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STRATEGY AND TACTICS IN RESEARCH*

BY

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Of the various fields of medical activity now coming under review in the discussions being held on the future of medicine in this country, that of research must command its full share of attention. If the greatest possible advancement of knowledge is to be secured, those in whose hands the reorganization of medicine will lie need a sound conception of all that is embodied in the idea and in the execution of research. We may not assume that everyone eager to provide us with a bright new medical world has the experience or insight necessary to deal with the problems of medical research, nor even that all of those actually engaged in it are endowed with the clearest vision on the subject. Some familiarity with the literature in which original investigations in my own field of medicine are recorded, various discussions that I have listened to, and schemes of research upon particular problems that I have known propounded have led me to believe that not a few of those who engage in research work, or who find themselves—often for quite irrelevant reasons—in a position to initiate and control it, have extremely limited views as to what constitutes good research, how to plan and set about it, or how to interpret and use to the best advantage the results it yields. The present time therefore seems apt for an attempt to formulate some general principles of a strategy and tactics of research. I shall concern myself principally with medical research, but my remarks may possibly have a wider application if they are well founded.

Research in the Past

Until comparatively recent times medical research has been pursued largely in the voluntary hospitals and their medical schools. Most of those from whose ranks the original workers were mainly recruited—namely, the honorary medical staffs—have from the nature of the voluntary hospital system been compelled to gain their livelihood by the private practice of medicine. As a result clinical research has been hampered in its full development, for those engaged in it have found themselves increasingly drawn away by the demands of practice, until in middle life most of them are compelled to relinquish all hope of serious original work. Even under conditions so adverse, their achievements in the past have been considerable, and have provided the foundations upon which a more fortunately placed younger generation may now build. Perhaps not all of this generation realize fully that some at least of the broad perspectives now opening before them they enjoy because they are, as it were, standing on the shoulders of their unsubsidized predecessors. Some of them, it may fairly be urged, are inclined to scorn the base degrees by which they did ascend, and therefore a tribute to the labours of past

generations in advancing medical knowledge is not wholly superfluous.

Yet under these past-time conditions advance was uncertain and somewhat fortuitous, and none will deny that the increasing pace of scientific work requires that greater opportunities for clinical research should be provided. The growing complexity of the problems before us, and of the research methods essential to tackling them, makes greater demands upon the individual than in the old days when the hewing out of the rough features of medical knowledge by relatively simple methods was possible for men who, in more leisurely times, were also engaged in the practice of medicine and surgery. Research is becoming increasingly a matter of ever finer analysis of fundamental bodily processes by technical methods of great complexity, and is too exacting a mistress to be satisfied with less than the first claim on the worker's endeavour.

Despite the steady increase of opportunities for the free pursuit of clinical science that the past 20 years has provided, further opportunities must be forthcoming if British medicine is to regain its position in the van of progress. It is not my prime purpose to discuss how far simple organization can achieve this result. Of organizers we need fear no dearth, nor of those ready to place all their hopes in planning and to cherish the most sanguine expectations as to what can be achieved by committees. My first concern is to consider the part that must be played by those who do the research work if this is to yield a good harvest.

One of the outstanding features of medical research of the past is that for the most part only the man with the urge to explore the unknown engaged in it. He could hope for no material reward from doing so. Indeed, he might easily suffer from his preoccupation with original investigation. It offered him no prospect of a career; what we may call the professional researcher in established laboratories was apt to look askance at him as an amateur, nor could he expect to join the select ranks of press-famous doctors by devoting himself to it. In fact, he too often ended as one of those pathetic figures of whom we used to see it stated in obituary notices in our medical press that "he did not attain that success in practice that his merits entitled him to, but his colleagues appreciated his worth."

Only the rare man who combined insatiable scientific curiosity with abounding vitality and enthusiasm could succeed in both research and practice. Victor Horsley was such a man, but we cannot tell when we may look upon his like again. Hughlings Jackson, to take an outstanding example of the original thinker in medicine, thought long and profoundly on the problems of neurology and psychology and greatly enriched knowledge because it was in him to do so and would not be denied. It is as difficult to believe that any formal organization of research in his day would have made him more productive as it is to suppose that English poetry would be any more rich and lovely than it is had all poets in the past been given salaried posts and relieved of financial difficulties: a thought that we

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may well ponder when thinking of the possibilities of mere planning for achievement in the realms of the mind.

The subsidizing of medical research has grown and will continue to grow, and thus many now engage in it, and will in future do so, who in days gone by would have been neither willing nor able. Their compelling motives are many and various. As always, a few have the true vocation and eagerly embrace the enhanced opportunities now opening before them. Many enter research for more pedestrian reasons: some because it offers a safe refuge from the exhausting labours, responsibilities, competition, and financial uncertainties of private practice; others because it affords them the illusion of an intellectually higher life; a few, perhaps, because they are misanthropes who, in the laboratory, can hope to escape contact with their kind; some just choose it upon a dispassionate calculation between the various openings a medical training offers them; and some, alas, merely as a stepping-stone to more attractive adventures in other fields of medical activity.

The True Discoverer

Whatever their reasons, a growing army of men and women are enlisting in the ranks of original investigators. Of all these, however, we can look for very few in the first ranks of true discoverers: those who infuse their technical competence with scientific imagination, who see their problem in essence before they proceed to tackle it; who know how to ask the experimental questions best calculated to elicit from Nature the relevant answers; who not only add new facts to store but see their significance and can embody them in the ordered knowledge that is science. Not content merely to dig for gold, they also refine it, knowing that it is useless until they have done this, and that isolated pieces of information are merely the raw material of knowledge. A capacity for the unrelenting contemplation of the facts of observation is their surest and peculiar weapon and their guide in experimental work. They are the strategists of research.

No planning can provide us with these men or determine the goal of their labours. When they possess the added gift of being able to attract younger men around them their powers of discovery are immeasurably enhanced; for they can train their successors, and can direct the labours of workers of lesser rank, the tacticians of research who when a problem is formulated can devise a means of tackling it. Under them also work the third rank of research workers, to whom can be confided limited and precisely defined tasks in the larger plan of campaign.

Here we have a sort of spontaneously arising organization that has grown round a personality and has owed little or nothing to deliberate planning. When this comes into existence it is the richest of all sources of original knowledge. Within our own time and in this country, physiology, biochemistry, and clinical science have given us brilliant examples of such schools under the leadership of Sherrington, Gowland Hopkins, and Thomas Lewis. Woollard, had he lived long enough, might have established such a school in anatomy. Given such leaders, research of the first order will flourish, however scanty the material resources available, however negligible the organization from without. Going further afield, we have a striking illustration of this in the school of Magnus of Utrecht, a true offshoot of the Sherrington school of physiology. Over a period of years a sequence of closely related investigations upon the physiology of body posture came from this active centre of research, and was recorded in a series of papers that were a model of what such records should be: concise, crystal-clear in language, and with step following logical step in the presentation of facts and argument. All this work was carried out in a cramped and ancient building, once a plague hospital, as ill adapted for modern research as may be imagined and as poorly endowed. Yet the personality of the leader inspired a band of paid and voluntary workers—de Kleyn, a practising surgeon, being one of the latter—to complete one of the most fascinating chapters in the records of neurophysiology. In short, it is round the man and not round the plan that the wheels of research revolve most rapidly.

All this is not to deprecate some attempt to organize our resources, but rather to indicate the limits of what planning and research foundations can achieve. Geniuses for research are not created by committees; they are not even invariably

recognized by them in time to afford them support. By the time Magnus at Utrecht could look upon a new and fully equipped laboratory his last experiment had been made.

Lines of Approach in Strategy and Tactics

These considerations bring me back to my starting-point—namely, the great importance of sound ideas as to a strategy and tactics of research if the greatest efficiency is to be attained. Perhaps some apology is called for from me at this juncture. It may be thought that only one whose active life has been spent in research can be competent to discuss this question. Up to a point this is probably true, yet, after all, the onlooker sees a great deal of the game, and it is as an interested spectator of the fields of neurophysiological and clinical neurological research during the past quarter of a century, and for part of that time associated with the editorship of *Brain*, that I venture to give my impressions of the quality of strategy and tactics to be seen in these fields of medical and allied research.

Claiming no more than this, then, I submit that a sound research strategy connotes a familiarity with what has already been accomplished in the field of investigation to be explored; but it calls for more than this—namely, for a clear grasp of the implications of earlier work and some discernment as to where it is probably leading. Some pattern has to be sought in the numerous and often seemingly unconnected items of information available, and the gaps in this have to be recognized as the field of knowledge is critically surveyed, since from this recognition it may be hoped to get a lead as to what questions remain to be answered before the complete pattern of the bodily function or process under inquiry can be revealed. The strategist's scientific imagination will show him what lines of approach offer the best hopes of success, and when to abandon any given line and start afresh. He may have, and must be able, to recast his ideas completely regarding the nature of his problem should unlooked-for results make this necessary. This in turn requires that he shall have the intellectual integrity and clarity to sacrifice his provisional hypotheses to the facts if these demand it. Experience shows that this dilemma is not always successfully solved by the research worker; for his hypothesis may have become dearer to him than the facts, which he then proceeds to tidy up in conformity with his preconceived notions, turning a blind eye to one and "modifying" another. By tactics of research I have in mind the drawing up of a precise experimental plan, once the general intention of the research has been thought out and formulated. It implies the devising of experiments—that is, of questions designed to extract the answers from Nature. In doing this the worker shows his capacity for cross-examining Nature and his purpose in doing so. His experiments, therefore, are not well-intentioned gropings in the dark but skilfully directed probings. Some there are who, while not being strategists of the first order, are adept in this tactical planning once the purpose of an investigation has been defined for them. Now, in the world of scientific activity as we find it there are not enough strategists of the first rank to go round, and thus it comes that many investigations are carried through with no longer view than the acquisition of some piece of information as an end in itself without any clear thought as to its possible implications. Once gained, this piece of information is put on record, but its orientation in the larger field of related knowledge and its building up into a general body of thought seem beyond the vision of the workers. Indeed, we sometimes ask ourselves what was in their minds in undertaking it. They have, as it were, rooted up industriously a sort of scientific truffle and have then promptly lost interest in it. The journals are full of these bits of isolated work, of which it may be said that they are no more than the raw material of knowledge. Not a few workers pass their active lives going from investigation to unrelated investigation on no discernible plan, never pausing to contemplate the full implications of their work, but, with an unrelenting haste and an apparent horror of contemplation, proceed on their mission as collectors of information. Sometimes one cannot but suspect that they cherish the illusion that he who publishes four papers is a better man than he who produces but one, and there is not a little in the current standards of values concerning these things that encourages them in this dreadful notion. Concerning these standards and

the question of the productivity of the research worker I shall have something more to say later.

Now, the research worker of the first rank does not leave his facts of observation lying on the grass in this unfinished state, nor does he collect facts just for the sake of doing so. He extracts the last grain of significance from his observations before he leaves them to pass on to a logical next step in his planned campaign of investigation. It is true, of course, that the raw material of less polished labours may from time to time provide him with essential links in some chain of evidence or, illuminated by his thought, may come to have crucial importance. Yet this is no justification for research work as lacking in purpose as in method. At the other extreme we find scanty or ill-substantiated observations made the foundation of some elaborate structure of hypothesis which they cannot support. To this variety, also, the literature of research is no stranger.

Shortcomings in Research

But perhaps the most pitiful travesty of original investigation is that undertaken for some irrelevant reason, such, for example, as that research work gives a committee, an institution, or a hospital a good name, and creates an atmosphere of scientific glamour highly gratifying to those promoting it or engaged in it. An apt variety of this aimless activity was the scheme I once heard proposed to take polygraph tracings from all the patients admitted to a certain institution over a twelve-months period, on the naïve expectation that "something's bound to emerge!" No one paused to ask what might be expected to emerge, or how, once emerged, it was to be recognized as a significant phenomenon.

Not less unpromising were the labours of a committee within whose terms of reference it was to initiate and support schemes of research, when it met to think out from the inner consciousness of its component members things to research about, much as one might ask at a children's party, "What shall we do next?" Closely related to this conception of research is the notion that all that is needed is an elaborate questionnaire for a collection of assorted workers of unknown capacity to conform to and to fill in, some statistical amateur then to distil knowledge from the completed returns. From this Gallup poll to research by automatic calculating machines seems but a brief step, for the use of the intellect has been virtually done away with.

But lack of imagination is not the only shortcoming of much modern medical—and other—research. A not less pervasive evil is the tendency to accept statements of fact, and even hypotheses, on trust, more especially when they emanate from writers to whose name prestige attaches. Prestige, indeed, may be a deadly peril in scientific thought. Every statement and every hypothesis on record must stand or fall on its merits, irrespective of its author. Nothing is more common than to read in the opening paragraphs of an original paper the phrase, "So-and-so has shown that," when more often than not "So-and-so" has shown nothing of the kind—he has only said or suggested it but has not demonstrated it. Once thus referred to, a false air of reality attaches to the statement and it passes insensibly into the category of things we accept as proved, where as likely as not it does not belong. In this way a hypothesis with no demonstrable validity may obtain a measure of acceptance and a lease of life to which its merits by no means entitle it—and it may even hold up fresh thought for a generation by its malign influence. Yet really critical examination would have revealed from the outset that it was fallacious or unsubstantiated. That this should be so is a reproach, and suggests that what I have spoken of as the unrelenting contemplation of the facts of observation does not engage original workers to the degree it should.

Necessity for Critical Analysis

This brings me to the role of critical thought in the field of research. This, and this only, can keep the world of original thought alive and fresh, can sort the wheat of sound hypothesis from the tares of rash and ill-thought-out speculation, the ephemeral from the lasting. When one considers the flood of papers dealing with new observations—or rediscoveries of old observations—that surges about us in a rising tide each year, it is clear that the imperative necessity for quite ruthless critical analysis becomes even greater. Yet who wants to

shoulder this role of intellectual inquisitor? No one, indeed, with any regard for his own advancement or peace of mind. It has become the craven fashion to decry critical analysis by attaching to it the unpopular name of "destructive criticism." Yet we must clear the ground of the lumber of unreliable observations and unsupported hypotheses before advance is possible. Those of you who have read—as we all should read—Trotter's *Collected Addresses* will find in them the following passage, which cannot be too constantly borne in mind. He says: "The common tendency to regard destructive criticism as always easy and generally reprehensible is one I do not share; I doubt if it could be acquiesced in by any sensible person making a frank survey of the intellectual world to-day. At no time in the history of the intellect has the sanitary work of destructive criticism been more needful." If for the words "intellectual" and "intellect" we substitute "medical" and "medicine," these words lose none of their force. Yet if there is nothing the research worker needs more as a stimulus and a guide, there are few things he dislikes more than critical thought applied to his labours. He is always able to persuade himself that any criticism is inept, unnecessary, or impolitic. This, I suppose, will always be so—for the investigator is as full of human frailties as the rest of us; yet this emotional reaction makes all but the most hardy shrink from those periodical and candid surveys of the field of knowledge the lack of which may allow the most fallacious of generalizations to dominate thought for a generation.

Yet it is of the very essence of scientific advance that we should have criticism and synthesis as well as observation. The situation is well summed up by Arnold Toynbee, in respect of history, in the introductory chapter of his *Study of History*. He says: "The three conditions of criticism, transitoriness, and succession under which the scholar has to do his work . . . are conditions that arise from the nature of scholarship itself, which makes its progress by a rhythmic alteration between 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 alternating between eating and digestion. The old fable of the belly and its 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."

The season of synthesis and interpretation is long overdue in medical science. We have more facts than we can digest—facts enough, indeed, to make salutary advances in ordered knowledge were we given the time and encouragement to make them. Yet the present climate of opinion is adverse to contemplation and the exercise of critical thought, and cries loudly and unthinkingly for ever more facts: the normal rhythm of thought is suspended and we sit bewildered amid the chaos of facts we are not encouraged to put in order.

There is another point about medical research that seems to require a brief reference, and that is the notion, most tenaciously held by those who know least about clinical work, that good research necessarily means work in the laboratory and experiments on animals. This is a crude fallacy; the medical research worker should be the master of all methods necessary to his problems, and the slave of none. He must be ready to extract from each method what it offers, and to choose the combination of methods essential to the successful prosecution of his task. Knowledge will not be advanced by creating a false antithesis between observation at the bedside and the study of the human subject by whatever means are legitimate and useful, on the one hand, and work in the laboratory and on animals on the other. The observation of human disease has by no means exhausted its resources, and even simple clinical observation may yet provide valuable and

essential lines of attack on problems of disease, that cannot perhaps be solved without the additional aid of the laboratory and animal experiment. I cannot believe that any medical research worker can afford to divorce himself from clinical studies. When he does so he invariably loses touch with reality.

External Dangers

If the shortcomings of much medical research that I have indicated really obtain, they are such as can be remedied only by those engaged in that research. Yet there are external dangers that increasingly surround research as it becomes progressively more organized from without by controlling bodies whose paid workers the researchers are. I have ventured to suggest that many more such workers are ready to make observations or experiments than appreciate the necessity for continued thought about their work and its implications. This tendency is not checked when research organizations expect their workers to manifest productivity in terms of published papers. The man who has every year to draw up his annual report showing what he has accomplished can scarcely be blamed if he does not spend much time in contemplation. He will not care to report that he has as yet published nothing, is not ready to publish anything, and has not got very much further than surveying his problem.

Some sacrifices of independence and freedom have to be made when research is widely organized and research workers are paid salaries. Even research foundations have to satisfy their "public" that they are getting results; they have to do some window-dressing. Only an informed and enlightened control can secure for the research worker that freedom from which alone his best work can flow. He must have time to think, and be encouraged to do so, and he must not be haunted by the fear that his productivity is being measured in terms of papers or that his share of the organization's financial resources is grudged him. Under the experienced and sympathetic administration of the more important research foundations he is least likely to suffer in this way, but if and when the State or public bodies—through hospital and other services—take a hand in the matter, and if considerations of political expediency begin to appear, the salaried research worker may end by becoming a kind of scientific helot from whom quick results, in directions not always determined by him, are looked for. Now is the time to safeguard him from this melancholy fate.

One last danger should, it seems to me, be foreseen in the organization of medical research in the new order—namely, the danger that the worker in pure research may be segregated in central institutes remote from hospitals and medical schools. I believe this would be fatal to true progress in medicine. It would take the spirit and the inspiration of inquiry from the schools, wherein alone are to be found the recruits to the ranks of original investigators. It would deprive the student of contact with the research worker, and would deaden the quality of the endeavour of everyone concerned in hospital work and medical teaching. The research worker himself would suffer no less from this isolation from the world of medical endeavour, from the true battle-front of medicine. Thus isolated he would soon lose his sense of reality and of proportion. He would cease to have the necessary vivid awareness of the nature and difficulties of medical problems. It has commonly been so in the past.

Conclusion

It is idle to look back, but how enviable in retrospect was the lot of the physician or surgeon of a past generation who engaged in research work to satisfy his own scientific curiosity! He could think at his leisure, could publish his results when these were ripe and at his chosen moment: he was not harassed by the thought that something was expected from him at stated intervals in the calendar, or by the fear that some organization would begin to think that he was not worth his keep as a research worker. It may be true that we can no longer leave the matter of medical research to his amateur care; but will the subsidized research worker, caught up in the machine of organized research, ever savour the true taste of discovery so fully? It is hard to believe that the human contacts the older workers made in the practice of medicine were not at least one source of their inspiration, or that the time they gave to the treatment of the sick was really the waste some modern advocates of reform would have us think.

Therefore, while fully recognizing that the times before us demand a more organized research effort than we have had hitherto, we may yet regret the probability that something of what was most favourable to the best work may be lost in the process, and we may hope that those who are reorganizing the fabric of British medicine will bear all this in mind and will make the conditions under which the salaried research worker has to labour and live most favourable to the free and unhampered development of his powers.

THE PATHOLOGY OF CLOSED INJURIES OF THE CHEST*

BY

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After two years of continuous war work I shall here attempt to review in broad detail some experiences gained in the routine post-mortem examination of battle casualties, selecting for special analysis thoracic injuries of the closed type. My material has been drawn from both Service and civilian casualties, and it can be claimed to be wide and varied.

By a closed injury to the chest I mean either that the skin and subcutaneous tissue were intact or that, if these were pierced, the course of the missile had been such as to leave the pleura intact and so the pleural space had no contact with the outside air. This study excludes all cases in which a foreign body had entered the pleura or thoracic viscera, and deals with a group of cases that, to my mind, are the most interesting and in some ways the most important of war injuries. The cases may be divided into two main categories—those due to direct violence to the chest and those in which the injury to the thoracic viscera is indirect—i.e., a result of injury elsewhere in the body. Fat embolism is an example of this latter group: here the lung changes are very definite, but are usually the result of injury elsewhere, such as fracture of long bones. The former group includes: (a) lacerations, (b) contusions, (c) blast injuries (pulmonary concussion); it is principally with these cases that I shall deal.

Lacerations

Laceration of the heart or great vessels is of little pathological interest. It rarely if ever occurs in closed chest injuries, and when it results in rupture is rapidly fatal. A case of ruptured aorta due to blast has recently been reported by Wilson and Tunbridge, but I have not seen any further case of laceration of heart or large vessels in my series of war injuries. Laceration of the lung, on the other hand, is of great importance, and has occurred in this series with considerable frequency.

Case 1.—A gunner aged 28 was admitted suffering from shock and complaining of pain in the right chest. A bomb had exploded near his gun position and he had been thrown up against part of the gun. It was not clear whether he had been actually struck by a bomb splinter: the only evidence of external violence was a cut on the forehead about 2 in. long just to the right of the midline and extending upwards from the root of the nose. He was tender all over his right chest and blood was present in the right pleural cavity. Radiographs revealed multiple fractures of the ribs and a large right-sided pleural effusion. He died about six hours after admission.

At the post-mortem examination there was no evidence of external injury to the chest. The 3rd to 10th ribs on the right side were fractured just beyond the neck, while the 6th, 7th, and 8th ribs were also fractured in the mid-axillary line. The right pleural space contained about 1,000 c.cm. of fluid blood and a small blood clot. The right lung was lacerated by the fractured ribs along the line of the angle of the ribs, there were multiple areas of haemorrhage all over its substance, and it was partially collapsed, a deposit of fibrin being present on the diaphragmatic surface and the lower half of the lower lobe. The left pleural space was intact but contained a small excess of serous fluid, the lung itself being congested but with no definite localized areas of haemorrhage. The gunner had been standing about 40 yards away when the bomb

* The opening address at a B.M.A. meeting (Over-seas Branch), April 30, 1942.