navy. Bryson, in his book on African Fevers, severely criticizes the methods of treatment in vogue at the time, which consisted of copious bleeding, shaving and blistering the scalp, forced exercise, and, worst of all, mercury pushed to salivation; as much as 120 grains of calomel were given as a single dose. Bryson points out that, although if a man recovered from yellow fever he ought to have a rapid convalescence, yet the poor wretches who recovered often took months to regain their health, if they were lucky enough not to die from mercurial poisoning. He condemns all these measures as unscientific and would replace them with absolute rest, cinchona bark and tepid sponging, if indicated. Bryson concludes his indictment of the treatment in vogue in the Navy of his time with a striking paragraph, as true to-day as when written in 1847.

"In most men's minds an over-anxiety begets a disposition to do too much, while, in the absence of a proper reliance in the salutary effects of nature, they impatiently resort to change of measures, which can only tend to harass and disturb the patient."
Truly, after reading Bryson's account of the medical practice of his time, one can sympathize with Molière's jibe at the profession, when, in answer to Louis XIV's question, "What does your doctor do for you?" he replied: "Sire, we converse, he orders remedies, I do not take them, and I recover." I will introduce my last subject with the remarks of another French satirist, Voltaire, who said of a certain physician: "He is a jolly good fellow and knows as much as the rest, and when he sees my teeth are falling out and I am suffering from scurvy, he says I have a scorbutic affection." Words and phrases can often be different and yet mean the same thing, and vice versa they can be the same and mean something quite different. Progress in medicine is greatly hindered unless there is no ambiguity about what a writer means. Voltaire's sarcasm illustrates a common failing. Doctors frequently consider that they have made a clever diagnosis and patients are satisfied if a synonym for a symptom is used. The loose use of the terms rheumatism and neuritis to describe any sort of pains whose etiology eludes us, is one of the best examples of this deplorable habit. In the Services we can stick out for a more honest standard of diagnosis on our official returns, on which we can surely afford to say: "I do not know." When I first joined the Service it was the custom that every patient must have a diagnosis, and the P.M.O. of my ward had all the tickets put out every Wednesday morning to see that the diagnosis was entered on each ticket. The Service category of P.U.O., though it has been a subject of much sarcasm, has everything to recommend it. It is honest and does not vitiate any conclusions a statistician may draw from sick records when it is strictly adhered to. In a recent paper which MacArthur read on indefinite fevers in the Tropics it was pointed out the unravelling of some of the many problems concerning this group was likely to remain impossible so long as many medical practitioners indiscriminately called fevers they could not place with reasonable certainty, enteric, influenza, dengue, malaria, sandfly fever or spirochetosis, according to the fancy of the moment. If progress is to be made in the prevention and treatment of such conditions, or of any condition, the diagnosis must approach certainty, and all doubtful syndromes must be honestly reported as undiagnosable, but carefully described in detail. We can at least be honest on official sick returns if we cannot be so to our patients. There is another way words and phrases lead to misunderstanding in medical literature: So many words have changed their meaning or are used by different classes of workers with different meanings. In a paper before this Section [10], I showed how different usages of the words "typhoid" and "enteric" had led to much confusion in medical literature. Another example of a word that causes endless difficulties and conveys a widely different concept in ward and laboratory is "virulence." When such doubtful expressions have to be used it is well for the writer to define as carefully as he can what he himself means by them. Huxley gives a good example of how the same word with the passage of time comes to mean two different things. The Latin word...
"spiritus" means "breath," which the ancients considered the most refined part of man. Hence, when the alchemists first distilled alcohol from wine they called it spiritus because they considered their distillate the most refined part of wine. Therefore, says Huxley [6], "We use the same word for the soul of man and a glass of gin." The precise meanings of words and phrases are important because one of the most difficult things in the world is to convey in words the exact shade of meaning the author hopes will be read into them. One has only to teach a little, to write a little, or compare an abstract with an original paper, to realize how easy it is for a student or reader to get hold of a concept which the teacher or author had not the slightest intention of conveying. In attempting original work, if one has to refer to someone else's investigations the original paper must always be consulted. Although abstracts are useful in finding papers bearing on one's own subject, yet in at least a third of the occasions where I have read both abstract and original paper, the author conveyed to me a different shade of meaning from that which he appeared to have transferred to the abstractor. I have seen reviews of my own efforts in which what I thought the important point had been omitted or misinterpreted. This was more likely my fault than the reviewer's, but it serves to demonstrate the real difficulty which many men find in writing unambiguous English. In official paper work I have always found that the most arduous part of my duty was translating the rough draft of a report into the King's English.

The highest possible standard of accuracy in diagnosis combined with a candid confession of inability to label a condition when such a standard is unattainable, is essential if progress in medicine is to be advanced by statistical methods. The environment of the Services is eminently adapted as a field for medico-statistical research, but it is no use pretending a lot of our vital statistics are as accurate as they might be, chiefly because medical officers will not confess their ignorance. In another address the importance of at least an elementary acquaintance with statistical methods and normal psychology was emphasized, if only to avoid the common arithmetical errors which often vitiate otherwise good work, and in order to realize that the mind is an instrument more sensitive to prejudice and emotion than to logic or fact. The points I have tried to make in this rather disconnected discourse may be summarized in the form of a short catechism which the would-be seeker after truth should always put to himself when confronted with any statement to which he is not personally indifferent. Do I believe—or disbelieve—this thing either, because I want to do so, because it is the custom of my herd and my heroes to do so, because of a post hoc argument, or because I have misunderstood the author? In this way I think one may reduce to a great extent, though never altogether, the subconscious bias that sways all our thoughts and actions. Finally, these remarks were made only as regards Science, who demands of her servants that, irrespective of their own or anyone else's emotions, they eternally struggle to unveil her cold and dispassionate sister Truth.
far it is advisable to carry this principle into everyday life I do not pretend to know. My own irrational reason tells me that suppose it were possible to step out of that phantasy which each of us knows is reality, we should be intolerably miserable, if not mentally dead. Life is only bearable because the mechanisms in our unconscious mind enable all of us, each in his own way, to be certain that black is white. This automatic action defends our consciousness from that merciless goddess Truth, and enables most of us to adapt ourselves fairly comfortably to any herd or environment into which we have been flung by that irresponsible jester Fate. Therefore as regards everyday life I would take the advice of that wonderful old psychologist King Solomon, who says: "Be not righteous overmuch, neither make thyself overwise. Why shouldest thou destroy thyself?" (Ecclesiastes vii, 16).

REFERENCES