STRAIGHT AND CROOKED THINKING IN MEDICINE*

BY

RICHARD ASHER, M.D., F.R.C.P.

Physician, the Central Middlesex Hospital

As doctors we like to be men of action as well as thinking men, but, however much we act, every action is preceded by a certain amount of thought—or ought to be. It is worth paying attention to the way we are thinking, studying whether our reasoning is sound or unsound, whether we have any faults of thinking, whether we are unconsciously using methods of thinking which are illogical or even irrational, and seeing if we can improve the quality and effectiveness of our thought.

Thouless in his valuable book, *Straight and Crooked Thinking*, classifies thirty-four dishonest tricks of argument (and very ingenious they are too), but his work is largely devoted to political thought—a field in which the quantity and variety of crooked thinking are of quite a different order from those of medicine. In considering medical crooked thinking, I have purposely not used much systematic classification, because one form of crooked thinking common in medicine is caused by overclassification and the forcing of facts into the compartments provided for them, whether they fit or not. I am therefore considering straight and crooked thinking in medicine under the headings of therapeutics, statistics, causes, words, etc., but without formal classification.

Most medical thinking is based roughly on formal logic, a subject which is rarely studied to-day, though it is used unconsciously in much of our reasoning. Logic teaches a man to recognize what conclusion can be drawn from a series of statements (called premisses), and if logic is correctly applied to true statements it must infallibly follow that the conclusion drawn from them is correct. For instance, if we consider the two premisses (1) all cats are mammals, and (2) all mammals have hair, then it follows inevitably that all cats have hair. Those of you who have learnt formal logic will recognize this as a syllogism in the mood of Barbara. Similarly, if we take the statements (1) all cats are quadrupeds, and (2) all dogs are quadrupeds, it does not follow that all cats are dogs, because there is what logicians call an undistributed middle term—the term quadruped is not distributed entirely to dogs—only some quadrupeds are dogs, not all of them. The use of logic can be extended to cover a much larger number of premisses. Here is an example with three premisses from a book on logic: (1) no one takes in *The Times* unless he is well educated; (2) no hedgehog can read; and (3) those who cannot read are not well educated. From this it follows conclusively that no hedgehog takes in *The Times*.†

Therapeutics

In medicine most of our premisses are the facts found by research workers, and many of the conclusions from these facts become translated ultimately into ideas about the cause and treatment of disease. Research proceeds now at so great a rate that it is essential that reasoning must keep

*Chairman's address to the Middlesex County Medical Society, on September 24, 1953.

†This example is quoted from Lewis Carroll's *Symbolic Logic*, which is recommended as a diverting book providing stimulating exercise in logical thinking as well as a refreshing allowance of Carrollian humour.

pace with fact-finding. As much trouble must be taken in reasoning from facts as it is in establishing their truth. Though medical data may not always be subjectable to the rigid application of formal logic, most views on therapy have behind them some attempt at a logical argument. The premisses and the arguments are not always expressed openly, so that crooked thinking is often concealed behind an impressive and persuasive array of words (particularly in brochures accompanying proprietary remedies). It is a profitable and useful exercise for all those who wish to think straight to analyse reasoning and weigh evidence by exposing the bare bones of an argument, and see how it looks. Do we not sometimes find that we are being influenced by arguments of this kind: (1) small people do not grow enough, (2) anterior pituitary makes rats grow, (3) therefore small people need anterior pituitary; of this sort: (1) pregnant rats without vitamin E miscarry, (2) therefore women who miscarry need vitamin E?

Let us consider some of the premisses again. (1) Anterior pituitary makes rats grow. Can we deduce anything from this? Can we even say because anterior pituitary makes rats grow that undue smallness in rats is due to lack of pituitary. Not necessarily, although it sounds quite reasonable. We are really saying if A causes B, then lack of A causes lack of B. Let us try this argument on something else. By the same process of reasoning we can argue living at a great height causes polycythaemia, therefore anaemia is caused by living at a great depth. Can we even argue that, because rats deprived of vitamin E miscarry, giving vitamin E to rats will prevent them miscarrying? If lack of A causes B, will plenty of A prevent B? Again a little thought will show how fallacious this argument is—for instance, lack of water causes rats to die, but giving them plenty of water will not make them immortal.

I am not arguing from this that pituitary extracts have no place in the treatment of dwarfism, nor vitamin E in the prevention of abortion, but I do say that a very different set of premisses and deductions are needed to give a logical basis to their use.

The logical approach to therapeutics is first to make a hypothesis from available facts from which we can deduce that some substance might benefit a condition, and then proceed to a controlled clinical trial to find whether it really does do so.

In other words, when we consider a line of treatment we start with the question "Should it work?" and finish with the question "Does it work?" Too many treatments are still advocated merely because there is a theoretical reason why they should work and without a practical controlled test being done to see whether they do work. For instance, because creatine excretion is increased in muscular dystrophy and glycine by mouth increases the supply of creatine to the body, the administration of glycine to sufferers from muscular dystrophy was advocated for a good many years without any convincing evidence that it was effective.

Are we not prone to accept a drug as effective in treatment because it ought to work rather than because it does work? Undoubtedly we are. Is there not a tendency to be convinced that because a drug reduces gastric acidity it does heal gastric ulcers, that because a drug increases cardiac output it benefits cases of heart failure, or that because a drug kills micro-organisms in a Petri dish it will kill them just as well in a living patient? The theoretical and laboratory tests for the effects of treatment are an essential part of clinical research and are responsible for many brilliant advances in therapeutics, and doctors must be on guard against being converted too readily to a line of treatment by theoretical arguments until these are supported by practical clinical tests.

Clinical Trials

When we are organizing practical clinical tests is it not crooked thinking to assess the success of treatment by measurements of associated factors not intimately related to the disability? Splenectomy, for instance, may be highly

beneficial in cases of congenital haemolytic anaemia, yet measurement of the fragility of the cells after splenectomy would show no improvement. Phenylbutazone may relieve a man's rheumatism enormously without necessarily bringing down his sedimentation rate.

There is no illness in which clinical improvement has been so variously assessed as in rheumatoid arthritis. As well as sedimentation rates we have instruments to measure the angular range of movements, graduated metal rings to measure the circumference of joints, dynamometers to measure the strength of grip, eosinophil counts to see what cortisone is doing to the blood, and yet anyone who has worked for long with rheumatism finds all these tests of little value compared with the direct measurement of the disability. Here in the cortisone clinic we may record weekly the time a patient takes to button up her stays or to lace up her shoes. We see how long she takes to walk the length of the ward, and note whether she can brush her hair or peel her potatoes.

Perhaps to younger people these tests seem less scientific because of their homeliness, but there is really nothing unscientific about them. Helmholtz said that all science is measurement, and many of these homely data can be measured quite informatively—the volume of soup spilled by a patient with tremor can be measured, the time to do up buttons can be charted, the amount of potatoes peeled in a standard time can be weighed, and so on.

Another error of thinking which profoundly influences therapeutics is to believe that improvement after treatment is due to treatment, the *post hoc ergo propter hoc* argument. Stated as bluntly as that, it seems obviously invalid, but hundreds of remedies are advocated on little better argument. In truth, the reasons a man may feel better after a pill are numerous—he might have felt better anyway, he might have felt better because he had such faith in the pill, he might have felt better because you had such faith in the pill, or he might really be improved by the pharmacological action of the pill. The best evidence of the effectiveness of a remedy is to have randomly allocated groups of patients—one treated with the substance, one with its inert counterpart—and nobody (doctor, nurse, or patient) having any idea which is the treated and which the untreated group. Now, the nature of treatments and the numbers of patients with particular illnesses make such ideally controlled experiments not always possible, but there are many treatments in common use to-day which have never been subjected to such testing—many drugs whose efficacy has never been scientifically demonstrated.

Use of Vitamins

If I had to suggest one particular field of therapy which needs to come under the scrutiny of statistical clinical trial it is the use of vitamins. More than two million pounds' worth of vitamins are prescribed each year under the National Health Service, and evidence that they do much good is very scanty. Are we not accepting arguments of this kind: (1) people deprived of vitamin B have sore tongues, (2) therefore everyone with a sore tongue needs extra vitamin B; (1) people deprived of vitamin C heal their wounds slowly, (2) therefore everyone with a wound needs vitamin C. These arguments alone are not rational—for instance, people deprived of salt for long enough may become mentally confused, but not everyone who becomes mentally confused needs salt.

The laity, too, have got the idea that as vitamins are essential to life any food that is full of vitamins is extra good for them. Subconsciously, I think they have adopted a sort of mathematical crooked argument that A is good, therefore 2A is twice as good and infinity $\times$ A is infinitely good. This is a dangerous argument. Cobalt, for instance, is (in minute quantities) essential to life, so is sulphur, yet a diet saturated with these substances might prove fatal. An even more dangerous use of the argument that 2A must be twice as good as A is, if one person has a moderate supply of wisdom and administrative ability, that ten people (that

is to say, a committee) will function with ten times that wisdom and administrative ability. Those of you who have tested this hypothesis in practice know how sadly untrue it is.

But to return to vitamins—the reason why they provide such a glorious field for the polytherapist and the commercial chemist is that they are more or less harmless (apart from very heavy doses of vitamin D). Therefore any doctor can prescribe them with the argument, "If you have vitamin deficiency, they will do you good; if you haven't vitamin deficiency, they can't do you harm—therefore logically we might as well prescribe them for everyone." This argument is dangerous for these reasons: (1) by extending the argument we would find ourselves giving every member of the population an enormous number of vitamins and drugs; (2) if people are given vitamins they receive an impression that their diet is defective; (3) the cost to the country is enormous; and (4) people receiving a form of treatment (even if it is prophylactic) are more liable to believe they are unhealthy.

In our attitude to preventive medicine we have got to be careful that too much of the "can't do harm might do good" argument does not lead to an excess of prevention. We must not be so busy preventing disease that we have no time to enjoy freedom from it.

I read once in an American health pamphlet that every father returning from his day's work should remove his coat and wash his hands in disinfectant before touching any of his children. Perhaps there may be a logical case from the hygienist's point of view, but if that went too far we might finally reach this sort of situation: "Oh, father, lift me up; there's a band marching past the window." "Certainly, my boy; just wait while I gargle and put on my mask and rubber gloves."

But I must return to my theme, which is that in therapeutics there must be good evidence from controlled statistical trials that what is being given is doing good. Here I should like to issue a warning that the mere use of statistics is no guard against crooked thinking. The danger of crooked statistics is that its fallacies are less likely to be noticed because of the mixture of awe, suspicion, and reverence with which statistical thinking is regarded by most of us.

An Example

Here is an excellent example of statistical crooked thinking. An investigator published an article showing that of 200 epileptic subjects, 24% had had infantile convulsions in the first two years of life, whereas of 200 normal subjects only 2% had had infantile convulsions. He went on to argue that the pronounced difference between the 24% in the epileptics and in the 2% in the control group made it clear that convulsions within the first two years of life ought to be taken as a manifestation of epilepsy and that any child having convulsions in infancy should be treated with anti-convulsant drugs for several years. The argument appears most convincing, doesn't it? The fallacy is not at all obvious. The incidence of epilepsy in the population has been left out. Now, this is about 1 in 400. So among 40,000 people there would be 100 epileptics, 24 of whom had infantile convulsions. But among 40,000 people there would be 800 normal people (2% of 40,000) who had suffered from infantile convulsions. So it would mean treating 800 people, of whom only 24 had epilepsy—in other words, submitting 32 normal children to prolonged anti-convulsant therapy in order to make sure of treating one epileptic early in life. I mention that article in detail to show the danger of being swayed by impressive figures.

Causes

Another perversion of thought applicable to statistics is to believe that a statistical relationship between two things implies a causal relationship between them. It does not do so. It is very probable that statistics would show erythroblastosis foetalis to be twenty times less common among

the offspring of opium smokers. This is no argument for encouraging opium smoking in the maternity wards. It depends on the fact that opium is largely a Chinese habit and that 99% of Chinese are rhesus-positive, and so cannot produce erythroblastosis foetalis. Similarly, if rheumatic fever is significantly more common in those who have sucked dummy teats in childhood, it may only mean that rheumatic fever is commoner among the poor, and so is dummy-sucking. If A varies with B, it does not show that A causes B.

Logic and the Laboratory

I now ask you to consider an attempt at rational thinking applied to the use of laboratory investigations. As regards tests of function, are we not inclined to argue, (1) this test measures the conjugation of benzoic acid; (2) the conjugation of benzoic acid is a function of the liver; (3) therefore this test measures the function of the liver? Might we not equally well argue, (1) this factory produces smoke; (2) this test measures the amount of smoke produced; (3) therefore this test measures the productivity of a factory? The point is that livers and factories both do a number of things, and measuring one of these things does not measure all of them.

Also it is in the ordering of laboratory or radiological investigations that rational thinking is so necessary. It is a salutary exercise in mental discipline to catechize oneself when ordering any medical investigation, saying, "Why do I order this test? What am I going to look for in the result? If I find it will it affect my diagnosis? How will this affect my management of the case? Will this ultimately benefit the patient?"

It would be only rarely that we could answer all these questions satisfactorily, as, for instance, in ordering a Wassermann test and a Lange curve on the cerebrospinal fluid of a patient suspected of dementia paralytica. Perhaps it would be a good idea to have a space on every laboratory form in which the doctor had to state exactly why he had ordered a test. I believe if answers were honestly filled in we might get this sort of thing:

(1) I order this test because if it agrees with my opinion I will believe it, and if it does not I shall disbelieve it.

(2) I do not understand this test and am uncertain of the normal figure, but it is the fashion to order it.

(3) When my chief asks if you have done this or that test I like to say yes, so I order as many tests as I can to avoid being caught out.

(4) I have no clear idea what I am looking for, but in ordering this test I feel in a vague way (like Mr. Micawber) that something might turn up.

(5) I order this test because I want to convince the patient there is nothing wrong, and I don't think he will believe me without a test.

Words

Finally, I want to consider the effect of words on thinking. Words not only provide a vehicle for conveying thought, but even provide wings for its flight which make it travel either straight or crooked.

One trouble with words is that if there is a word for a particular thing we become convinced that such a thing exists: this trouble makes precise work so difficult, particularly in the field of psychiatry.

If there is doubt whether, say, a patient is suffering from mania or catatonic schizophrenia, the observer is inclined to think there must be an answer—he believes these two diseases exist as nosological entities. Yet, as there are no truly objective tests to settle the matter, it might be wiser to regard such terms as descriptions of behaviour with no more privilege to exist as diseases in their own right than have conditions such as restlessness, talkativeness, elation, and the like.

It is often helpful to straight thinking to pause and consider what we are meaning by a word, for we may find it is playing tricks on us. Take the word "typical," for in-

stance. If we hear in a chest those musical rales that may occur in a pneumothorax and we describe them to students as typical, we may convey to the student they are common, whereas we really mean they are classical (always mentioned in books) or diagnostic (making the diagnosis certain).

A similar self-catechism is helpful if we use words like natural, unnatural, normal, or abnormal, or the word "purpose," and if we ask ourselves exactly what we mean by them we may realize how uncertain we are what we are thinking.

Another source of crooked thinking caused by words arises from the bestowal of a name indicating a pathological process when describing only a symptom or syndrome. We call the common cervico-dorsal myalgia of our out-patients "fibrositis," but we have no notion whether it is caused by inflammation of fibrous tissue or not. We call the symptoms following gastrectomy "the dumping syndrome," though there is still no general agreement regarding their cause. Socrates said, "He who first gave names, gave them according to his conception of the things which they signified, and if his conception was erroneous, shall we not be deceived by him?"

One word which encourages crooked thinking is becoming increasingly popular at the moment—the word "group." We say this probably belongs to the allergic group, the psychosomatic group, or the collagen group, without having any very clear idea of the boundaries or contents of those groups. We use them to cloak our ignorance, feeling that once we have cast diseases into these vast receptacles, these aetiological dustbins, they are satisfactorily accounted for. I believe that those brave enough to lift the lids off these bins and poke about among the rubbish there may find clinical salvage of inestimable value.

Another word is to be deplored because it allows woolly thinking to flourish. This is the word "imbalance." The word could be expunged from the medical vocabulary without any loss. Whenever we read of autonomic imbalance or endocrine imbalance we are in for a piece of nebulous thinking. It is all right to suggest a disease is associated with overaction of the sympathetic or underaction of the parasympathetic, or due to too much suprarenal cortical secretion or too little thyroid secretion, but to leave it imprecisely labelled as due to imbalance is a travesty of thinking that should not have survived the discipline of a scientific education.

Conclusion

I must apologize for the rather vague way I have wandered from one subject to another. All I have tried to suggest is that we ought to be increasingly critical of our own and other people's thinking, to analyse all arguments that our books, our teachers, and ourselves are using; to seek out crooked thinking and avoid it, and to follow the path of straight thinking, wherever it may lead us, guided by those helpers of whom Kipling wrote:

I keep six honest serving men,
They taught me all I knew;
Their names are what, and why, and when,
And how, and where and who.

A medical mission comprising two doctors, two technical health assistants, and two male nurses accompanied the 3,173 pilgrims who went to Mecca in 1953. It was provided with an adequate and varied stock of medicaments, surgical equipment, and a large refrigerator. More than 6,000 consultations were given (skin diseases 29%, digestive troubles 10%, surgical cases 5%, malaria 3%, miscellaneous 53%). The general state of health is reported to have been excellent. No case of infectious or epidemic disease was notified either during the voyage or after the return of the pilgrims. There were 32 deaths (1% as against 2% in 1952) due to sunstroke, diseases of the circulatory system, and debility in aged persons.—W.H.O. *Weekly Epidemiological Record*, No. 31, 1954.