SIR DAVID BRUCE: AN APPRECIATION OF THE MAN AND HIS WORK

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I SHOULD like to begin by saying how much I appreciate the honour of being asked to give this lecture and also to say how inadequate I feel to my task.

It is one thing to have known a remarkable person and quite another to convey his quality to a later generation. If, when I have done my best, anyone says to me, "But that is not the David Bruce that I knew," the answer is simple. During Bruce's lifetime there were several dozens of him that walked the earth reflected in the minds of those who knew him. Now that he has been dead for twenty-four years this number diminishes, but no doubt as large a collection of phantoms animates his legend.

I am describing Sir David Bruce in the light of the man as I knew him in the last twenty-two years of his life. I am naturally giving an account of him chiefly from my angle, which was that of a much younger worker, but not that of a junior officer serving under him in the R.A.M.C. But I also hope to give some idea of his peculiar quality as a man and a scientist.

Bruce was born in 1855 and he was in the middle fifties when I first met him. He had still ten years of service in front of him.

This lecture makes no claim to be in any sense a complete biography, though it is naturally biographical in character, and I should like to acknowledge my debt to the excellent obituary notice of 1932 in the Proceedings of the Royal Society.

In thinking of Bruce one has always to remember his period. Born during the Crimean War, and reaching Edinburgh University in 1876 at the rather late age of 21, he came under the general influence of the great expansion of science, including medical science, of the last half of the nineteenth century.

Biology in its widest sense made great strides in the years from 1876 to 1886, and Bruce had always a particular interest in natural history ; indeed most of his work shows the biological approach. The new outlook in medicine, and particularly the recognition of the causal agents of communicable disease opened up by Pasteur and Koch and many others, were bringing a change in the attitude of mind of the younger men.

Lister was already well on his way to revolutionize surgical procedures, and all this was going on round Bruce in those formative years.

One could hardly have found a man more admirably suited by temperament, by the peculiar type of his intelligence and by his character to this particular period in medical science. There were problems, especially in ætiology and in

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tropical medicine, awaiting just Bruce's type of attack. He had a very direct mind, the biological acumen and the tenacity to make a success of any problem in this field where the frontal attack was applicable. A man is very fortunate when he suits the age into which he is born.

Bruce qualified in medicine in 1881. It did not take him long to discover that he was not cut out for private practice, but the attempt was made at Reigate, where he had the good fortune to meet the woman who became his wife. She was Miss Mary Elizabeth Steele and was the daughter of a distinguished medical practitioner of that town. They were married in 1883 and Bruce then joined the Army Medical Service.

It was when Bruce was stationed in Malta as an Army Medical Officer only a year later that he made his first remarkable contribution to medical knowledge by discovering the infective agent of Malta fever, which was the cause of a great deal of serious sickness among the troops and sailors in the Mediterranean area. He called the organism *Micrococcus melitense*. In 1920 Meyer reorganized the group, which by this time contained also the closely allied type discovered by Bang and found to be the causal agent of the most serious form of contagious abortion in cattle, under the name of *Brucella* with the two species *Br. melitense* and *Br. abortus* and a third less generally accepted *Brucella tularense*.

During a series of investigations in Malta in 1904, when Bruce was chairman of the Royal Society's Malta Fever Commission, the connection of Malta fever in man with the infection in goats and its dissemination by the use of the milk of these animals was established by Zammit.

After his first spell of work in Malta, Bruce, accompanied by his wife, spent some time in 1889 in Robert Koch's laboratory in Germany.

I am not going to trace Bruce's scientific work in detail as, to begin with, it will be familiar to many of you and it can be read about in the appropriate places. But I do want to draw your attention to Bruce getting into his stride in the middle eighties of last century, applying the then new techniques of the infant science of bacteriology, and using his frontal attack with the greatest success. In a brief sketch like this I propose to select certain aspects of Bruce's work for attention. I want to give you an outline of Bruce's admirable attack on the problem of nagana in 1894-1896 and an account of some of his activities in the First World War, particularly his interest in the occurrence of tetanus among troops in the field and in hospital.

Nagana

The nagana research in Zululand in 1894-1896 is not only an excellent piece of work but is very characteristic of his capacity. He was accompanied on this expedition by his wife. In retrospect, in spite of the considerable hardship involved, Lady Bruce spoke of it as one of the happiest periods of their lives.

In 1918 Sir David gave me a copy of the Nagana Report ceremonially inscribed in red ink. It is a valued possession. It is interesting to note in passing that the author is described on the title page as Surgeon-Major David Bruce,

A.M.S. This rank is now I believe obsolete, and the date of the report is before the reorganization of the Army Medical Service in its modern shape.

Bruce had when he started out in this field of research certain things to go on : (i) Nagana was present in a particular type of country. In Zululand the area chosen for the study was low and humid and lay between two rivers. This, to quote the report, was " 'fly' country, the home of nagana and malaria and uninhabited except by wild animals." (ii) The "fly" was identified as *Glossina morsitans* or the tsetse fly. (iii) It was already known that the disease was characterized by—to quote again—"The constant occurrence in the blood of an infusorial parasite either identical with or very closely resembling *Trypanosoma evansi* found in Surra, a disease of horses in India and Burma." Bruce had already seen this organism in the blood of African cattle in 1894.

Two local theories concerning the disease were current : (i) That held by the white settlers, that the fly caused the disease and might by itself have a poisonous bite. (ii) That held by the African natives that the presence of game in large numbers caused the disease and in some way contaminated the grass and the drinking water with their saliva or excretions. Both these theories have, as we now know, some element of truth in them.

There was another theory that the disease was the product of certain physical conditions obtaining in regions to a certain degree tropical, and that the immediate cause was either malaria or a vegetable poison. Bruce, as you will see, kept this third hypothesis in mind, but as we now know it was far from the mark.

Bruce established himself on a hill above the fly area where he could keep his experimental animals outside the danger zone. He started by finding out what he could about the fly itself. He then obtained a small supply of tsetse, confined them in a gauze-fronted, little cage and fed them for some time on normal susceptible animals, mostly dogs but also horses and donkeys in order to retain the natural conditions of the disease. When he had established that the animals had not become infected, he then showed that the flies themselves even when broken up in saline and injected under the skin of normal dogs were not able to produce the disease. Bruce had a bit of luck here as one of these flies might have been a carrier and at this time he had no source of known clean flies. In his later work, when the pupæ of *G. palpalis* could be obtained in numbers, there was always an abundant source of newly-hatched flies which had never fed.

Having thus cleared the flies as a source of nagana in themselves, he turned his attention to seeing if, and how, they transmitted the hæmatozoon or "virus," as he was still apt to call it.

He allowed flies to feed on heavily infected dogs and then transferred the cages quickly on to normal dogs and found he could transmit the disease. This was known to later workers as direct transference of the disease as distinct from cyclical transmission. Bruce did not at the time of the report think of a cyclical transmission, but found that the direct transmission could occur occasionally, but much more rarely, after an interval of twenty-four hours, and on one occasion after forty-eight hours.

Bruce was in no sense a protozoologist and never became one. His interest in

the Trypanosome was as an agent of disease and he was not touched by the controversies about the nature of the nucleus or the kinetonucleus and so on. He was unmoved when he learnt that the useful and simple fixation by drying and the use of the Leishman-Giemsa stains burst the nucleus and gave a pretty but inaccurate picture.

He tried drying the infected blood on a piece of thread and slipping it under the skin of an animal and succeeded on rare occasions in producing an infection ; probably a few trypanosomes were imprisoned alive in a not completely dry clot. He concluded that the blood soon became inert on drying.

At this stage I want to point out that the transmission of malaria by mosquitoes had not yet been discovered, and there is an interesting and rather touching paragraph in which he discusses the result of taking a horse into the fly belt and preventing it from eating or drinking, while observing that many flies settled on it. In discussing this the thorough Bruce does not consider that the means of passage *must* have been the fly because, as he writes : "There may have been other ways of taking the disease, for example, inhalation. The disease called ague or Malarial Fever in man is of all diseases probably the one most nearly related to the fly disease in animals. They are both caused by blood parasites belonging to the Protozoa, and both are found under similar physical conditions. In the case of the much studied and familiar Malarial Fever, none have, up to the present, had the courage to assert that man could be immune by merely taking care what to eat and drink while in a malarial district, or in other words only to eat cooked food and drink boiled water while there. On the contrary it is asserted by the latest authorities that merely breathing the air of malarious districts is sufficient to set up the disease ; in other words, that the parasite can obtain entrance into the system from the air."

The paragraph is too long to quote *in extenso*, but Bruce feels sceptical about the hæmatozoon being able to form a spore in which it can exist as a dry impalpable dust. Indeed he well might.

He settled the main point, however, in another way ; he brought up flies from the fly belt in large numbers and let them bite a horse which was carefully kept on the hill, and successfully infected it. He also found that the blood of the wild game shot in the fly belt would cause infection when injected into his experimental animals.

This is Sir David Bruce at his best—again the energetic frontal attack, the sifting of the evidence and the tenacious testing of his own conclusions. We must remember when we read work of this date that there was no "cloud of witnesses" whose published work could give directives to help in the interpretation of the facts which often stood alone, and the scientist had to make what he could of them and come to some conclusion.

Bruce quotes some curious errors current at the time of writing the report, which he did not question, such as that *T. lewisi* in the rat and the trypanosome of surra were identical. And Lingard had averred that he had caused the disease in a horse by injecting *T. lewisi*. Lingard noted that the incubation period was very long—63 days—but that the infection was very virulent when it developed.

He obviously had not ensured that the horse did not become exposed to a surra infection on its own.

This report laid the foundation of the trypanosome work in Africa, and though there was much more to be done, a proportion of which was carried out under Bruce's direction, the broad outline was clearly contained in this research. The most important filling in of the story was done by Kleine and his co-workers who discovered the cyclical development in the tsetse.

This early work of Bruce's was done in Zululand under great difficulties and in isolation except for the ever-present help of Mary Bruce, who stood up to the rough life; used her skill and ingenuity in constructing all manner of useful things; made stained preparations; did a lot of the searching of fresh blood films for the flicker of the trypanosomes' undulating membrane, and took her full share of the field work even with a rifle.

The Bruces spent several more years in South Africa, and Sir David served with the Army as a surgeon during the Boer War with his wife doing the work of a nursing sister. They were involved in the siege of Ladysmith.

Bruce had been elected a Fellow of the Royal Society in 1899 while absent in Africa, and from the period of his return in 1901 he was recognized as a worker of great importance.

I have dwelt particularly on the nagana research as Bruce did this work by himself with Lady Bruce, but not as the leader of an able team of younger scientists. From now onwards he began to be more and more involved with Royal Commissions and to find his talents employed in inaugurating and co-ordinating the researches of a group of workers. In fact we have here one of the early examples of the use of a team to attack scientific problems or to meet a medical crisis.

It comes with a certain shock of surprise to the modern scientific worker that this method is little more than fifty years old.

Bruce was seconded by the War Office early in 1903 to supervise the work of the Royal Society's Commission sent out to deal with the very serious outbreak of sleeping sickness in Uganda. His experience with nagana quickly showed him the main lines on which to approach the problem, and in a few months the disease was found to be another trypanosome infection with the tsetse fly *Glossina palpalis* as the insect vector. This work brought Bruce the award of the Royal Medal of the Royal Society for the year 1904. After this, honours of various kinds accumulated, all of which he honestly enjoyed without any pretence. His later trypanosome work at Mpumu in Uganda and in Nyasaland was less personal, and he had very able collaborators, such as Captains Bateman and Hamerton of the R.A.M.C., to mention only two. Many other workers made valuable discoveries and the body of knowledge is now a formidable literature, but the problem of the control of trypanosomiasis in Africa is not completely solved even now.

At this point I should like to say something about Lady Bruce. No good account of Sir David can be given without referring to his marriage. It was a most satisfactory partnership and achieved a completeness and a beauty that was

an interesting comment upon the harsher aspect of Bruce as seen in the eyes of some of his contemporaries.

Lady Bruce was a woman of great character and great courage. She was generous-hearted and gifted with both wit and humour. She was well educated in the truly civilized way of a certain type of Englishwoman in the latter half of the nineteenth century. Her gifts were many; she could draw and sketch well and was musical. Lady Bruce made any shack or hut into a place fit to live in and endowed it with a touch of beauty, and most characteristically she was a gardener. She was a hardy traveller and a good shot. The African women around Mpumu used to tell me how she would cycle down to the lake with a rifle over her shoulder and that she could shoot a hippo. In fact, she was, they said, a "Kitalo," which means a wonder.

Lady Bruce was careless about dress, but it did not matter. When I went to visit her in 1911 before going myself to work on Mpumu after the Commission had left, she very kindly gave me all sorts of good and useful advice, nearly all of which I followed to my great advantage, except in the matter of millinery.

Incidentally, I found a neglected garden on Mpumu which had been Lady Bruce's and which was not too difficult to bring back into shape. It afforded me much pleasure.

Mary Bruce had no pretensions to good looks as such, but her vivid personality shone through and she was a fascinating woman. Her influence on Sir David was very great and, while she was never in any true sense a scientist, she was a continual inspiration to him and helped in the work of detail, making excellent drawings and designing and carrying out useful things, such as the curtains round the fly corner in the Mpumu laboratory, to mention only one thing that occurs to me.

Lady Bruce did, I think, canalize Sir David's activities. She certainly curbed his intransigence and his impatience. I well remember having tea with her one day in about 1917 or 1918 while Sir David was Commandant of the College. He blew in with obvious storm clouds upon his brow and, on being asked how the meeting at which he had been had gone, said, "There is a good deal of opposition," upon which Lady Bruce remarked that if he would avoid telling people at the outset what incredible fools he thought they were, he might find things work more smoothly. She was the most loyal of wives and they were practically never parted. In the end, his last few years were greatly saddened by her prolonged illness. Sir David often referred in conversation with me to the extent and the value of her help in every aspect of his work and life.

Bruce towards the period of his later trypanosome work was beginning to pay the penalty of his success, and on returning to England from Nyasaland in 1914 he was made Commandant of the R.A.M. College and, much to his regret, did not go on active service in the 1914-18 war. In 1903 he had become a member of the governing body of the Lister Institute as the representative of the Royal Society, and at the death of Sir Henry Roscoe in 1915 he became chairman of the Institute.

It was at this time from 1914 onwards that I saw a lot of Sir David Bruce as

he took a keen interest in the work going on at the Institute. I had begun to work on the spore-bearing anaerobes present in war wounds and so came into contact with him.

During the year 1915, a disastrous epidemic of typhus broke out in Serbia and there was a general anxiety about the possibility of the disease spreading among the troops and among the populations of other countries. The louse had only just begun to be incriminated as the vector. The infective organism was not definitely known although several bacterial types had been isolated, none of which proved in the long run to have anything to do with typhus.

In 1914 the late Dr. Penfold, who was then a member of the Lister staff, had isolated a hæmolytic coccus from the blood and urine of typhus patients in Belfast. The possibility of this organism being useful as a vaccinating agent in protection against the disease attracted Sir David, and I was asked to carry out the project.

It was not a very good idea as the connection of the organism as a causal agent with the disease was by no means established. Sir David, however, was not impressed by the two main objections ; the one which I have just stated and the further one that vaccination might not afford protection. . . . He had so often been successful with the frontal attack . . . it might be a long shot but he felt it must be tried. So I vaccinated the monkeys in 1916 and I took them to Ireland, where the disease was still to be found in the far west. In the event the vaccination gave no protection and both the treated and control animals took the disease, giving the characteristic febrile reaction and one death. Dr. Penfold's hæmolytic coccus was again recovered from the blood of two patients.

Now we know that typhus was not to yield to this kind of approach.

About this time the War Office set up a Committee for the investigation of tetanus, with Sir David Bruce as its chairman. I am only going to touch rather lightly on the tetanus research, as, while I worked at one aspect of it for some time, Professor Tulloch carried out a much more detailed and extensive piece of work, and I am told he is giving an account of the investigation in the R.A.M.C. JOURNAL. It must be remembered that these were the days before there was any word of an active immunization against tetanus. The injection of antitoxin into all wounded soldiers at the first dressing station had cut down the incidence of this terrible infection to very low numbers. But Bruce's interest was aroused by the occasional late reappearance of tetanus cases especially as a result of further surgical intervention among wounded troops who had received antitoxin at the field dressing station. So at his suggestion I undertook the examination of a series of cases classed as "septic wounds" but showing no symptoms of tetanus.

The cultures grown from 252 cases could be divided into three classes : 32.9 per cent. which showed no anaerobes, 41.6 per cent. which showed growth of anaerobes but none with round endspores, and 25.5 per cent. which showed the growth of organisms with round endspores along with other anaerobes. Amongst this third group 9 or 15 per cent. of the cultures were toxic, producing a tetanus

reaction when injected into mice but not in controls which had been given tetanus antitoxin.

Bruce took a great interest in the work and I remember him coming frequently to see me at the Lister. Sir David in his full General's uniform was an impressive figure, and I used to have a slight feeling as of one about to receive cavalry when he appeared with a certain amount of the accompanying rattle of Sam Browne belt, etc.

I found this particular piece of work rather uncongenial, and I think he appreciated this as he used often to leave me with the rather cryptic slogan, "On, on and no regrets." Exactly what this referred to, I never discovered. There are incidentally some picturesque sayings of Bruce's, one of which appeared in one of his later trypanosome papers referring to some difficulties in African field work : "The sun is hot and the country is difficult." This used to be quoted by later and younger workers on Mpumu in Uganda on hot days with heartfelt agreement.

The final big tetanus report was Bruce's last piece of work and, as I have already said, I am leaving this in the able hands of Professor Tulloch.

Bruce retired in 1919 but served for some years on many advisory committees. He was president of the British Association in 1924 when the meeting was held in Toronto, and his presidential address bore the title "The Prevention of Disease." He started out with an apology, which sounds curiously old-fashioned in these days, for addressing the Association on this matter which might not seem to them to be a very pleasant choice. He excused himself on the grounds that, after all, it was humane and important as advancing the happiness and efficiency of man. This is an interesting address ranging over all the aspects of prevention and over all the known types of disease which were then amenable to prevention. Infections of all kinds from those caused by bacteria and protozoa to the less well understood rickettsial and virus diseases were dealt with ; dietetic deficiencies, then rather a new branch of medical knowledge, and glandular imbalance of various types were all considered.

The first meeting of the Association held in Canada was in 1884, which coincided with Bruce's own start in scientific investigation. He used this span of forty years to show the advances made, and in the last three paragraphs of the address he stated what amounted to his scientific creed.

I quote a few characteristic sentences :

"Before that time (1884) we were still in the gloom and shadow of the dark ages. Now we have come out into the light. Man has come into his heritage and seems now to possess some particle of the universal creative force in virtue of which he can wrest from Nature the secrets so jealously guarded by her and bend them to his own desire. . . .

"Mankind is still groaning and travailing under a grievous burden and weight of pain, sickness and disease . . ." and he finished with the words : "It is the duty of science to go steadily forward, illuminating the dark places in hope of happier times."

Bruce kept his connection with the Lister Institute until his death. Retire-

ment gave him little pleasure as he was without the type of hobby that could be pursued with advancing age and he had no vital concern with the arts. He was a good shot and had been a good tennis player. He resented it when in the mid-twenties he had to spend the winters in Madeira because of a tendency to chronic bronchitis.

As a person Sir David Bruce was picturesque, able and forthright; he had what might be called a practical type of imagination, but was not very subtle. He held the opinion indeed that subtlety belonged to the group of intellectuals that (without using that now almost detrimental term) he considered suspect and thought of as those "long-haired chaps." He had, however, a real enthusiasm—indeed a passion—for science and never doubted that it held the key to progress.

I only actually met Sir David when we were both in London, and I never saw him other than immaculately dressed in morning coat or well-cut lounge suit or perfect uniform.

Bruce belonged to his day and he made very good use of it. He earned the well-deserved distinction of leaving a mark on the Medical Science of his period.

[Dr. Robertson illustrated her lecture with lantern slides selected from Sir David Bruce's own collection.

We are glad to note that the Lister Institute has decided to give Sir David Bruce's large collection of lantern slides to the Royal Army Medical College as being the most suitable body to have the care of them.—ED.]