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GENERAL IDEAS IN MEDICINE*

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On August 2nd, 1775, John Hunter wrote to Edward Jenner a short letter, which contained what are perhaps to-day his best-known words. Jenner had asked for his opinion on a problem of natural history, and Hunter suggests that the question could be settled experimentally; and then he breaks out into one of his brusque, impatient phrases, "But why think, why not try the experiment?" In this small outburst the piety of later times has sometimes seen profundities it might have surprised its author to be notified of, and has even given to it the status of an aphorism pregnant with the wisdom of the newer world. We may incline to regard these rather high-flown interpretations as belonging to the sphere of Hunterian Orators, where such tropical growths naturally bloom. Nevertheless, the phrase on which they are founded is a text well suited to guide some reflections on the relation of thought and action in the advancement of knowledge. Any such discussion must traverse treacherous ground, and we should therefore not be afraid of limiting ourselves to elementary considerations or of defining our terms with care.

In the life story of mankind two implements of knowledge can be plainly seen at work. I have already called them thought and action, and our first tasks must be to make clear what is meant by them and to identify some of the many names by which they may be known. By action as contrasted with thought I mean man's direct dealings with phenomena by way of observation and experiment, or in the broadest sense of the word, experience. By thought as contrasted with action I mean the rational contemplation of experience and the integration of it into generalized expressions; I shall speak of the use of reason or of the rational mind and of abstract thought as having the same meaning.

It is one of the most curious features in the history of man that his sole uniquely human character, the rational mind, has borne a relation to the advance of knowledge that has always been capricious, often ambiguous, and sometimes hostile. It seems so natural to take it for granted that reason has been the steady friend of truth that a statement of the facts has an air of paradox. If, however, we are to fight shy of the paradoxical, no inquiry into the history of knowledge will take us far. Not less surprising than the enigmatic relation of reason to truth is perhaps the lateness of the date at which it first seems to have occurred to man that the intellect could be deliberately used. Cultures of great vigour and elaboration had climbed to their zenith and died before we get our first glimpse of reason at work on human destiny in the great days of ancient Greece. There we see the rare spectacle of a new and powerful instrument in

hands fully competent for its use. The world has come to agree that there never has been anything else quite like the efflorescence produced by the happy union of the Greeks' discovery of abstract thought, and the exulting brilliance with which they applied it. Up to that moment man's only implements for discovery and for preserving its fruits were the practical arts. The characteristic limitations of that method had made him the deferential slave of phenomena; henceforth he was to be in some sense their conscious master. It will be our task to try to get a clear idea of the instrument that could produce this revolution, and then to trace its influence in later times.

The Nature of Abstract Thought

The discovery that we first clearly find in use by the Greeks was that the qualities of objects can be thought of as separable from them. Such abstracted qualities can be recombined and handled within the mind with a facility and fruitfulness impossible in our dealings with actual objects themselves. If complete abstraction of the qualities of the outer world were possible our reconstruction of them in thought would be as valid as the originals, and a true experimental science within the mind would be feasible. It seems probable that the Greeks thought this to be the case, and excusably in the first flush of their discovery. That error has beset the progress of knowledge for two and a half millennia, and compels us to regard the discovery of abstract thought as in some degree a Grecian gift in the old ambiguous sense. For it is a stubborn, though, unfortunately, an elusive, fact that valid abstraction from phenomena in general is only a limited possibility. As far as we know number is the only quality that can be completely abstracted from objects, so that the experience of number within the mind is just as valid as the experience of number attached to external objects, and of course far more easily subject to experiment. Thus the science of number is the single science that is at once fully abstract and fully experimental. The science of spatial relations runs it very close, and it was two thousand years or more before suspicion arose that the abstract space of the mind was not exactly the same thing as space in general. The Greeks were quick to see how well number and space lent themselves to the new method, and very obviously reserved a special reverence for the two corresponding sciences—not always without queer results.

As we move away from number and space abstract thinking becomes less and less able to represent usefully the actual world. In biology the most elaborate abstractions are apt to be mere crude diagrams, and, through disproportionate emphasis, easily pass into caricature. The decline in the effectiveness of abstraction with increase in the multiformity of the phenomena is a familiar fact.

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The implement so useful to Galileo in attacking mechanics is scarcely recognizable in the hands of Herbert Spencer generalizing biology, even if we grant the powers of the two men to have been equal. It is not, however, the limitations of the method that concern us chiefly here. What is more important in the history of science and of medicine is the tendency of abstract thinking to interpose between the inquirer and the phenomena and to give a diminishing importance to the latter. One universal law has always governed the evolution of knowledge; it is that progress is directly proportional to the closeness of the relation of the inquirer and the facts. Now facts unfortunately are not the natural diet of the mind. They are laborious and often undignified to collect; they are apt to be formless, ugly, and even nasty; they dirty the fingers, they smell and sometimes bite. How different from the noble shapely, and above all well behaved, conceptions of the mind, which are so manifestly of a higher order of reality! The fastidious Greeks were very sensitive to this difference and not a little apt to look down on the mere base collection of facts. Even the tremendous Aristotle was not quite untouched by this intellectual queasiness.

Reason Enthroned

Faith in the ultimate supremacy of reason must tend towards a dogmatic rationalism. This tendency was not without its ill effects on the pursuit of knowledge by the ancient Greeks, but was to a great extent compensated for by their unique curiosity and aptitude for free thought. It could not keep such a people out of contact with facts, as we see repeatedly throughout the long period from the clinical observations of Hippocrates and the biology of Aristotle down to the experimental physiology of Galen. But when it was handed on to minds less happily situated and less richly endowed, and had been developed into the flawless and invulnerable rationality of the scholastics, rationalism became a much more formidable barrier to the progress of knowledge than it had been as the relatively innocent foible of the Greeks. Mediaeval rationalism was strong enough to embody in a reasonable structure all known facts. It needed no new ones, and could scarcely have regarded the collection of such as other than foolish and unnecessary.

One of the most deeply enigmatic events in human history is that after the time of Galen science, and with it medicine, underwent no large change but that of decline for a full thousand years. I shall not yield to the temptation to speculate about the causes of this astonishing coma of the human spirit. Many explanations of it have been propounded, and have been for the most part as confident in their presentation as they were incredible in their substance. While bluntly admitting that the fact is unexplained, we may note that in its circumstances there were features that may have been related to it. The most obvious of these was the development of dogmatic rationalism into a system which could give a coherent and consistent account of nature, of man, of the spiritual world, and of any other kind of world that might be supposed to exist. The second and far the more mysterious feature was a lapse of that kind of curiosity for the outer world that makes phenomena interesting in themselves. This is the fundamental impulse of science; without it science is impossible, with it science cannot fail ultimately to appear. Whether or not we may suppose the eclipse of curiosity to have been causally related to the long night of science, it is certain that no dawn could have broken until that eclipse had ended.

The way in which science was born again confirms this principle. It has been shown by Singer that the renaissance of science in the sixteenth century had been pre-

pared for by a long period, during which a reawakened interest in the outer world was gathering momentum. He takes the memorable year 1543, in which were published the masterpieces of Vesalius and Copernicus,¹ as a convenient date to mark the first full light of the scientific renaissance; and he gives evidence of a steadily growing body of work produced in the previous two hundred years that proves interest in external phenomena once more to have been alive. Thus the renaissance of science was of necessity and in fact preceded by a renaissance of curiosity.²

Rationalism at the Renaissance and After

The revival of science was not due to or accompanied by any corresponding decline in rationalism. The giants of the Renaissance appear to have been untroubled by any suspicion that reason was not the ultimate arbiter of truth. The symmetry, the acceptability, in short the reasonableness, of a doctrine still seemed the major and final evidence of its truth. No doubt had arisen of the assumption that the sense of rationality corresponds with a similar order in the outer world. Medical theory could still base itself on the four elements, the four humours, and the four temperaments as fundamental truths; and it was long before astronomy ceased to prove that the orbit of a planet must be a circle because that is the perfect curve. All that had happened was that facts had re-established their birthright. There were no qualms about how troublesome these upstarts were likely to become. They were, of course, to prove a decisive factor in the situation.

From the time of the Renaissance the accumulation of fact was to proceed at first with a moderate, but at length with an overwhelming acceleration. The faith in reason underwent no debilitation from within, but began to suffer a certain dilapidating erosion from without. Rationalism lost its scholastic severity and perhaps gained thereby in attractiveness. The change introduces us to a third period in its evolution, which in contrast with the ingenuous rationalism of the Greeks and the austere rationalism of the Middle Ages we might call latitudinarian. By this time medicine had become distinct enough for us to consider it more or less apart from science in general. We have reached the period lying between the massive discoveries associated with the names of Vesalius, Servetus, and Harvey, on the one hand, and the not less revolutionary work of Pasteur and Lister on the other. Here we have a stretch of roughly 200 years, comprising the eighteenth and about half of each of the adjacent centuries. Discovery, of course, proceeded actively throughout this time, and the accumulation of knowledge soon became enormous. The causation of disease was still obscure, for no single fundamental element of it had been established. The field therefore was as free to the theorist as it had been in ancient Greece, but there were two fresh elements. These were, first, the now very great store of facts, and, secondly, the decline in rationalistic rigour.

The characteristic feature of the period was the exuberance with which doctrine flourished. Almost every eminent medical personage—who might well at the same time be making solid contributions of fact to medicine—was the exponent of a theoretical system of disease. This system, devised by its inventor according to some inspirational method of his own, was often as detached from reality as it was precise and dogmatic. It is impossible to review the innumerable systems that sprang into life and often so surprisingly survived; I can only mention a few names taken almost at random from Garrison's well-known work,³ and trust to be able to convey some of the quality of their doctrines.

Early in our period there is Georg Ernst Stahl, whose life almost exactly corresponds with the interval between

the death of Harvey and the birth of Hunter. He taught an animism which regarded the body as a mechanical puppet, whose functions were maintained by the direct action of the soul. Misbehaviour of the soul was therefore the cause of bodily disease. It seemed to be a necessary consequence of this theory that drugs could have no action on the body, and like a good rationalist Stahl denied that they had. His responsibility for the many animists and vitalists who followed him is perhaps less heavy than for the phlogiston theory with which he burdened chemists, for this theory is perhaps the most perfect example in the world of ideas of the mysterious viability of the false. Then there is John Brown, whose life coincides roughly in time with that of Hunter, and who was the author of the famous Brunonian system. This product of reason is said to have been remarkably complete and consistent; it divided diseases into sthenic and asthenic, and treated them respectively with opium and alcohol, drugs to which Brown himself, less tough than his system, early succumbed.

The air of caricature never fails to show itself in the products of reason applied relentlessly and without correction. The observation of clinical facts would seem to be a pursuit of the physician as harmless as it is indispensable. Reason, however, could scarcely stop at so elementary a phase as this, and it seemed irresistibly rational to certain minds that diseases should be as fully classifiable as are beetles and butterflies. This doctrine found its most eminent cultivator in the great Sydenham, but bore perhaps its richest fruit in the hands of Boissier de Sauvages. In his *Nosologia Methodica*, published in 1768, the year of Hunter's appointment to St. George's, this Linnaeus of the bedside grouped diseases into ten classes, 295 genera, and 2,400 species. Towards the end of our period these particular developments met an opponent in Broussais, who lived till Lister was 11 and Pasteur 16. For Broussais disease in the sense of the nosologist had no existence. Diseases were for him consequences of local irritation and resulted in gastro-enteritis, which was the essential pathological lesion of all maladies. Broussais' quality is shown by his aphorism *La nature n'a aucun pouvoir de guérison naturelle*: believing this, he knew that recovery depended solely on the exertions of the physician. Since the condition he had to contend with was always an irritation and could be met by reducing the patient, he set himself to starve and bleed with a dreadful rigour. The lapse of a hundred years has made this doctrine seem no more than gruesome balderdash, but it was not without plausibility for the contemporary world. In fact, no less a surgeon than the great Dupuytren was a believer, and was accustomed to add to his mere surgical powers of reducing his patients the sterner measures of his colleague.

If these instances give a fair sample of what the intellect was doing for medicine for 200 years, it is not perhaps surprising to find Hunter about the middle of that period exclaiming impatiently, "Why think?"

The Decline of Rationalism in Medicine

When we pass on to consider the state of affairs that obtains at present the first thing that strikes us is that no one is likely to make that exclamation now. I can imagine no reproach less likely to be needed. A re-orientation has occurred towards what can be done by rational speculation for the advancement of knowledge. We can scarcely bring ourselves to listen to anyone who is not actually engaged in investigating facts, and to call an author speculative is a severe censure.

This change has been the result not of deliberate choice but of the pressure of circumstances. These circumstances have been the overwhelming success of the method of experiment in recent times, and the fabulous

rate at which the accumulation of ascertained fact has progressed. There can be no reasonable doubt that the decline in the influence of rational speculation has coincided with a period in which the increase of knowledge has been rapid beyond example. Even if we can be sure, as I think we can, that the increase of knowledge was in no sense caused by the decline in speculation, we are left with but another confirmation of a conclusion that has become increasingly clear throughout this discussion. That conclusion is that at no time right up to the present day has medical doctrine borne any close and functional relation with medical discovery. If we handle this rule without trying to enforce it absolutely, and with a reasonable breadth of interpretation, we shall find it applies to every period of medical history. Although this conception is not generally and consciously recognized, and as far as I know has never even been specifically enunciated, it has long been acted on both in medicine and in science. There can therefore be no purpose in failing to admit that, in general, discovery has been the result of action rather than of thought. Fact has led to fact, observation to observation, experiment to experiment; and there has been little or no long range guidance from rational contemplation and foresight. The relation of doctrine and discovery has not only been without co-operation, it has often been hostile; for the progress of knowledge has again and again been arrested or embarrassed by supposedly rational arguments and accepted principles. The historian is apt to be indignant at the obstacles to new knowledge offered by the censure of theologians and the persecution of ecclesiastics. Theologian and ecclesiastic have no doubt done their best, but the effect of their utmost zeal has been insignificant in comparison with that resulting from the conscientious use of the rational mind.

The Function of Rational Thought in Medicine

If we accept the view that the cultivation of the theoretical side of medicine has on the whole delayed progress, should we not rejoice in the fact that that kind of study has been almost wholly destroyed by the pressure of experiment and its great success? In that case we should be able to give a definite answer to the Hunterian interrogation with which I began. Why think? Why indeed! The situation, however, is not quite so simple as to be settled in that way, and there are at least two strong reasons why a permanent abeyance of the theoretical, speculative, and rational element in the pursuit of medicine could not be regarded as wholly satisfactory.

In the first place experience seems to show that a branch of knowledge strictly limited to experiment and without any kind of speculative admixture tends in time to lose its inspiration and drift into a dry and rigid orthodoxy. Some such decline was perceptible in the physical sciences towards the end of the nineteenth century, and there can be little doubt that a strict reliance on experiment alone would in the long run have a similar deadening effect on scientific medicine. In the second place medicine is, as we shall point out more fully later, a composite subject. One of its elements is an experimental science, but a large part of it obeys the very different discipline of a practical art. In consequence it has often to deal with and act upon incompletely definable situations, and to develop the faculty of practical judgement on imperfect evidence—an activity characteristically absent from an experimental science. As long, therefore, as medicine continues to be so largely an activity of a non-scientific kind, every faculty of the active, rational mind is to be desired in the practice of it. But as we have already been at pains to point out, the exercise of the rational mind in the attempt to integrate and theorize medicine has been abundantly tried, and has always proved singularly ineffective, if not harmful.

The issue from this dilemma is a very obvious one, although one which there seems to be a strange disinclination to take. It lies in dispersing the delusion that practical thought is easy, needs no training or precaution, and is available forthwith for anyone who turns his mind to it. Psychology has shown how little these expectations are likely to be realized, but it has not accepted the complementary duty on the positive side of producing a definite method of elementary instruction in effective thought. Among the reproaches, just and unjust, to which that science is exposed this is one of the more serious, that it has failed to show us by precept, and especially perhaps by example, how to think. It would be presumptuous in me to pretend to the ability even to enter on such a task, but one or two fundamental principles may be mentioned as the more obvious of the foundations of such an undertaking.

The physical organs and aptitudes of the body do their work only under definite conditions, and in using them no one doubts the need for practice and for acquiring a sound technique. The intellect, on the other hand, seems to its user to be an apparatus already perfected, needing no exercise or instruction, but sprung into being ready to carry out its functions without flaw or friction. In actual fact it is as much a biological product as the bodily organs, and subject to similar conditioned functioning; it is as squeamish in its way as the stomach, and as selective as the kidney or choroid plexus. Some of the restrictions on the working of the rational mind are probably structural and therefore inveterate, like the inability to conceive space in more than three dimensions and possibly others we do not even suspect. The practically important ones, however, are functional, and can be reduced or in some degree eluded through awareness and training. It is a rather crude but substantially true description of these obstacles to free thought to say they are all due, more or less directly, to the interpenetration of all thinking—even the most completely abstract—by the needs, the fears, the wishes of the thinker. The dispassionate intellect, the open mind, the unprejudiced observer, exist in an exact sense only in a sort of intellectualist folklore; states even approaching them cannot be reached without a moral and emotional effort most of us cannot or will not make. There are few terms in such deep disgrace nowadays as altruism, and perhaps justly so, for it has been the excuse for a good deal of silliness. Nevertheless, something in the nature of altruism in the plain sense of the word and with no mystical implication is necessary for effective thought. For there can be no approach to truth without some threat to the thinker's personality.

Simple Tasks for Thought in Medicine

Happily these altitudes are as much beyond my theme as beyond my capacity, and I turn with relief to a humbler task. We shall probably all agree that medicine permits and calls for a great deal of practical thinking. The first topic that suggests itself is in some special need of sound and rational doctrine happens also to be the most elementary; it is that of the fundamental definition of what precisely medicine and the work of the doctor are. We are much concerned nowadays about medical education and the designing of curricula. It would appear that fundamental to all such considerations should be clear conceptions of the nature of the subject itself. This is particularly necessary and a little difficult because the subject is a composite one, and considerations very relevant to one of its elements are apt to be confusing if inadvertently applied to another. The constituents of which medicine is made up are readily discernible: they are three—a practical art, an applied science, and an experimental science.

Medicine as a Practical Art

It is always readily agreed that medicine is an art. The indispensable task of defining precisely what that phrase means is, however, less frequently undertaken. Medicine is a practical art in the same sense as is the work of the farmer, the smith, or the joiner. When this primary stage of definition is reached it becomes clear that definite qualities and modes of activity must be possessed by this element in medicine. These are of great practical importance, and it is surprising that they have had little exact notice. Now that the prestige of science is so high the statement that a great part of medicine still retains the status of an art is often made with a note of apology. Nothing could be less justified by a realistic sense of cultural values. The method of the practical art was the first instrument forged by man for the subjugation of chaos. At the dawn of civilization the preservation of knowledge was far more important than its discovery. The accidental fruits of experience and the creations of genius could be saved from an infallible oblivion only by being preserved in the precepts and tradition of a practical art. The superlative need for preservation made the arts inherently conservative, for there was, and is, no unequivocal difference between the change that was progress and the change that was decay. New knowledge was therefore accepted as reluctantly as old custom was given up.

An art carries its possessions in precepts and rule of thumb which are applied to individual cases in the light of a trained judgement. It does not possess principles of general validity automatically applicable: that is the method of applied science. An art is taught first in precepts and rules, secondly in experience of its material, and thirdly—and most important—by example. The method of apprenticeship is thus the keynote of education in the practical arts, because it brings the pupil into familiar contact with his material and gives him the constant example of his teacher in the actual things he himself will ultimately have to do. English medical education has long been recognized as having a characteristic product. This quality, whether we regard it as good or bad, has been due to medicine being regarded as essentially a practical art, and to the consequent predominance of apprenticeship in some form as an educational method.

If we have grasped correctly the nature of the practical arts we shall be able to specify to some extent the attitude of mind and the kind of thinking necessary for the satisfactory practice of them. It is commonly said that one of the chief objects of medical education should be to make the student think scientifically. The saying is perhaps as good an example as could be found of the need in which medicine stands for the exercise of the critical mind. To think scientifically may be supposed to mean one of two things. First, it may signify the adoption of a general habit of thought induced in, and characteristic of, those who practise experimental science. Unfortunately, however, it is not possible to show that the scientific worker outside his job displays more wisdom, insight, and practical judgement than anyone else of the same general capacity. Secondly, to think scientifically may mean to use the kind of mental process necessary for the satisfactory pursuit of experimental science. The scientific worker among other qualities must have an especially severe standard of evidence and proof; he must draw no conclusion that is not strictly justified by the evidence, and he must be content to leave in suspense any decision for which the materials are not quite complete. Now the last thing a doctor is free to do is to exercise the scientific suspense of judgement, and he scarcely ever makes a decision that is justifiable

on strictly scientific grounds. The advice to think scientifically would seem, therefore, to risk paralysing his judgement rather than activating it.

The truth appears to be that what the user of a practical art needs is less the strict and limited instrument of scientific method than what may be called a soundly cultivated judgement. This requirement is more difficult to specify and much more difficult to secure. Apart from inborn capacity, it seems to depend on familiarity with the material of the art, otherwise experience, and on a broad and sound general culture which, while including a proper awareness of science, is by no means limited to it. The ancient and honourable art of medicine is being increasingly and inevitably pressed on by applied science, and suffers as well from misunderstanding and loss of prestige. It remains, however, the backbone of medical practice and indispensable to mankind. There is therefore an especial need to-day that its characteristic mode of activity should be understood, and should not be confused with those of the other elements that make up the complex of medicine.

Medicine as Applied Science

The conversion of the practical arts into applied sciences is a characteristic and familiar process in modern civilization. The rate at which this change is going on is often the subject of enthusiastic and even excited comment. To the sober realist, however, it is clear that the rule of science in medicine is still not much more than strictly local and much qualified. The cases are very few in which general principles can be applied to the individual instance with the direct precision of an engineer designing a dynamo. The diagnosis and treatment of errors of refraction, certain cases of bacteriological and of chemical diagnosis, and others of physical diagnosis and treatment, with the dietetic deficiencies, make up the examples of nearly pure applied science. Elsewhere methods of precision must be very strictly subject to the art of medicine if they are not to become a mere snare. The affectation of scientific exactitude in circumstances where it has no meaning is perhaps the fallacy of method to which medicine is now most exposed. When we observe how fully the quack has assimilated the language of science it is easy to see the need for exact definitions lest we ourselves fall, with the worthiest intentions, into something not much better.

Medicine as an Experimental Science

When we turn to contemplate the element of experimental science in medicine, we find that the need for some effort of definition is not less than it is elsewhere. Experimental medicine, or, as it has been more conveniently named, clinical science, is at a stage when the clear recognition of it as an independent form of scientific activity—if such it really is—is peculiarly important. If it is a science capable of being pursued and developed as such, it is a competitor for support on equal terms with the other sciences, and occupies a field particularly likely to attract endowment.

Now it is often supposed that the case for the independence of clinical science is met by pointing out that the scientific element in medicine is merely applied physiology and applied pathology. From this not very recondite truth it is concluded that any further benefit to medicine from physiological and pathological discovery can come only from work done by professed physiologists and pathologists. This view seems to ignore the way in which the sciences are distributed and have come into being. Independent sciences have arisen in the past, not apparently according to whether people said they must or

they must not, but through finding a natural field and doing good work in it. According to their fields of work the sciences can be divided in a broad and simplified way into two groups. There are what we may call the *general* sciences, such as physics and chemistry, by which phenomena of a certain *kind* are chosen for study. Then there are the sciences we may call *topical*, by which phenomena of a certain *distribution* are chosen for study. Belonging to the second group are such sciences as astronomy, meteorology, and geology. The fact that they could all be described as applied physics and chemistry does not in the least impair their continued independence. These sciences have readily assimilated new discoveries in physics and chemistry that were relevant to their work. They have, however, not been content merely to wait on progress in these sciences, but have been ready to undertake physical and chemical researches on their own account in problems presented by their material, and often with the happiest results. It is certain that meteorology and geology would not be in their present healthy state if they had not been prepared boldly to invade the realm of the physicist or chemist when occasion arose.

The analogy between the position of these disciplines and that of clinical science is close and instructive. Clinical science takes as its field the study of sick people, thus choosing a range of phenomena forming a natural group of the highest importance and interest, and no more arbitrary from the theoretical point of view than are those chosen by the meteorologist and geologist. The chief sciences that are general in the sense we have defined in regard to clinical science are physiology and pathology. While taking full advantage of any appropriate discovery in these sciences, it need no more wait on the advance of them than geology has waited on the advance of physics and chemistry, but is bound to undertake physiological and pathological researches of its own when they become relevant to its purpose. The art of medicine will long remain the chief implement of the practical doctor. Admirably adapted as it is for the conservation of knowledge it is in its very nature ill suited for the discovery of new truth. I venture, in conclusion, to recall a classical example of this fact and its consequences.

In 1745 James Lind described the cure of scurvy by lemon juice; and in 1840 Steinhäuser, that of rickets by cod-liver oil. If these doctrines had been enunciated by teachers of great prestige they might have become accepted clinical precepts and slowly acquired empirical stability. They did not have that good fortune. In spite of the desperate urgency of the problem of scurvy in the eighteenth century, and that of rickets in the nineteenth, no proof or disproof was established, because it is not in the nature of a practical art to seek for certainty in the scientific sense. The truth remained undistinguished among the innumerable opinions current about these diseases for 150 years in one case and eighty years in the other. As late as the admirable *Encyclopaedia Britannica* of 1911, the most authoritative opinion inclined to ascribe rickets to an intestinal toxin and scurvy to an unknown microbe. The practical art of medicine alone knew the urgency of the problems, and actually for many years possessed the key to both. It was therefore in an extraordinarily favourable position for a direct attack on them. The method alone it, as an art, did not and could not possess. That belonged to physiology, which for long years had no interest in the problems, and approached them at last as it were indirectly and almost with reluctance. When the cause of scurvy was at last scientifically ascertained in the classical experiments of Holst and Fröhlich two circumstances of the discovery attract our notice. It was made 173 years after Bachstrom

had categorically and correctly announced the cause of scurvy and 162 years after Lind had described its cure, and it was made in the course of a research primarily undertaken for another purpose. It is difficult not to believe that an active clinical science, by its situation so favourably placed for a direct attack, could have saved some of these long and costly years.

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SOME ASPECTS OF INFANT FEEDING*

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To ensure that a baby will make normal progress it is necessary that it shall retain sufficient suitable food. If we concentrate our attention on suitability and on adequacy and on some examples of under-feeding and of over-feeding we shall find material for profitable discussion.

Suitability

The percentage composition of the feeding mixture is used extensively as the guide to the suitability of an artificial feeding. It would be wise to abandon this basis in favour of the caloric basis, to which a great deal of attention has been directed since 1925, when Grover Powers¹ published his stimulating paper. The distribution of the total calories between the basal components may be calculated readily by splitting the caloric value per ounce into fractions derived from carbohydrate, protein, and fat respectively. Each of these fractions can be expressed as a percentage of the whole. The number of calories out of a hundred derived respectively from each component forms the caloric basis, by means of which different feeding formulae may be compared. On this basis terms such as "high-protein diet" acquire a new and a better meaning.

In the "majority group" of Powers, which included most of the successful feeding formulae, "the percentage of the total calories in protein varies between 10 and 20 per cent., and in fat between 15 and 30 per cent."² I am of opinion that the lower limit for protein and the upper limit for fat are both strained, and I am able to state that when the percentage of the total calories in protein lies between 13 and 18, and, in fat, between 15 and 40, the mixtures prove safe and suitable for the vast majority of babies in Melbourne (16 per cent. from protein and 32 per cent. from fat are safe average proportions). In other words, one aims at a protein ratio by calories between 1 : 4½ and 1 : 7.

Provided that the fat percentage of the milk used lies between 2.5 and 4.5, these conditions will be fulfilled if 70 per cent. of the total calories comes from cow's milk and 30 per cent. from added sugar (Table I). Enough water must be added to bring the fluid intake up to two and a half ounces per pound of body weight per day in cold weather, and to three ounces per day in hot weather.³

TABLE I.—Feeding with Milk 70 per cent. and Sugar 30 per cent. of Total Calories

Irrespective of the caloric intake or of added water, the distribution of each 100 calories will vary with the fat content as under. (Carbohydrate assumed to be constant at 5 per cent. and protein at 3.5 per cent.)

Percentage of Fat	Distribution of 100 Calories			Protein Ratio by Calories
	Carbohydrate	Protein	Fat	
2.5	54.70	17.20	28.10	4½
3.0	52.78	15.91	31.21	5½
3.5	51.31	14.82	33.87	5½
4.0	49.91	13.85	36.23	6½
4.5	48.70	13.02	38.28	6½
5.0	47.71	12.26	40.03	7½

The caloric value per ounce of the milk used will also vary with the fat content, and the number of ounces of milk to be used in a given case will be determined by dividing the number of calories to be supplied from milk by the caloric value per ounce of milk. In Table II will be found calculations of these variants.

TABLE II.—Feeding with Milk 70 per cent. and Sugar 30 per cent. of Total Calories

The effect of the varying fat percentage on the caloric value per ounce of milk, on the total carbohydrate percentage from milk and sugar, and on the caloric value per ounce of milk and sugar. Addition of water will make a proportional decrease in these figures.

Percentage of Fat	Caloric Value per ounce of Milk	Carbohydrate percentage of Milk and Sugar	Caloric Value per ounce of Milk and Sugar
2.5	16.57	11.12	23.71
3.0	17.91	11.62	25.63
3.5	19.24	12.11	27.54
4.0	20.57	12.60	29.45
4.5	21.91	13.09	31.35
5.0	23.24	13.60	33.28

Those who always like to know the percentage composition of the mixture used should note that there is a constant difference between the fat percentage (Col. 1) and the sugar percentage (Col. 3) of 8.6. Those who like to use a mixture of constant energy value per ounce can calculate the amount of added water required from Col. 4, the caloric value per ounce of milk and sugar. Those who prefer low-protein mixtures can dilute proportionally with water. If each of you will give this matter of the caloric basis of comparison your earnest thought in the light of its applicability to the system of feeding you practise, it is probable that we shall find in it a common ground for justifying the good results we each obtain, and also recognize that some of our pet principles are mere shibboleths.

We know that the biggest variable in cow's milk is the fat percentage, and for accurate feeding we should have a good idea of the fat content of the milk that is used. In every institution for babies it should be the custom to estimate the fat percentage daily. It may be of interest to refer to the method adopted at the Presbyterian Babies' Home, Melbourne. The milk arrives in five-gallon cans, and a sample is taken of the mixed milk for fat estimation; it is usually about 4 per cent., but may be 4.8 per cent. The bulk milk stands in a refrigerator, and after being undisturbed for three hours, if the estimation was 4 per cent., one pint is dipped off the top to reduce the fat percentage of the remainder to 3.5. A little more or a little less is removed according

* Read in opening a discussion in the Section of Diseases of Children at the Annual Meeting of the British Medical Association, Melbourne, 1935.