SUBJECT AND METHOD IN MEDICAL RESEARCH.

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Many people in the Medical School hold and foster the belief that to do research work is desirable and useful, no matter what the nature of the investigation may be. Although, on superficial inspection, this idea may appear to be indisputable, it actually rests on very specious and misleading principles. If it were nothing more than a theoretical idea, there would be no object in criticising it; but it is carried into practice, which results in a most lamentable waste of time, since a large amount of work is done that has no value at all.

Some are guided by the maxim that all knowledge is worthy of acquisition. This cannot be true in the realm of medicine, where worth is decided by usefulness. If it were true, it would not be necessary for anyone to restrict himself in any way concerning the subject he should choose for his investigations; and we should deem it a justifiable expenditure of energy to determine the average number of hairs on the dorsal surface of the middle finger of European males at the time of puberty. However, it is blatantly obvious that such an investigation would be absurd, but only because whatever might be learnt would be quite devoid of the possibility of application to any problem of the least biological importance. Except that their sound bears to the ear a certain awe-inspiring dignity, such items of research as a study of the action of hydrocyanic acid on human teeth, or the measurement of a thousand Bantu ulnae to find a relation between the size of the olecranon and that of the whole bone, are just as free from practical value as the example given above: and analogous subjects are very popular in the Medical School.

We admit whole-heartedly that knowledge without practical utility can yet be most commendable, provided that it extends the outlook and deepens the understanding; but we fail to detect anything conducive to increased breadth of mind in subjects of the type to which we have referred. It is also often asserted that knowledge of any kind is worthy to be sought if it gives the seeker pleasure. We may agree with this; but it is important to remember that the accumulation of scientific information has progress as its objective, and it is therefore not meet that scientific laboratories should become a playground for dilettantes.

Another common argument depends upon a wrong idea concerning the proper method to follow in research. It is usually expressed thus: “Many matters have never been investigated. It is well for us to probe into them for that reason alone, for we do not know that we may not strike something of supreme importance.” The following parallel will show how unprofitable this principle is. Let us suppose that the police force, as a routine procedure for securing criminals, began to pounce upon citizens at random in order to examine them and see how they fitted in with known facts relating to various malefactors. It is quite possible that several criminals would be caught, some of them dangerous; but many more would escape the vengeance of the law, while much labour would have been wasted in the examination of innocent people. In the same way, labour is relatively fruitless in science when it is undertaken in the hope of happening on the momentous in a fortuitous selection of subjects.

It may be regarded as a universal rule that systematic procedures lead most successfully to the attainment of ends. The scientific method cannot be systematically employed unless certain fundamentals are understood. In the first place, the purpose of science is to reveal the laws to which phenomena are subject. Where the laws relating given phenomena are unknown a problem exists. Now, in medicine, knowledge is gathered in order that the laws governing dysfunction in the human being may become known, for all efforts in medicine are ultimately directed to the solution of one cardinal problem—the elimination of disease. This problem resolves itself into two smaller (but still immeasurably large) ones—the prevention of disease and the cure of disease, and these are nothing but collective expressions for an endless host of problems.

Surely, then, the logical procedure in medical research is to select a problem having a tangible bearing, however indirect, on the eradication of disease; to perform the investigations necessary for its unravelling; and to use the laws discovered for the disclosure of further laws. When this method is used negative findings are no less precious
than positive ones, and needless wastage of time, work, and thought is circumvented, for only those researches are carried out which the solving of problems may demand.

All that we have said refers quite as much to investigations on the part of groups as to those of individuals. The impending examination of Bantu children by the Medical School is a timely example. Neither tradition nor inspiration, but a clear understanding of what practical questions require most urgently to be answered, should determine what the details of this examination shall be.

It must not be inferred that our criticisms apply to all research in the Medical School, for much that is done is most praiseworthy from every point of view. On the other hand, fallacious ideas about what is necessary in medical research are common everywhere, and are not infrequently manifested in medical literature. But that does not entitle us to let the matter rest. It is our duty to the people of a continent to find the means of delivering them from the miseries of disease; and we shall do justice neither to our capabilities nor to our opportunities, until all of us distinguish the kernel from the husk, and study the essential and ignore the contingent.

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