

## The Management of Medical Research.<sup>1</sup>

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TWENTY years have now elapsed since I had the honour and pleasure of addressing Anderson's College Medical School at the opening of its winter session of 1903. This is, indeed, only a short interval in cosmic time; for—to use a figure which will exhibit the rapidity of scientific advance nowadays—all these years amount only to twenty vibrations of the electron which we call the earth round its nucleus the sun, in this atom which we name the solar system! However, for us it has been a considerable period. Many of those who faced me twenty years ago as students are now placed in the seats of the mighty, and will, I hope, support what I have to say to-day. Alas! two of the faces with which I was then familiar are missing—Prof. R. S. Thomson, dean of the Medical Faculty, and Sir James Marwick; some of the distinguished men who were helping us—Dr. Laveran, Dr. Robert Koch, Sir Patrick Manson, Sir William Osler, Lord Lister, Sir Alfred Jones, Sir Rubert Boyce—are no more; and, above all, I must mourn that great pupil of the School, a ruler of many Colonies, and my own master, friend, and supporter, Sir William MacGregor.

On that occasion my address was entitled "Medical Science and the Empire," and in it I described the efforts which we were making to reduce malaria in British possessions. Four years previously we had verified, corrected, and completed the old conjectures that malaria is carried in some way by mosquitoes; and three years previously the Americans had proved the similar conjectures regarding yellow fever. Schools of Tropical Medicine had been established in Liverpool and London, and were about to be created in many parts of the world. At that time I myself hoped that malaria would be banished in a few years from all our principal cities in the tropics; and I had visited West Africa from Liverpool on three occasions for that purpose. I shall never forget the assistance rendered during my second and third visits by two Glasgow men, the late Mr. James Coats, who gave us two thousand pounds to start our anti-malaria work in Sierra Leone, and Dr. M. Logan Taylor, who remained in West Africa for two years, carrying out the practical measures and trying to persuade the local authorities to continue them.

My address—which I believe was not published, but which I still possess—was full of that morning enthusiasm. I argued that the time had already come when medical science could revolutionise the tropics; when it could render them worth living in by banishing the great endemic diseases which overshadowed them; when it could assist civilisation (coming from the temperate regions of the earth) to conquer the rich regions of the Sun and of the Palm. I even dared to quote the great words of the poet regarding Columbus, that he

"Gave to man the godlike gift of half a world;"

and I hoped that we should be able to do the same. This had been the faith which had compelled us—

<sup>1</sup> An address delivered to the Anderson College of Medicine, Glasgow, on October 9, at the opening of the winter session.

others besides myself—for many years: not to add to abstract science, not merely for the sake of parasitology or entomology, not to compile text-books or to fill libraries; but to help the sick and the dying—millions of them—and so to open up the world. When I last spoke to you I hoped that all this was going to be done in a year or two! I am wiser now. Kipling says that we must not try to hustle the East; so, I have found, we must not try to hustle the West either! Men think slowly. It requires a new generation to understand a new idea, even the simplest one.

Some notable advances have, however, been made. Mosquito-reduction against malaria was first urged and defined by us in Sierra Leone in 1899; and was commenced there by Logan Taylor and myself two years later in 1901, and, almost simultaneously, by the Americans under W. C. Gorgas in Havana, and by Malcolm Watson in the Federated Malay States. In 1902 Sir William MacGregor and I visited Ismailia on the Suez Canal—with the result that malaria was banished from that town within a few months. Then the Americans commenced the construction of the Panama Canal, with Gorgas as chief of their sanitary staff, and kindly asked me to visit Panama in order to see them at work in 1904. The result is well-known—the Canal is now finished, with a minimum loss of life. But you are probably not so familiar with the equally great work of Malcolm Watson in the Federated Malay States—because it is merely a British achievement! For more than twenty years he and his friends have fought on against King Malaria and all his allies—rain, heat, jungle, marsh, and ignorance—and is gradually winning forward, step by step. While Gorgas had behind him the full official support of the wealthy American nation, Watson and other British workers in this line have been mostly obliged to rely only upon private initiative and such small funds as they could rake together for their purpose. Not less important has been the work of the entomologists, from F. V. Theobald onwards; but I am not now narrating the history of this movement, or I could speak of many other brave efforts made during these last twenty years. Not perhaps quite as much as I had hoped for, but still something. What may be called "economic sanitation" among our troops, our officials, and our large and numerous plantations, has been greatly improved, and thousands of lives and thousands upon thousands of cases of sickness have been saved. Perhaps, even already, we may echo the words of the Duke of Wellington: "Yes, 'twas a famous victory."

During the same period science has won or is winning many other victories as great. As regards tropical medicine, we have been advancing against plague, cholera, typhoid, sleeping-sickness, kala-azar, hook-worm, beri-beri, bilharzia, and leprosy; and as regards the diseases of temperate climates, we have diminished child-mortality, diphtheria, tuberculosis, numerous ailments due to local infections or to physiological insufficiencies, such as myxœdema, and, quite recently, have inflicted a defeat upon diabetes. We

are getting on. How? By patient, obstinate, and ineluctable investigation—not in the fields of medicine only, but also in those of physics, chemistry, and zoology. Finally, it is just here that we have scored our greatest victory—against our own stupidity. We, or let us say the public outside these walls, are at last beginning to learn that investigation really matters: we are discovering discovery!

It was not always so, even among doctors. I remember a medical administrator saying, "I cannot keep a number of men idling about here with microscopes," and a High Commissioner exclaiming, "You say you do not know how to manage this outbreak! Surely you medical men *ought* to know." He seemed to think that all we had to do was to consult the Hippocratic Books. The idea that investigation is an essential part of practice has been of very slow growth. In India, when a European doctor was asked to cure a lady of the zenana, he was at one time not allowed to see her, and she was not permitted to do more than put out her tongue at him from behind a curtain. A distinguished English physician, who was, I believe, connected with my own family, is said to have deprecated all clinical examinations: we should know how to cure by instinct. To the public mind the physician loses caste by "wanting to know." He must practice, he may teach, but he should not require to investigate anything!

It has taken us centuries to free ourselves from the serpentine coils of this prejudice and to reach our present position—where investigation is the key-industry of all industries. The evolution of this revolution is interesting. The ancient Greeks certainly valued, not only practice and teaching, but also discoveries when made; yet we are not aware that they ever explicitly organised or encouraged research. Readers of the history of science often wonder how the old philosophers and geometers managed to live at all—probably by teaching and possibly on patronage or charity. They were private enthusiasts, and their fundamental discoveries do not appear to have been rewarded in any way. I am told that it is not known whether Plato demanded fees, as well as a knowledge of mathematics, for admission into his Academy; and the same may be said, I understand, regarding Aristotle's Lyceum. Several of the mathematicians, such as Eudoxus of Cnidos, appear also to have been practising physicians. It is to be presumed that the Museum at Alexandria was in essential particulars like a modern university, where teaching is the official duty of the staff, but where research and practice may be conducted at option between the lectures and classes, often with the assistance of students. We are told that after the collapse of the ancient empires and about the time of William the Conqueror, when Europe was plunged in darkness, the Arabs in Spain possessed a library of 600,000 volumes, an academy, and a fund for the endowment of learned men, probably employed for teaching.

Europe did not advance so far as this for centuries, but the monasteries maintained many learned monks, such as Roger Bacon, with whom the new dawn of science commenced. The great Italian anatomists of the sixteenth century were either practising physicians or members of universities. I think that the first real

"research institute," subsidised by public and private funds for pure investigation only, was the famous Uraniborg of Tycho Brahe, founded in Denmark in 1576. It well subserved the proper purpose of such institutes, which is the collection of numerous and exact observations and measurements that are beyond the power of private investigators. Tycho Brahe brought no new integration into astronomy, and even opposed the fundamental theory of Copernicus; but his data enabled Kepler and Newton to revolutionise the science. It is interesting to note that Copernicus himself was only a "private enthusiast," a man of affairs, and a physician; and also that after twenty-one years the politicians stopped their subsidy for Uraniborg, as Mr. Alfred Noyes has described so pathetically in his fine epic of science, "The Torch Bearers." In those days the greatest men were often obliged to pick up a living as best they could—even by the use of alchemy and astrology. Kepler said sarcastically that "Mother Astronomy would surely starve but for the earnings of her daughter Astrology." Even in the observatories and museums which began to be founded after Uraniborg, official duties must have greatly interrupted investigation.

Thus we see that at all times, as often to-day, science has been compelled to get her living by more lucrative but less important pursuits, especially teaching and professional practice. Last century, however, the idea of special research institutes was taken up again with vigour, and the Pasteur Institute in Paris, the Jenner Institute in London, and a score or more similar foundations were established in most of the world's great cities, sometimes by private benefactions or bequests, sometimes by State subsidies, and often by both. Here we find a new principle at work—that of maintaining skilled investigators for research only, apart from teaching and practice. Allied to those, we now possess numbers of industrial research laboratories employed by commercial companies on the improvement of agriculture or of manufactures—and we know what America and Germany have done in this line. Then, again, our hospitals now possess laboratories both for clinical pathology and for research; while the professorial laboratories in all departments of science at our universities have been greatly enlarged and improved, though teaching is still, and quite properly, a part of their duties. Yet another advance is that of research scholarships, by which numbers of promising students are now employed for a few years on such investigations as attract them.

Lastly—and at very long last—the State itself has now joined in the pursuit of truth by means of large annual subsidies, such as those which are distributed in Great Britain by the Department of Scientific and Industrial Research and the Medical Research Council. It would be a difficult task to form even a rough estimate of the world's present expenditure on subsidised research. I think it must reach quite a million pounds a year. This is a small sum compared with the world's expenditure on armaments or education; but it is an improvement on the time when Socrates was obliged to argue in the market-place or Diogenes to fulminate psychoanalysis from a tub.

The improvement has been greatest in connexion with medical investigation. It was not so many years

ago that an American who had studied the matter told me that the world then possessed many fewer endowed professorships on pathology and hygiene than on Sanskrit, philosophy, and theology. This was rather surprising. Every one in the world is certain to suffer from some malady at least once; but no one need suffer from Sanskrit or philosophy unless he pleases, nor even from theology—during this life. But there has always been a thin vein of unreality in academic affairs. Now, however, even Sanskrit is beginning to pale before cancer. On the other hand, so recently as last June, I saw the announcement that the chief countries of the world contribute annually an average income of 9,594,254*l.* to the various Protestant Foreign Missions. This is nearly ten times the amount which I conjecture the world is now giving for the whole of its scientific investigations in all fields. North America gives to the Missions an average of 6,327,597*l.* a year and Great Britain gives 2,310,000*l.* a year; Germany has dropped out owing to the fall of the mark, but other countries contribute the balance. We are not jealous, but our mouths water at the thought of these vast sums. On one side, the missionaries, from your great *alumnus* David Livingstone onwards, have been the pioneers of civilisation and have done great work. On the other side, we think of the millions of people now dying prematurely every year of diseases which are probably easily curable or preventable, though we do not know how to cure or to prevent them at present.

On the whole, I think that the war-funds of science are likely to go on increasing year by year as the public becomes more and more convinced of results. The fundamental question is therefore now being asked, How best should we spend the money? Remember that, as I have shown, the endowment of investigation apart from teaching is only a recent innovation, and probably, like all new methods, has not yet been perfected. How can the best results be obtained for the least expenditure? The question must ultimately be decided by you young men: for us it remains only to attempt a preparatory analysis.

Regarding *medical* research there are two schools of opinion, which we may call the *Bulls* and the *Bears*. One school, the Bulls, say: "We must spend every penny we can raise on constant investigations managed by capable committees and carried on by trained research workers, maintained if possible for life in order to be sheltered from the necessity of teaching or practice, and provided with the most up-to-date laboratories, plenty of materials, and easy access to scientific literature. It is true that some money may thus be wasted, that some of the results may prove wrong, that some of the workers may not turn out so capable as they were thought to be: no matter. A single great success will be worth all the money that is likely to be spent in this way. Pour out the cash; catch all the young men you can and set them at their measurements and microscopes, and keep them at it as long as they are willing to stay. The larger the number of seekers the larger the number of finders. Drop the failures, cut the losses, and think only of the profits." To them the other party, the Bears, reply: "You can spend what money you like but you cannot buy discovery. All that your managing committees

and trained investigators are likely to do or achieve will be the study of details along already well-trodden paths. They will inoculate legions of rats and guinea-pigs, and will publish profound but incomplete papers every quarter, which will be of little or no use in practice. They will carry out researches—yes, academic researches, and too many of them! But the world does not ask for researches; it asks for discoveries—not for the incomplete but for the complete article. Has a single great medical discovery been made by managing committees and subsidised investigators? Discoveries are made by genius—and that you cannot buy."

Such are the opinions which one hears on both sides. Personally I agree and yet disagree with both. There is only one way to decide. Research and discovery are themselves natural phenomena, and we should study them scientifically. I said we have discovered discovery: let us also investigate investigation. How? By consulting the great and triumphant history of science, particularly the stories of the chief advances. If we do so we shall see that the two parties are merely quarrelling over the two faces of the same coin. Science proceeds, not in one, but in two ways: first by collecting facts and then by basing inductions upon them. Thus, in the classical example already cited, it was Tycho Brahe who spent his life in collecting trustworthy observations regarding the positions of the heavenly bodies, but it was his pupil Kepler who, after twenty-five years' study of Brahe's figures, established the great induction that all the planets move in similar elliptical orbits round the sun; and it was Isaac Newton who, eighty years later, explained all these orbits by the single law of universal gravitation. That is, one man collected the facts, but other men explained them. For a second example: by the middle of last century numbers of workers, including Buffon and Linnæus and a host of private enthusiasts and amateurs, had observed, distinguished, and described innumerable kinds of plants and animals; then came Darwin, who explained these facts—much more numerous than he could ever have collected single-handed—by his theory of natural selection. For a third example: think of the host of physicians, surgeons, and apothecaries who have studied and described the characters and symptoms of human maladies without being able to explain them. Then came Semmelweiss, Pasteur, Lister, and Koch, who created bacteriology.

Certainly observation and induction have often worked together in the same research, with brilliant results. More often they pull different ways and break down. Every one knows the man who begins with his induction and then fits his facts to it—or thinks he does. On the other hand, the "working hypothesis" frequently suggests invaluable, though possibly negative, experiments. Then we have the men—generally young men—who make a new generalisation with every new observation: I was one of them once. Often, however, observation and induction require very different faculties, which belong to different men, often living in different ages. If we were all Newtons there would be no problems left to solve.

Science needs all the faculties—the eye of one man, the hand of another, and the brain of a third. Observa-

tion is at least as necessary to it as induction. Therefore I do not agree with the party of the Bears when they depreciate subsidised investigations carried out by full-time workers under managing committees. The present state of medical science requires constant physiological, pathological, therapeutic, and biochemical researches, often involving delicate measurements which cannot be made by medical practitioners outside laboratories, or even by teachers in the medical schools in their spare time. Spend therefore as much money as you can raise for this purpose; let every budding Pasteur have his chance; and pray for a Rockefeller. But at the same time considerable waste must be expected and allowed for. One does not envy committees of management. As Sir Ernest Rutherford recently said in his address to the British Association: "Those who have the responsibility of administering the grants in aid of research for both pure and applied science will need all their wisdom and experience to make a wise allocation of funds to secure the maximum of results for the minimum of expenditure. It is fatally easy to spend much money in a direct frontal attack on some technical problem of importance when the solution may depend on some addition to knowledge which can be gained in some other field of scientific inquiry, possibly at a trifling cost."

I can adduce many other difficulties. Workers are apt to be called away to other posts before their task is complete. Then who can know when an old vein is exhausted, or whether a proposed new line is really promising, unless he himself has worked at the job?—and few committees can consist of specialists in all possible lines. In my own subject I have known men employed who had never read the literature, who dug up again old disused workings, or who chased the wild goose with a pinch of salt for years—all costing money. But the greatest waste is caused by the large number of incomplete articles, constantly being published, which, though they may be good so far as they go, are lost in the mass of literature—so that when the man who clears up the question finally arrives he is obliged to rediscover all the matter for himself. But in spite of these difficulties I agree with the Bulls. The world must continue spending money in this way; and it will improve the system with practice.

Now for the other side—the obverse of the medal. One of our most distinguished physicians told me a few months ago that some one had accused him of not really being a man of science because he did not work in a laboratory! Yet he has made more valuable additions to medical knowledge and practice than has fallen to the lot of most laboratories. Consider this point carefully. The work of the laboratory has almost always been the collection of facts and measurements, the elaboration of detail, the testing of theories; but the other side of science, the great inductions which have solved problems or have applied facts directly to the cure or prevention of disease have been made mostly by that humble individual, the "private enthusiast"—generally either a teacher or a "mere doctor." William Harvey was a mere doctor; Edward Jenner, a mere country doctor! What laboratory did Jenner require? He did not even use a microscope, and yet he gave to humanity the greatest single boon

which it has ever received, and also initiated our present knowledge of immunity. G. F. E. Küchenmeister, who first proved alternations of generations in parasites, was a practising doctor. Pasteur was a professor of chemistry. Lister was a practising surgeon in Glasgow. Robert Koch was also a mere practising country doctor when he discovered the bacilli of anthrax and of surgical sepsis, the staining of bacteria, and plate-cultivation, thus making practical bacteriology. Manson was a doctor in China. Laveran, Bruce, Reed, and Leishman were or are army doctors. Need I mention any more names?—I should have to hurl almost the whole history of medicine at you! Where were the laboratories of these men?—in their own hospitals and consulting-rooms. Where were the laboratories of Kepler and Newton?—in their own brains. Who are making the innumerable advances which we see to-day in connexion with medical, surgical, and sanitary practice regarding almost all diseases? Very largely our professors, our teachers, our laboratory workers, it is true; but also, and not less, our clinicians and our hygienists.

We see then that there is much to be said for the Bears as well as for the Bulls. It is an historical fact that most of the greatest advances have been made by men who were not subsidised for their researches. I think, therefore, that the whole field of public support for science should be broadened so as to include such men. At present the public gives considerable sums for institutional investigations with the test-tube, the scalpel, and the microscope, but little or nothing for workers outside. That is, it supports, and rightly supports, observational science, which is largely ancillary, but scarcely helps those great intellectual investigations which mostly obtain the final or useful results. It would have subsidised Tycho Brahe's observatory at Uraniborg; but it would probably have refused a penny to Kepler, or to Newton, or to Jenner. It pays for digging the foundations of the Temple of Medical Science, but leaves the building of the walls and towers to the practitioner and the enthusiast—often at their own cost. It pours out money for the expectation of discoveries to come, but refuses to give anything for discoveries already completed by private individuals!

It seems to me that all this is very "bad business." We should pay not only for expectations but also for results. I should like to see the whole medical profession brought into the research fold—not in laboratories, but in their practice, their consulting-rooms, and their own brains. Some one will say that the private enthusiast will continue to work whether we help him or not—surely the meanest argument ever used!—but will he? Then some one else will exclaim that there is nothing to hinder any and every medical man from investigation. I am not so sure. True, hundreds or thousands of them are now actually thus engaged, and, in fact, are obtaining the important results just mentioned; but large numbers of medical men cannot always afford such a luxury, because they have to maintain their practices. The reason for this is that while clinical researches which improve medical and surgical treatment often *enhance* practice—and very deservedly so—other scientific work, such as physiological and pathological studies, which are off

the main lines of clinical research, often notoriously *injure* practice. There is still a feeling that a man will not be "a good doctor" if he takes to flying the scientific kite too often. Thus every one knows that both Harvey and Jenner ruined their respective practices by their scientific studies. For another example, it was said of Thomas Young, the father of physiological optics and discoverer of many great theorems on light, heat, and energy, that he "was not regarded as a successful practitioner, because he studied symptoms too closely, although his treatment was admitted to be effective." In other words, he cured his cases by studying their symptoms instead of studying the correct bed-side manner! Wise or not, this feeling has to be considered by practical men. Then there is a third class of effort—perhaps the very highest class of medical work—which is concerned with the prevention of the great epidemic diseases. At present it receives no payment whatever, either from practice or otherwise. What has been done, for example, for Mr. W. M. M. Haffkine or for Mr. H. E. Hankin—both laymen and private enthusiasts—whose studies have saved untold numbers of lives from cholera and plague in India and elsewhere; or for the almost unknown doctors who discovered that plague—the world-destroying plague—is carried by the rat-flea?

Such drawbacks, and others, are unfortunate, because they tend to impede enlistments in the great voluntary army of medical science. Our friend the private enthusiast is a rare species; and the successful enthusiast is very rare indeed. You cannot subsidise him beforehand, because you cannot discover him until he has *done his work*. You can supply him with laboratories, test-tubes, and microscopes—if he wants them, but you cannot pay him for his thoughts, his calculations, or his natural aptitude, nor, above all, for that passion for discovery—for discovery not merely for investigation—which drives him over every obstacle to his ultimate goal. You cannot subsidise him, and you cannot reward him either. It is beyond the power of the whole earth to reward him; his discovery is his reward. But still you can do something for him in a small way. In 1802 and 1807 Parliament compensated Jenner for the loss of his practice; in 1884 the German government did the same for Robert Koch; and quite recently, I understand, the Canadian government has, very wisely, shown the same consideration to Dr. Banting for his brilliant labours on insulin.

The *least* that the world can do for the successful investigator, whomsoever he may be, is to pay honourably such of his little out-of-pocket expenses and losses as he may have incurred in the world's service; and the *most* that the world can do for him is—to keep him at work. This is the way in which money can now be most profitably spent for science. I see that Sir Alfred Yarrow has recently given a fine donation, which is to be devoted partly to this purpose. If I were a millionaire I should follow his example.

It is often said that there is no such thing as discovery, that each advance is built upon previous advances. True; but what is the interval between these advances? Many people carry on incomplete investigations, and just miss their triumphant culmina-

tion. The culmination is the discovery. I have often wondered how it was that those wonderful people, the ancient Greeks, missed four great discoveries which they seem to have been on the point of achieving—the calculus, evolution, electricity, and vaccination. As it is, the world was obliged to wait for nearly two thousand years before these little "advances" were made. It awaited the proper men. Only the other day an able biochemist told me that probably most of the facts regarding the complicated diseases of metabolism are already known, but that another Newton is required to integrate them. Such, I think, may also be the case regarding other grave medical problems, as, for example, that of cancer. Possibly the discovery may already be made, but there is no one to drag it forth into the light. In science, as in art, the man is everything.

I must make one more remark. What always amazes me is the fact that there are millions upon millions of human beings whose health and whose very existences are constantly threatened by numbers of diseases, and yet who never subscribe one farthing for the medical researches which endeavour to defeat these terrible enemies of theirs, and often succeed in doing so. Yet thousands of these same people pour out their subscriptions and bequests for all kinds of projects, many of which are futile; while even those good and generous people who maintain our hospitals and universities seem often to forget that behind hospital practice and behind university teaching there is the great science which inspires both.

I have tried to give you a brief review of what may be called the natural history of discovery. "The management of medical research" will lie in the hands of you young people; but you must study the book of the past in order to direct the advances of the future. I hope that most of you will be "mere practising doctors"; but, if so, let every afferent and efferent nerve of your thoughts connect the brain of science with every sense, muscle, and faculty of your practice. The practitioner nowadays cannot live apart from science, trying to evolve wisdom from his own meditations, like a hermit in the desert: you must not only observe, but also think; and not only think, but also read. Your first duty will be the cure or prevention of sickness; but some of you in your leisure may perhaps try to solve problems, may become enthusiasts, may even become wild enthusiasts!—I cannot imagine a nobler fate. Even, perhaps, one of you—probably not more—may be destined to become the Newton or Einstein of some hitherto undreamed-of synthesis. I hope so.

Science has indeed measured the stars and the atoms, has knit together the corners of the earth, and has enabled us to fly over oceans and deserts; but her greatest victory remains to be won. Why should we men, heirs of all the ages, continue to suffer from such mean things as diseases? Are you going to be defeated any longer by bacilli, rat-fleas, and mosquitoes? It is for you to conquer them; and remember that every gift of science is a gift not to one country or to two countries, not only for to-day or for to-morrow, but also to the whole world and for all time, until, as the poet said,

"The future dares forget the past."