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THE INTEGRATION OF MEDICINE*

BY

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In the course of discussions upon the future and development of medicine two themes recur with melancholy iteration—namely, that specialism is an evil, and that it is inevitable. For this dilemma in which it seems generally agreed that we find ourselves no one proposes a remedy, or lights the path we must follow if we are to discover one. In his Harveian Oration of 1931, before the Royal College of Physicians, Sir Robert Hutchison spoke with insight into our situation when he said, "Specialism is inevitable; but though favourable to the accumulation of facts, it is bad for the philosophy of knowledge. There is too little speculation and too little use of the imagination; and most scientific literature is barren in ideas." Therein he put his finger upon a malady of modern science.

Yet whether the remedy the Harveian orator proposed is one we can adopt is open to question, for he went on to say that "it might be a good thing if there were a close time in laboratory work, say for five years, to enable us to digest the vast accumulation of knowledge we already possess and to think out new lines of advance." Yet I doubt if in the present climate of opinion, five years of research inactivity would increase interest in the use of the intellect, while, on the other hand, if we could stimulate this interest, a five-year truce with facts would be neither necessary nor desirable. The average modern scientific worker, paradoxical as the suggestion may seem, is more apt to be a man of action than of thought. He tends to distrust ideas. He has not always learned to achieve a harmony of observation and of general thought. His output of facts is necessarily reduced if he pauses to think about them. It may then easily be said of him that he is not making "discoveries" as fast as he should. In any case we cannot dam the stream of research effort, even if we do see too much of it losing itself in the sands.

It is significant that it is in medicine, and not in its so-called ancillary sciences where the situation is not essentially different, that an acute consciousness of the dilemma I am considering should first have dawned. Reasons for this are easily found. Let us take the case of physiology as example. This is now busily exploiting the harvest of technological advances in all fields of science, and is adapting to the solution of its own problems the methods which these sciences—physics, chemistry, and biochemistry—are so richly providing.

It has thus come about that a given physiological problem, such as the nature of cerebral cortical function, may now be approached from so many angles that it assumes the appearances of, and is too readily taken to be, many disparate and unrelated problems. All effective communication in thought tends to be broken between the users of the different exploratory techniques, and information accumulates while generalization wilts and dies. To use an expression familiar to readers

of war reports, the physiologists are "fanning out" into the unknown, and in this active stage of their advance fail adequately to realize that co-ordination of thought and aim must be retained if physiology is to be a coherent body of knowledge. Let me take again, as an example, the problem of localization of function in the cerebral cortex. Over fifty years ago Hughlings Jackson, the clinician who discovered the fact of localization of function within the cortex, being a man of ideas, also made some penetrating generalizations as to the nature of this localization. The era of experimental study of this problem opened very shortly afterwards, and has since been pursued with ingenuity, with eagerness, and by an increasing number and variety of techniques—the most modern ones being of great refinement and delicacy. Yet there has been no comparable development of thought upon the subject, and a crude notion that sees in the cerebral cortex a mosaic of sharply delimited localizations of fragments of function has survived virtually unaltered since the pioneer experimental investigators first bent their energies to the task of discovery. During all this time Jackson's interpretation and synthesis of the facts have scarcely influenced thought. It is not that this interpretation has failed to generalize the facts, it is simply that it has largely failed to excite the interest of research workers. The taint of ideas that clung to it, and the bend sinister of its clinical pedigree, have together provided the experimentalist with an excuse not to think about it.

Nor is this preoccupation with the collection of information the only factor in the disorderly state of knowledge on this and allied subjects. As Trotter has pointed out, and abundantly exemplified, in more than one of his penetrating addresses, physiologists commonly, and naturally enough, pursue their science for its own sake. Theirs is therefore by definition a liberal profession, while that of a physician is a useful one. I make this distinction in no invidious sense, but in the sense employed by Newman in his lectures on "The Scope and Nature of University Education," where he says, "That alone is liberal knowledge which stands on its own pretensions, which is independent of sequel, expects no complement, refuses to be informed (as it is called) by any end, or absorbed into any art in order duly to present itself to our contemplation." This quality, which physiology as a branch of liberal knowledge shares with such physical pursuits as cricket and fox-hunting—as Newman also points out—is not shared by medicine, whose activities have an end other than themselves: namely, the health and well-being of the community.

I do not wish to be taken as implying that the fruits of physiological research are not useful—indeed, we know that they are not rarely potent weapons in our hands—but as emphasizing that this last result is, when it happens, fortuitous, and is not one that the physiologist as such intends or is interested in. In short, in medicine we have to apply our knowledge, however garnered, to well-defined ends, to the prevention and cure of

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illness and, as a corollary to this, to the training of the doctors of the future. It is this that brings home to us, whether we will or not, the dangers inherent in the unceasing expansion and differentiation of the field of knowledge. As has been aptly said, once we seek to go beyond the basic elements of medicine as we know it, we tend to know more and more of less and less. Thus it happens that those responsible for the training of our successors too often find themselves imparting unrelated categories of information and partial and often conflicting generalizations culled from different fields of medicine, and it is becoming nobody's business, and seems less and less within anyone's capacity, to teach medicine as a whole, or to build into a coherent body of knowledge the several contributions of the specialists.

It is therefore because ours is a useful rather than a liberal profession that we have been forced to face the situation created by the accumulation around us of more, and more diverse, information than we can digest and assimilate. Hughlings Jackson was clearly aware of this, over 50 years ago, when he said that "we have multitudes of facts, but we require, as they accumulate, organizations of them into higher knowledge; we require generalizations and working hypotheses. The man who puts two old facts into new and more realistic order deserves praise as certainly as does the man who discovers a new one. There is an originality of method." But in this, as in much else, Jackson was before his own time and ours; and thus it is that physiology, true to its nature as a branch of liberal knowledge, sees no reason why it should not continue to browse at will upon the rich pastures of uncropped knowledge, giving no thought to any philosophy of knowledge, while medicine, faced by its ultimate purpose, has clamant responsibilities in the ordering of the knowledge at her disposal, and in the maintenance of a balance of activity and thought, that she dare not continue to evade. Medicine, then, has come to see that the unending additions to knowledge call urgently for a corresponding measure of integration. Yet we must try to strike a fair balance in this matter. Let it not be thought that I am making odious comparisons between physiologist and physician. We are both in the same boat, and in medicine we need not flatter ourselves that it is primarily out of any intellectual disquiet, or out of a divine discontent with the chaos of information that lies around us, that we have been brought to realize our dilemma—namely, the necessity and the evil of specialization. We are as innocent of any philosophy of knowledge as the physiologist. Our concern arises because much that is implied in the term "specialism" has come to be an obstacle to the teaching and practice of medicine.

Nevertheless, by whatever channel this awareness of the disorderly state of our science has reached us, we are at least generally agreed that we cannot indefinitely go on as we are, and that something must be done to bind the broken foundations of medicine and to make it something more than a congeries of ingenious techniques and unrelated fragments of knowledge. This, at any rate, however gained, is something gained.

Integration Keeps Pace with Differentiation

The thought that I am trying to develop will be familiar to many of you. It is summed up in the aphorism so familiar to neurophysiologists, "Integration keeps pace with differentiation." This is a fundamental principle in the evolution of the nervous system, and I cite it because I believe it to have a vital meaning for us in my present connexion. We owe its original formulation to the now disregarded genius of Herbert Spencer, from whom it was taken and so fruitfully employed by Hughlings Jackson. Derived anew from this latter source, it became a guiding inspiration in Sherrington's monumental contributions to experimental physiology, and its influence may be seen in the title he gave to his classic work of 1906, *The Integrative Action of the Nervous System*. Yet it is not so much with integrative action of the nervous system in respect of the organism as a whole that I am now concerned as with the development within that system, as it becomes progressively more differentiated, of structures and functions designed to control and to unify the several parts and to make them into a harmonious whole. In short, integration does, as an observed fact, keep pace with differentiation in the evolution of that system.

This conception is capable of a wider meaning than that given to it by the physiologist. I submit that it expresses something inherent in scientific thought and constantly found in truly scientific endeavour.

Let us now replace the term "differentiation" by "observation" and the term "integration" by "interpretation and synthesis." Observation leads to the increase and differentiation of information, while interpretation and synthesis are its integration into ordered knowledge, and I suggest that in the process of scientific thinking interpretation and synthesis must keep pace with observation if a coherent body of knowledge is to be forged. From this we pass easily and naturally to the notion that there is a rhythm in scientific thought, the two elements observation and interpretation alternating.

The feeling that there is such a natural rhythm in thought, a cycle in which observation and interpretation alternate, is widely implicit in our literature in respect of all branches of knowledge. It finds expression in such a statement as that of Lord Acton, "The main thing to learn is not the art of accumulating material, but the sublimer art of investigating it." While in Matthew Arnold's essay on *The Function of Criticism* we read, "The grand work of literary genius is a work of synthesis and exposition, not of analysis and discovery." Coming to our own field, we find Abraham Flexner writing: "Data of one kind or another are not so difficult to obtain; but generalization is another matter. . . . The two processes—the making of hypotheses and the gathering of data—must go on together, reacting upon each other." But the notion gets its fullest and most explicit formulation in a passage from the first volume of Arnold Toynbee's *Study of History*, which I have already quoted elsewhere, and do so again, to reinforce the cogency of my theme with the authority of this erudite thinker on history. He says:

"Scholarship makes its progress by a rhythmic alternation between the two activities—the collection of materials and their arrangement, the finding of facts and their interpretation—just as a physical organism lives and grows by an alternation between eating and digestion. The old fable of the belly and the members points the moral that neither activity is superior or inferior, prior or posterior, primary or parasitic, but that each is inseparable from the other as a part of the same whole, and complementary to the other as a phase in the same recurrent process. For the alternation perpetually recurs in virtue of the very nature of thought. When the mind is employed in finding facts, its sheer success inhibits it sooner or later from fact-finding uninterruptedly and *ad infinitum*. Sooner or later it finds itself so formidably beleaguered by the mass of facts which it has gathered round it that, until it has sorted them out and arranged them in some kind of order, it can no longer sally out into the Universe to gather more. Then the mind changes its activity perforce and employs itself for a season in making syntheses and interpretations. Yet now, once again, its sheer success inhibits it from working uninterruptedly and *ad infinitum* at bringing order out of chaos. Sooner or later, it finds that it has reduced to order all those materials which it had collected in its last fact-finding reconnaissance. Fresh facts must now be found before the process of synthesis and interpretation can be carried further. . . . No collection of facts is ever complete, and no synthesis or interpretation ever final. . . . This rhythm is native to thought in all its different channels. In the channel of Physical Science, we have seen that thought has recently passed out of a fact-finding phase into the next phase of synthesis and interpretation."

Thus Toynbee, and, *mutatis mutandis*, what he has to say of the study of history applies to medicine and the sciences ancillary thereto. In a remarkable foreword to his book on anaphylaxis and immunity, Maurice Arthus, with a true French fire and clarity, tells us how this balance is to be achieved, and how vital to science it is. This foreword has recently been translated into English under the title of *The Philosophy of Scientific Investigation* by Sigerist, and published by the Johns Hopkins Press. He says:

"The experimentalist must ponder and meditate deeply over the problems raised. . . . In order to make some progress in the experimental sciences one must meditate a great deal. I have repeatedly mentioned," he goes on, "the necessity of controlling the facts observed, of discussing the interpretations proposed and the meaning attributed to them, in order to accept as true and valid only what has stood the indispensable test of scientific criticism. This presupposes a special mental attitude which, unfortunately, is hardly developed in the schools, colleges, or perhaps even in the universities—namely, critical sense. This is the tendency of the mind to seek the true value of facts and results, of methods and of concepts."

It should not be necessary for me to labour this point any more, yet I believe that few would contend that during our time either medicine or its ancillary sciences have consistently honoured these principles of scientific thought, or have conformed to this rhythm of which Toynbee speaks as "native to thought in all its different channels." We have in fact, with some brilliant exceptions, been far too readily satisfied with fact-finding, and too little regardful of those intellectual instruments by which facts are interpreted and generalized. The work of Hughlings Jackson was such an exception, and another that stands out in my mind is that of Sherrington in the field of physiology. I can yet recall the feeling of exaltation with which, as a student of physiology over thirty years ago, I first read his *Integrative Action*. Here were new facts of observation in rich abundance, and how beautifully co-ordinated! With what logical precision the exposition of them advanced from step to step, and with what penetrating scientific imagination was their significance revealed! This perfect rhythm of observation, interpretation, and synthesis, which is the hall-mark of scientific genius, has characterized all Sherrington's work and has made of it a unified whole, a brilliant chapter in the story of the nervous system.

In the clinical field we see the same brilliant qualities informing the studies of Trotter and Davies on cutaneous sensibility. Here, within the brief scope of two papers, we find as complete a harmony of observation and interpretation and as finished a piece of medical research as modern medicine has to show. A comparable trend is seen in Head's work on sensation, a rhythm of observation and interpretation and a continuity of thought over a long-sustained research; and if the observations in some details have since proved fallacious and the interpretation sometimes unduly abstract, the whole is an impressive example of the normal rhythm of scientific thought with its own elements of grandeur.

To sum up this stage of my thesis, therefore, we see that as new facts increase in number and diversity, the need for their interpretation and synthesis increases in corresponding degree. By these means we forge those general principles that keep knowledge coherent, and make of it truly scientific knowledge.

One is sometimes asked what, precisely, one refers to by principles of medicine, and I admit that the term is apt to be used without due definition. Let me tax your patience by yet another example from the field of neurology. The generalizing mind of Hughlings Jackson, surveying the wide range of phenomena of disease of the nervous system, discerned that the totality of these phenomena could be subsumed under four headings, that the functions of the nervous system could react to damage in but four essentially different ways, whatever the aetiology of the pathological process concerned. He thus was able to formulate four corresponding generalizations. He then went further, and, taking the phenomena of nervous disease as a whole, he saw that these constituted an orderly reversal of the natural evolution of function in the nervous system, whether considered phylogenetically or ontogenetically. He was thereupon able to formulate a further and wider generalization—namely, a general principle of dissolution of function in the nervous system. Although seventy years have elapsed since this was expounded—years rich in new knowledge of relevance to the problem—it still provides a generalization of wide validity and great usefulness. Thus, however discouraging the therapist may find diseases of the nervous system, we have in neurology a coherent body of knowledge more closely based upon anatomy and physiology than any other branch of medicine, saving only cardiology, which has a body of general principles of a like order. Such a consummation should be our aim over the whole field of medicine.

No one will deny that there are dangers inherent in the use of generalizations—for example, the tendency to fit the new facts into the procrustean bed of theory, where knowledge may become rigid and die. Abstract thinking has indeed been described as "a major vice of the intellect," and no one has spoken more cogently of its dangers than Trotter. Nevertheless, he has also told us that ideas keep science fresh and living, and when properly used are in no danger of ceasing to become the nimble servants of truth. Therefore, even though the intellectual instruments of abstract thought and

generalization may turn in our hands, we must yet use them if we are to advance.

Now, those of you who may be ready to accept the point of view I am trying to advance may still ask me how we are to apply it. How are we to reduce the vast accumulation of facts to some sort of order, to elicit general principles, and to build a coherent science and art of medicine? From the very nature of things we shall never achieve finality, but at least we can try to tidy up as we go along.

I believe, in brief, that we must be imbued with a vivid realization of the fact that scientific thought and activity make up a rhythm of which observation on the one hand, and synthesis and interpretation on the other, are the elements, and that the mere eliciting and recording of factual details is by itself not science, and does not become so until the cycle is complete. In other words, we need a wholesome respect for ideas as an integral part of scientific thought. I cannot help thinking that if in every centre of original work, clinical and laboratory, workers were constantly aware of the principle that integration of knowledge should keep pace with its differentiation, we should find many fewer disjointed and apparently purposeless observations swelling the monstrous bulk of scientific literature. I sometimes doubt whether one is justified in recording in print a new observation unless one also seeks to indicate what it holds, and to apply the inductive process to it. One should not fling a raw fact on to paper in public, as a keeper flings a chunk of raw meat to a tiger. I believe that in medicine we have a unique advantage in this respect over the purely experimental scientist, in that medicine, while becoming increasingly an experimental, has long been and must continue largely to be an observational science. In its observational aspect it deals with a supremely difficult material under conditions that make constant demands upon intuition and judgment. Nature is not interested in scientific method, and the experiments she provides for us in the guise of disease and injury we have to take as we find them; we cannot subject them to the necessary but artificial simplification that is the essence of a good experiment. We are therefore forced to think, to synthesize, and to interpret our evidence to a point rarely necessary in the designed laboratory experiment. While, therefore, we must welcome the increasing role of experiment in the study of medicine, we must be on our guard not to be infected by the distrust of ideas characteristic of much experimental work, but continue to use the intellectual assets which experience of clinical observation gives every good doctor.

I confess that I have inflicted many quotations upon you, a fault which one filling the role of orator should never commit, but my object in doing so is to emphasize, as clearly as I am able, that my thesis is no new one, no private bee buzzing in my bonnet, but one that has often been maintained and exemplified by those whose labours in the realms of observation and ideas have shone most brightly before us.

Tasks Involved in Rebuilding of Medicine

If you have followed me so far, you will probably agree that there is one simple way in which each of us can contribute to the integration of medicine—by seeing that in any addition we may be able to make to the sum of medical knowledge we tidy up as we go along and conform to the natural rhythm of scientific thought, and that we try to build our fragmentary contribution into the general body of relevant knowledge.

A second task involved in the rebuilding of medicine is to ensure, as and when the human resources are available, that those who hold key positions in centres of medical education and research shall be men interested not only in research and routine teaching but also in general ideas.

This may prove no easy task, for such men are rare, and the trend of values hitherto has not been such as to encourage them to follow their bent. You cannot find them by offering professorships, as some suppose, but you can always offer them full opportunities when you find the men. Encouragement must begin when they are young, and before they become moulded or resigned to the ephemeral patterns of current orthodoxy. The young worker must be allowed to retain his

intellectual independence and to pursue the problems he has found for himself—as the best men do find them for themselves; he must also be allowed some vestige of freedom by the editors of those scientific journals in which his work reaches the world, in developing his ideas, so that his papers are not reduced by the editorial blue pencil to those drab sequences of experimental protocols that for too many appear the ideal of a scientific paper. An editor should be “no envious Juno sitting cross-legged over the nativity of any man’s intellectual offspring,” as Milton says; and whoever believes that the censorship of ideas survives to-day only in the totalitarian State imagines a vain thing. The original sanctions may have gone and a censorship’s formal existence have ceased, but the itch to suffocate the infant idea burns in us all in greater or less measure.

I may seem to have wandered far from my opening theme—namely, the evil and the necessity of specialism in medicine—but I believe that in fact I have not done so. The danger of specialism is not so much that a man devotes his major activity to a restricted field of knowledge in medicine, but that he is so apt to become absorbed in its details, and still more in those of the complex instruments that are now so freely at his disposal, that he unwittingly cuts adrift from the general body of medicine and goes off on a voyage of his own.

There are in truth no irrelevances and no contradictions in nature, nor can there be in any coherent body of knowledge. The specialist, therefore—and I use the term in its strict and not in its journalistic sense—must cling fast to the foundations of medicine, for only thus can he integrate his contributions to knowledge and orient them, and avoid the multiplication of those conflicting *ad hoc* hypotheses with which medical and allied literature are littered.

So, too, the teacher of medicine has a like obligation to interpret so far as possible and to keep a lively interest in general ideas. This applies with greater force to the academic than to the purely clinical teacher, who has a major responsibility in handing on the art and the techniques of medicine.

Last, there are our textbooks. It has now become the almost universal practice for a textbook of medicine to be the algebraical sum, if not often the integration, of the knowledge of a body of specialists. There is a danger inherent in this plan, forced upon us though it may be, for it demands in the editor of such a compilation gifts of detailed knowledge and of generalization that few can hope to possess. In these circumstances, also, an editor has to have a clear idea as to the circle of readers he is providing for, as to the particular want he desires to satisfy. What is best for the student is not necessarily adequate for the follower of higher studies, and what the latter requires is not the diet for the student. Too often we see an attempt made to make the best of both worlds, with the result that neither is wisely catered for. Of all textbooks it may not unfairly be said in our day that they are more interested in facts than in ideas, and an urgent task before us is some recasting of them. It is significant of the trend of current thought that whereas formerly textbooks of medicine claimed to expound the “principles and practice” of medicine, no modern work of the kind makes even a titular claim to concern itself with principles.

Finally, there is a pseudo-integration of medicine against which we are not always sufficiently on our guard. This is exemplified in the numerous doctrines of those who see their own speciality everywhere in medicine, who have their own touchstone by which all the phenomena of illness are to be assessed. They measure every man’s corn by their own bushel, and are obsessed by this or that cult of ideas and are always proselytizing. They make up those cohorts of hobby-horse riders who cavort noisily to and fro across the field of medicine, throwing up little but dust into our eyes.

The Role of General Education

Now if this notion of what is requisite to advance in medicine has validity it must presuppose an adequate general education as a foundation. If those who come after us are to be equipped to observe and describe accurately and interpret logically and imaginatively, their training to these ends must begin while they are still young, and their school education should not be based upon short-sighted utilitarian considerations, or

be of what I may call a polytechnic order, but should include as its very foundation a thorough training in the use of that noble instrument for the expression of thought and feeling, our mother tongue, in the expansion of that instrument by the right use of analogy, and in its logical employment.

In brief, what I am invoking are the three arts of grammar, rhetoric, and logic. If we lack a command of language we cannot hope to produce, formulate, or entertain general ideas or make even the simplest essays in abstract thinking.

Now, those who are familiar with the aims and methods of the mediaeval university will recall that these three arts, composing what was called the trivium, formed the basis of university education. It may perhaps be argued that in the Middle Ages, the ages of scholastic philosophy and of theological disputation, these three arts tended to be followed as an end in themselves, that the training in clear exposition and thought was what I have already called a liberal education with no end other than itself, and that it led to mere logic-chopping and to no advance in natural knowledge. Some of this may be true; I express no view upon it, and I am not proposing the revival of these arts merely as an entertaining and stimulating variety of mental gymnastics. I put them before you as an essential preliminary to that clarity of thought and expression that is one of the weapons of scientific advance, of the tools with which we pursue natural knowledge. From which it follows that the greater our command of language, the more clearly and deeply we can think and express our ideas. I submit, therefore, that a command of language, and its logical use, are vital preliminaries to any scientific training.

Some may ask me where rhetoric comes in. In its original and its mediaeval sense it refers to the expansion and condensation of discourse by analogy and figure of speech. Wherever we find lucid and vital English, there we find rhetoric. What a text of lovely and illuminating rhetoric are the psalms, for example. Rhetoric has its place in the expression of those ideas we derive from the study of facts, nor can we teach without it.

In short, what I am proposing is that a humane education is an invaluable asset to any youth embarking upon the study of medicine. I am aware that I raise the banner of a forsaken cause when I say this; but nevertheless, twenty-five years of clinical teaching have fully persuaded me that when I find a clinical clerk who can stand up and read at a ward visit a case history that is a well-ordered, lucid, and fluently expressed account of the patient and his situation, that student will almost invariably be found to have had a sound education, and not a mere course of instruction of the polytechnic order, a utility education. This is why we must all deplore the practice of turning boys over to premedical subjects exclusively, or almost exclusively, directly they jump the hurdle of the school certificate. Too often they come to us semi-articulate, their budding ideas imprisoned behind the bars of an inadequate command of expression. It is between the ages of 14 and 18 that the capacity for pursuing a line of thought and for abstract thinking develops, and it is then that the adolescent should find himself provided with the necessary instruments of speech. “Give me,” says Milton, “the liberty to know, to utter, and to argue freely.” If we changed this by saying “Give me the literacy to know, to utter, and to argue freely” we should not really be altering the sense, for it is the capacity to express it that gives liberty to thought.

Conclusion

What I have had to say is somewhat remote from the medicine that presents itself to us in our daily lives. Yet as we grow older the urge to lift our eyes from the details of the little plot in which each of us labours becomes ever stronger, and we are impelled to look round upon the wider field of natural knowledge as a whole. Some may even feel the pull towards some philosophy of knowledge, towards those common truths to which all science must be obedient. Man does not live by facts alone, but craves also for generalizations, and the desire for some philosophy of knowledge burns, if with varying intensity, in all of us.

Looked at in this spirit, how untidy and in places how overgrown the field of medicine seems to be; in other places how bare, how precariously balanced the whole upon the uncertain

foundations of biological knowledge! So it must remain until we develop a wider and deeper consciousness of what constitutes ordered knowledge, and by what cycle of thought it is to be achieved.

Yet to feel some discontent with medicine as we find it does not imply any lack of pride in its achievements, nor any diminished sense of privilege in seeking to serve it. If one is a critic, it is, I trust, in the spirit expressed by Milton when he says, "For he who freely magnifies what hath been nobly done, and fears not to declare as freely what might be done better, gives ye the best covenant of his fidelity."

ON THE DIFFERENCE BETWEEN THE SEXES IN DISPERSION OF INTELLIGENCE

BY

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It is a striking fact, though it is in no way surprising, that in mean performance on intelligence-test scales there should be no difference between boys and girls. On the other hand, and especially in view of this first result, it is one of the oddest facts in the whole field of human biology that boys should be more variable than girls. Furthermore, as will be shown, the difference is not merely a small one, considerable though the academic interest of this would be; it is a large difference with important practical implications. Gifted boys are more numerous than gifted girls, the discrepancy increasing as we pass further away from the average performance of the whole population; till, actually, the performance of the cleverest boy in each 1,000 boys is equalled by only one girl in each 2,000 girls; thus at this level highly intelligent boys are twice as numerous as girls. All this is, however, exactly counter-balanced at the other end of the scale, dull boys being more numerous than dull girls and very dull boys correspondingly much more numerous. In other words, girls tend to be grouped closer to the average for the whole population, while boys tend to spread further over into extremes of brightness or dullness. In the planning, therefore, of such things as the provision of facilities for higher education, a greater percentage allotment should be made for boys than for girls. Similarly, greater provision for boys is needed in special classes and schools for the backward and, to the extent that mental deficiency depends upon intellectual retardation, much more in institutions for mental defectives.

Existence of the Difference

The sex difference in dispersion of intelligence is an empirical observation: so far as I know, nothing analogous has been demonstrated, and it is difficult to think of any convincing biological explanation. In these circumstances it has been naturally and rightly felt that strong evidence should be forthcoming before so curious a finding should be accepted.

From the early days of accurate mental measurement indications of the difference began to appear, though by no means invariably in all studies. It is easy to see why results should have been conflicting. In the first place, relatively large numbers are required in order to detect a difference in variability—considerably larger than those required in order to detect a difference in means; from this point of view most samples were very small. In the second place—and this is much more important—it is very difficult to secure a random sample of intelligence-test results. Bias in the sampling of school-children tends especially to affect the extremes; hence the sex difference in dispersion is particularly liable to be artificially minimized. For example, my colleagues and I (Roberts, Norman, and Griffiths, 1935) have shown that the omission of private-school children from a complete group considerably diminished the difference. For these reasons, Winsor, writing as late as 1927, could still conclude, after a careful review of the literature, that the evidence was insufficient.

Not until the appearance of that magnificent piece of work, the survey carried out by the Scottish Council for Research in Education (1933), was all legitimate doubt at an end. In this survey both the difficulties mentioned above were admirably countered. The numbers tested were vast, being no fewer than 87,000; and every effort was made to secure completeness of sampling, the aim being to test a complete annual age group throughout the whole country. It is true that some unavoidable bias remained, but in excellence of sampling no less than in numbers this group was a great improvement on its predecessors.

A good verbal group-test was applied, the results being as follows:*

Difference in Mean Performance between Boys and Girls

	No.	Mean Score
Boys	44,210	34.506
Girls	43,288	34.411
Difference = 0.095 ± 0.105		

The difference is less than its standard error, so that, even using such large numbers as these, it is impossible to decide whether boys or girls have the higher average performance.

The variances tell a very different story:

Difference in Variance between Boys and Girls

	No.	Variance
Boys	44,210	253.747
Girls	43,288	225.605
Difference = 28.142 ± 1.885		

This very large difference is no less than 14.9 times its standard error.

The existence of the difference is abundantly demonstrated, but two important points must be considered: it is demonstrated using this particular test and for this particular sample; is there any reason to anticipate that using another scale or another sample the same result might not be obtained?

First, as regards the test, good scales designed to measure general intelligence correlate so highly with each other that it is, to say the least, extremely unlikely that the sex difference would not appear whichever was used. Direct proof, however, is not lacking. The Scottish investigators retested a sub-sample of 1,000, using the Stanford-Binet scale. A certain lack of randomness in the sampling complicates interpretation, but it was shown that the difference is clearly apparent on the Binet scale also.

My colleagues and I used a different verbal group-test, the advanced Otis, in our survey of school-children at Bath, as described in a series of papers (Roberts *et al.*, 1935, 1937, 1938a, 1938b; Roberts, 1940). Our results are as follows:

Difference in Mean Performance between Boys and Girls

	No.	Mean
Boys	1,742	100.743
Girls	1,606	99.790
Difference = 0.953 ± 1.187		

Difference in Variance between Boys and Girls

	No.	Variance
Boys	1,742	1296.38
Girls	1,606	1066.26
Difference = 230.12 ± 53.75		

Just as in the Scottish survey, there is no significant difference in mean performance but a very large difference in variability, this being 4.3 times its standard error.

As regards the sampling, it has already been mentioned that the Scottish group was carefully selected; such bias as there was could not affect the magnitude of the difference materially, and in any event would tend to diminish and not to increase it. The Bath group is, of course, much smaller, but even greater care was taken to ensure completeness. The aim was to ascertain and test every child whose time of birth fell between certain dates, and whose home was within the boundaries of the city on a given date. As described in the papers quoted, this aim was very nearly attained.

It can be safely concluded, therefore, that any good test of general intelligence applied to any good sample will show the

* All calculations in this paper on the Scottish survey results are based on the tables given on pages 61 and 62.