

AN *(1926-56)*
~~A PERSONAL~~ ACCOUNT OF ADELAIDE MEDICAL SCHOOL AS
SEEN FROM THE POINT OF VIEW OF THE FIRST
PROFESSOR OF HUMAN PHYSIOLOGY AND PHARMACOLOGY
APPOINTED TO THAT INSTITUTION.

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The colony of South Australia was founded in 1836 and had been in existence for only 35 years when the first action was taken to establish a University! There were already public schools serving preliminary educational requirements, and youth desirous of tertiary education had no alternative but to proceed either to Melbourne or Sydney or even further, to Europe, and most students apparently preferred Europe.

Two such students, Joseph Verco and Edward Stirling, returning from London and Cambridge, were the foundation upon which the Medical School of this colony was built.

The University of Adelaide was established by Act of Parliament in 1874, and in 1881 Royal Letters Patent made degrees granted here acceptable in any part of the British Empire.

The Government of South Australia granted 50,000 acres of country lands as an endowment, and 5 acres of parklands adjacent to the City of Adelaide as a site for buildings, and promised 5% per annum on all capital funds to a limit of £10,000.

In 1874, when the University Act was passed, provision was made for the granting of degrees in Medicine, after examination. In 1881 Edward Stirling was appointed Lecturer in Physiology, and in the next two years, the formation of a medical school was discussed. In 1893 Sir Thomas Elder made a gift of £10,000 for the establishment of a Chair and in 1884 the Council adopted a recommendation for Chairs in Anatomy and Chemistry. Physics and Botany were already in existence. So the School of Medicine in 1885 was in the hands of five young men - Stirling (physiology, 37), Watson (anatomy, 36), Bragg (physics, 23,) Rennie (chemistry, 30).and Tate (natural science, 45). Each had to build up his department as best he could with the limited resources available, and during the first two years provision was made to complete a five year course, - the Adelaide Hospital providing the clinical teaching. This brought about the official relationship between hospital and university.

Physiology was originally housed in the basement under the northern end of the Physics department, and in 1886, adjacent to the original university block, there was constructed the initial portion of a much larger medical building, containing a lecture theatre and two laboratories. In 1887 students entered the hospital for clinical work, and in that same year the first female medical student was admitted. The flow of medical students numbered about five per year, and by the end of the first five years there were 27 students.

Professor Brailsford Robertson succeeded Sir Edward Stirling in 1919 and the title of the Chair was changed to Physiology and Biochemistry, and the arrangements of the new Darling Building opened in 1922 are all of his design.

This building, "to accommodate the laboratories necessary for instruction in science fundamental to medicine" was erected by a bequest in memory of John Darling Jnr. It housed the departments of Histology, physiology, biochemistry, pathology and zoology, and was designed to accommodate an annual enrolment of up to 150 students, which was 3-4 times the number then passing through.

in Adelaide

I arrived in March 1926 as Sheridan Research Fellow and Lecturer in Mammalian Physiology. I had come from Cambridge, England, where I had been Beit Medical Research Fellow and Research Student at Trinity College under Prof. Sir Gowland Hopkins. ^{Before World War I} I had already taken the Degree of M.Sc. with double First Class Honours in Organic and Physical Chemistry with the object of following a career in Chemistry and had been in touch with Prof. Wallach ^{of} Göttingen with that object in view. World War I however, interrupted this projected course, and after demobilisation from the N.Z. Expeditionary Force, I was appointed Public Analyst and Police Toxicologist for Otago and Southland with right of consulting practice. My laboratory was to be in the then New Medical School ^{of Otago University,} and my chemistry expertise was to be available to the new Departments of Bacteriology and Pathology. This meant the development of a Clinical Chemistry Lab. to serve the Hospital. Biochemistry at that date was scarcely known as such, except in the U.S.A.. Before accepting this offer, I asked for, and received permission concurrently to study medicine. Together with Dr. P.P. Lynch B.Sc. ^{at the same time} who ~~also~~ started ^{the} on a medical course, I spent some strenuous years coping with a steadily increasing work load of consulting industrial chemistry and clinical chemistry as well as pathology demonstration.

I was therefore a very experienced as well as qualified professional chemist (I had passed the severe professional examination as an Associate of the Royal Institute of Chemistry, London, and in 1922 was elected Fellow) ^W when I applied for Adelaide ^{However,} but I was more of a Clinical Pathologist than a Physiologist. ^{and} In fact, I had already begun research in thyroid disease and had published with Prof. A.M. Drennan a paper in the Journ. Exp. Path. ^I and ~~was~~ continuing this work in Cambridge, where I published two additional papers, one in the Journ. of Physiology, and the second in the Journ. of the Chemical Society.

Whilst working on the ultra-violet absorption spectrum of thyroxin supplied by Dr. E.C. Kendall (Mayo Foundation and later Nobel Laureate) I found that he was on the wrong track looking for a tryptophane connexion and ^I travelled ^{to the USA} as ship's Surgeon ^{in order} to visit him. I also traced the thyroid secretory path into the thoracic duct. But the extremely mixed varieties of experimental animals at Cambridge led me to King's College, Camden Hill, where Gladys Hartwell had for six years worked to establish what might be called a Standard Rat. This was a black and white variety of a Norwegian strain which bred true and gave constant growth rates. These appealed to my long quantitative training and outlook. Hopkins rats were quite effective for demonstrating gross responses, but for more sophisticated studies they were useless. So, I established a Hartwell rat colony in the Cambridge Biochemistry School, and also brought them with me to Adelaide. From the Adelaide colony others have been established in Australia. I should add that I had been invited by the Mayo Foundation to become Bio-Physicist there and to complicate matters I was offered the lectureship in Chemical Pathology and a Fellowship of St. John's College, Cambridge, ^{but this offer} unfortunately, ^{came} after I had accepted the post of Sheridan Research Fellow and lecturer in mammalian physiology, Adelaide. As it was, the Medical Research Council tried to retain me and prevailed upon Adelaide to allow me to remain in Cambridge to complete my work on Thyroxin. I was not to know, and could not have imagined, that my action to keep faith with Adelaide ^{(pharmacology and} would in the event lead to the offer of a Professorial Chair ^{human physiology)} by the Chancellor, Sir George Murray, at the 1926 Commemoration, and to a Knighthood some years later, after declining the offer ^{to be} of successor to Sir John, (later Lord) Boyd Orr, by an Aberdeen University Committee in London.

I record these facts in order to show the quality of outlook and dedication that prevailed to a great extent at that date in academic circles. It must also be remembered that the number of Universities and the number of Departments in those then existing Universities was perhaps one fifth of what exists today in the British Empire. Moreover, communication was by surface mail, once weekly to Europe. A letter took five weeks to reach London. An immediate reply would require three months. The effect on publication of research was such that, even if accepted, a two year delay was

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involved and this was in addition to the delay in publication that might ensue. There were two distinguished holders of Chairs in the Medical School. Professor Wood-Jones the anatomist (he was a noted comparative anatomist) and ^{Prof.} Brailsford Robertson, ^{another} ~~also~~ a scientist of world repute who, although a Biochemist, also taught Physiology, or rather left its teaching largely to Hedley Marston his unqualified but able demonstrator. It was this latter circumstance that had led to the creation of my position. Robertson was not a medical man, and showed his disdain for the medical profession rather openly. He had graduated from Adelaide and had married the daughter of Sir Edward Stirling who had taught medicine and physiology and had fostered Robertson's return to be his successor.

Robertson had worked abroad in California and Canada and held a Ph.D. degree from Berkeley, where he had studied proteins under Jacques ^{Loeb} ~~Loeb~~. He had a brilliant mind and his talents were wasted in that isolated and insulated Adelaide of the period. The scientific gap between Robertson and the medical fraternity was unbridg^eable. Nevertheless this remarkable and talented man was personall^y responsible for raising funds to erect the Darling Building, for the establishment of the Australian Journal of Experimental Biology and Medical Science, The Animal Products Research Foundation, The Medical Sciences Club and the University Club. The very dynamics of the man disturbed the placid surface of the local medical elite, ^{pool of the} ~~pool~~, if one ^{may} ~~can~~ use such a metaphor. The medical families, intermarried with the families of great land owners, constituted a ruling oligarchy. I was an intruder myself because Physiology and Pharmacology were unknown to the oligarchy, literally. But I was at least a medical man, albeit one who was in a lower class, the academic class!

There were to me, however, great compensations. I was free to develop my own department, and the academic atmosphere was delightful. ^{I have} mentioned ^{My colleagues were Prof. Kerr Grant, physics,} ~~my colleagues were Prof. Kerr Grant, physics,~~ Sir Douglas Mawson the explorer, Prof. Chapman the engineer, Prof. Cleland pathologist and naturalist, Prof. Wood-Jones a famous comparative anatomist and entrancing lecturer, Prof. Darnley Naylor a charming, cultured, classicist and Sir Archibald Strong, Early English expert and Orientalist. It was, in effect, a Collegiate atmosphere and, uniquely in Australia, a privately endowed University presided over by a Council of Gentleman. Need I say more?

That physiology did not exist is well illustrated by two or three ⁶ classical examples. At a meeting of the Medical Sciences Club a physician of my age attempted to demonstrate by means of rubber balloons that the heart sucked blood from the venae cavae, contrary to what I was teaching, i.e. that the venous return was due to extra cardiac mechanisms. On another occasion, a leading gynaecologist had learned (from students) that oxygen uptake and output from the corpuscles was largely effected by the carbondioxide and in

fact that the carbon dioxide carriage within the physiological range was a haemoglobin function. This, to his mind, was plain nonsense. Yet one more example: "What's this you are teaching about digitalis? Students tell me that it acts on the vagal centre, not on the heart and that it strengthens the cardiac muscle by preventing excessive lengthening in diastole. Digitalis is a dangerous drug - a poison." And so on. Manifestly he had a garbled account. One would have thought that Erasmus, Darwin and Sydenham had never lived, and that James Mackenzie, Cushing and modern pharmacology had not revealed the fundamental causes of the clinical therapeutic effect.

My first action was to endeavour to introduce operative mammalian physiology after Sherrington, but using the rabbit which was plentiful and a pest rather than the cat which is a sacred animal. But unlike the cat which has nine lives, the rabbit has but one and that held only tenuously. All attempts to decerebrate were failures. I then resorted to anaesthesia.

I was fortunate in obtaining a technician, Ernest Eldridge, a 'man of Kent'. He was "bequeathed" to me by my colleague Frederick Wood-Jones, the Professor of Anatomy, and as demonstrator ^{I had} Dr. R.F. Matters (now Sir Francis). It proved impossible to continue with the part time assistance of Mr. Marston, Robertson's man. Eldridge was quite inexperienced as a technician, but what that 6ft 2 inches of man achieved was beyond praise. Moreover, he had qualities of command which were invaluable. He was held in affectionate respect by the students and also all others with whom he came in contact.

Dr. R.F. Matters had just returned from the U.K. where he had taken a postgraduate course in obstetrics and gynaecology. He had graduated from Sydney University and had been a general practitioner at Woodside. He and I, after much trial and error, succeeded in establishing a safe routine method of anaesthetising the rabbit. It consisted of administering by stomach-tube, paraldehyde in doses carefully based upon body weight. The animal was then kept unconscious by applying ether to a muslin "face" mask. This use of a basal anaesthetic plus supplement - when necessary- led to a perfectly safe and uniform anaesthesia. It also taught the student an important "clinical" appraisal of the anaesthetic state. The "anaesthetist" for the day came into the practical lab. at 1.30 p.m., weighed his rabbit, calculated the dose, and

placed the animal in a holding box and then affixed a wooden gag in the mouth, through a hole in which a rubber catheter was fed into the oesophagus. This called for care and skill because it was quite easy to enter the trachea if the procedure were conducted without care. Eldridge watched over this stage with all the native skill for which he was known, and in the end students learned the "feel" of the catheter as it was inserted.

Dr. Matters was slowly establishing his practice in the teeth of entrenched and outspoken opposition and was therefore able to work part-time as my Demonstrator. Without the help of these two the mammalian class could not have been established let alone managed on a large scale. For one thing, we three, with Eldridge anaesthetising, had to find out the best surgical procedure for each of the many classical experiments which we taught the class. It was therefore not surprising that we almost abandoned the attempt owing to repeated failures.

In the first place, we were using only extemporised apparatus. Robertson had not equipped the department adequately and there was little available for such an ambitious attempt. Eldridge manfully pieced things together and with the University carpenter, Mr. Rieger, he constructed heated operating tables, operating lamps, venous and arterial cannulae manometers etc. whilst more sophisticated gear was being constructed to my design and Eldridge's supervision in the Physics Department workshop. In this I must pay tribute to Physics Professor Kerr Grant, (later Sir Kerr). Without the generous assistance of that gentle and able physicist and philosopher I could never have established my Department. At times, I had many valuable instruments of his entrusted to my care. I confess to "standover" tactics without which we could not have succeeded. After all, we were building something which had never been done before, and doing this whilst teaching the practice of complex animal surgery at the same time.

We have witnessed, during my life time, the steady erosion 8
of the teaching function in favour of research in the departments of medical
schools. ~~Robertson was merely a forerunner.~~ True, I had been responsible
for the introduction of clinical biochemistry at Otago Medical School, but
I did the bedside work entailed and taught it in relation to pathology. This
is my reason for opposing the appointment of non medical men to Professorial
teaching posts in Medical Schools. It is really a question of priorities.
Teaching is the main function of a medical school, and research, which calls for
expenditure on technicians and apparatus must come second or even third where
patient care is involved. Our fourth year intake of students marked a turning
point in our fortune. We had at last equipped the laboratory with good
apparatus - all designed by me and constructed in the University Physics
workshop, and we were blessed with the finest group of students by any measure.
The zest, intelligence and dedication of this group made it forever obvious
that the more exacting the task, provided that it manifestly was an approach
to the principles of medical practice, the more they liked it and the better
were their results. At the beginning of the year, I read the Act of Parliament
covering animal experiment. I recounted my experience in the U.K.. I made
it clear that they would wear clean white operating gowns, that I would tolerate
no light hearted approach to operations upon a living animal. It was a great
privilege, and that I would eject from the class room anyone not paying the
closest attention to his particular task. Anyone "standing by" for any
reason would leave the lab and wait outside, etc.. The scheme worked
perfectly. I called on Sir Charles Sherrington the year following, having
worked my passage as a Ship Surgeon, and showed him a photo of the class in
action. He asked for a copy and said "You have carried my mammalian teaching
much farther than I could possibly do here in Oxford. If you will write a
practical manual, and you ought to, I will see it through the Oxford Press."
He was most interested. I had a photograph sent with the signatures of all

the students of that splendid class.

* Photo.

There was a sequel to this account. In Pick's pharmacology lab in Vienna, the head technician, a Czech ex-artillery man, would allow no one to watch him preparing a dog for some highly sophisticated experiment. He started at 6.00 a.m. and at 8.00 a.m. the doctor for whom the animal had been prepared, came, injected whatever was to be studied, recorded the protocols and left Kwapil to fix the kymograph records. I showed Kwapil my photographs. "Who are these doctors?" "They are not doctors Herr Kwapil, they are my medical students". "You mean that students do this kind of work? Ordinary students?" It was enough. Kwapil honoured me by concluding "Come to my room anytime you like when I am making a preparation". The news travelled far and fast, even as far as Rothbergers Experimental Pathology Institute, that Kwapil had lowered the barriers to a visiting Englander!

Fürth

I made a number of good friends, some famous, in Vienna. Von Fürth (the adrenalin biochemist ^{who discovered Adrenalin}), Pick and Horst Freyer (Pharmacologists) Frölich (Frölich's syndrome - Physician), Rothberger, (Pathologist), Durig, (Physiologist), and Loewi (Physiologist - acetyl choline) as well as others such as von Brucke, von Hueber and Heller, among the younger ones. Their story is in most cases tragedy. One, I was able to rescue as my subsequent Lecturer and finally Reader - Franz Lippay, a brilliant physiologist, and a most highly decorated unter-offizier. Courage was his middle name and Hofrat Durig, his chief, connived with me ^{to bring him to Australia (1938)}. It was a poverty stricken Vienna that enabled me to live there, but the cultural atmosphere, or the indomitable spirit of those colleagues of mine, made my personal sacrifice seem small and the reward immeasurable.

Dr. John O'Connor (who followed Matters) and I began work upon a practical manual ^{of Mammalian Physiology} when he won a Beit Memorial Medical Research Fellowship and

Verney.

went to Cambridge to work with [^] He later became Prof. at Leeds. Then came World War II and the book was never completed.

On the research side, trouble began with the damage caused by rough

dockside handling of my ultra-violet spectromoter. This instrument had been presented to me by Mr. Mark Mitchell, as a wedding present! Before leaving England I had learned with Sir Charles Martin of the Lister Institute, that Steenbock's irradiation of cholesterol had conferred antirachitic properties. From the facts I concluded that the effect was due to some impurity ^{of the cholesterol,} and hoped to find out by ultra-violet absorption spectroscopy. Now, owing to this unfortunate accident, I had to re-calibrate my wave length scale. This was a most tedious task and a year passed before I could use the instrument. But I was fortunate in having some pure cholesterol prepared by Brailsford Robertson. When the absorption spectrum of pure cholesterol was compared with that of the ordinary commercial product it was at once obvious that, when irradiated, the latter gave a different set of absorption bands. I reported this at a meeting of the Medical Sciences Club, to some dozen members. Within a fortnight, the report of Hess and Steenbock's further experiment proved that my guess was correct ^{and} that the damaged spectroscope had cost me "priority". Curiously enough that did not depress me. After all, I had been proved right! Oddly enough a physicist at Heidelberg had reported the same result to a local physical society at the same date.

On my arrival ^{in Adelaide, had} Wood-Jones brought to me a letter from Francis G. Benedict, Director of the Carnegie Institute of Boston, Mass. ^{USA,} Benedict asked Wood-Jones if he would be prepared to undertake Basal Metabolism studies on the Australian Aboriginal as part of a world wide study sponsored by the Institute. He would supply apparatus etc. for the purpose. Already studies on Chinese in Shanghai had revealed a 10% lower metabolism than European subjects and Benedict himself, repeating the work on Chinese students at Mt. Holyoak College, confirmed the results even though these students had for years been living on the ordinary U.S.A. diet. "This is your cup of tea" said Wood-Jones, "You reply to this letter." Enquiry soon revealed that the nearest Aborigines were 400 miles away living on a Mission Station. In fact, to reach truly nomadic tribes would require finance beyond my reach. As it was, I explained to Benedict, I ^{would be} unable to do anything, but that ^{on my own,} I would make the effort if opportunity offered. He sent the portable metabolimeter he had designed for the purpose, together with

chemicals and record books. Within a year, the Rockefeller Foundation, acting on Wood-Jones' request, provided funds for anthropological research. They had offered to establish a University Chair, but all of Wood-Jones' efforts failed to move the Vice Chancellor, Prof. Mitchell, who could not accept the responsibility of ^{continuing} ~~carrying~~ the Chair after the six years' starter provided by the Foundation. As a result of this folly, Wood-Jones resigned and went to Melbourne for a year or so and then to Honolulu, ⁶⁹ But his efforts did produce the support I have mentioned and I was to benefit therefrom. I made a preliminary test of the metabolimeter on natives at the Koonibba Mission on the west coast of South Australia. The natives were an unprepossessing lot - venal, lazy and dirty. The worst example of the impact of western civilisation. When the Anthropological Committee was formed and an expedition ^{to Cockatoo Creek}, some 250 - 300 miles north west of Alice Springs, was planned, I was allowed to join them bringing Dr. Matters and Ernest Eldridge. After my Koonibba experience, I was more than sceptical. Our choice of Cockatoo Creek was based on the report of an experienced bush-man named Fred Colson, and an itinerant Lutheran Missionary, Herr Kramer who, during the preceding six months, had reconnoitred the region and learned by sign language and interpreter that a ritual gathering would take place there in August.

It is appropriate to record the names of the anthropological group - all amateurs. Dr. T.D. Campbell, Director of the Dental School was the only one who, from close association with Wood-Jones had made some dental studies on Aborigines living at the local reserve. Prof. Cleland ^{assisted} by Prof. Johnston (pathologist and naturalist) ~~and~~ a biologist, ~~respectively~~, Dr. Kenneth Fry, a medical practitioner and Rhodes Scholar, Dr. Pulleine a very cultured ophthalmologist and Prof. ^{Wilkinson} ~~Williamson~~ (anatomist) and ^{also a Mr.} P. Stocker of Sydney an amateur cinematographer who defrayed his own expenses. It was this group that founded the study of the Aborigines in Central Australia. My team, of course, had the arduous task of making precise measurements of oxygen uptake at 4°C below zero at 5.30 a.m. followed by double checking the metabolimeters, gravely affected by temperatures, and calculating the results. Then our subjects had to be weighed, blood pressure, pulse rates etc. taken, ^{and} in fact we were occupied for some 12 hours daily.

The remarkable feature of our study was the co-operation of the natives. They had "confronted" us (the men) in full war paint, i.e. greased and blackened with charcoal and armed with spears. Just how our interpreter and Herr Kramer persuaded them to co-operate is a mystery to me. The men were told that our ministrations would strengthen their "wind". We lay down before them as they sat on their haunches and demonstrated on ourselves what we would do to them in the early morning. They gave no sign one way or the other. Yet, to our surprise, lying there in the cold, quite naked, they just behaved like perfect experimental animals. The usual calculations based on height and body weight showed no departure from normal (European) data. But there was a new problem. They were not lying in a warm bed! Why didn't they consequently use more oxygen? Subsequent expeditions, twice with the anthropologists and four more on our own, aimed to measure Respiratory Quotient, Specific Dynamic Effect, actual surface area by direct measurement and skin and body temperatures. From that point it became obvious that much more delicate skin temperature measurements were needed. Sir C.J. Martin's flat bulb thermometers were too slow and inaccurate, and Benedict's copper constantan couples were not much better because they remove heat from skin. After designing a very minute sensitive couple - constantan ^{and} stainless steel, 0.5mm square, we mapped the body skin temperature and also measured the blood flow through the radial artery at the wrist by means of a Sahli volume bolometer especially constructed to my specification for portability made by Jacques of Basel whom I visited. The results showed that the natives conserved body heat by vasoconstricting their skin vessels to reduce radiation. They were able, at this range of ambient temperature, to control heat loss rather than increase heat production, ^{in order} to maintain normal body temperature.]

As a result of these expeditions a certain serendipity led to the unravelling of a century old problem, i.e. the nature of the toxic substance in *Duboisia Hopwoodii* or "Poison Bush". It was the chance comment of the natives as they watched me drying some of the bush before a fire. They said that they chewed this when they could get no native tobacco and they also mixed the *Duboisia* leaves with tobacco leaves when *Nicotiana* was scarce. From being in the first place interested in the highly toxic effect of this *Duboisia* upon camels, I was led to an investigation in which Pregl's new

microchemical procedures proved their value. I had brought to Adelaide Pregl's apparatus and micro-balance. I had learned of this procedure in Graz and Zurich and it enabled me to discover and to identify a new alkaloid twice as toxic as nicotine, namely dextro-nor-nicotine. For almost a century the true nature of this alkaloid had evaded the efforts of scientists in Britain, France and Australia. The pharmacology, chemistry, toxicology etc. were reported in the Aust. Jour. Exp. Biol. & Med. Sci, the Berichte der Chem. Ges. and the Archive der Pharmacodynamie et de Therapie. Dextro-nor-nicotine led to a fruitful follow up of physico-chemical, and infra-red spectroscopic studies. The latter were made by Raman technique, the only work of its kind in Australia. Toxicology of the isomers of nicotine and nornicotine was also studied until war broke out, ^{in 1939.} It also opened a new line of neuro-physiological research ^{initiated} ~~opened up~~ by David Kerr, *one of my students.*

^{had} I returned from Europe in 1935 with the newly synthesised Prostigmine provided by Hoffman la Roche. As luck would have it, a severe case of Myasthenia Gravis was in the Royal Adelaide Hospital and I was able to repeat Dr. Mary Walker's results using this new and safer preparation. I then decided to measure the cholinesterase activity of the blood of this patient during an attack and during quiescent periods. The results gave support to the view that excessive cholinesterase reaction was involved and that therefore acetylcholine played the role of a transmitter of the nervous impulse in somatic nerves. More importantly, it revealed what I had already concluded, that Margaret McKay, a student with a poor academic record, was a capable and trustworthy investigator. I had purposely set her to work with me in this investigation. Later she obtained her Ph.D. at Cambridge and became a valued member of the staff at the Lister Institute. Dr. Drury went out of his way to thank me for Margaret McKay! This case led to other investigations of cholinesterase in cases of Myotonia, and of stringhalt in horses, all with confirmatory results. *(Gloverston & M. Draper.)*

Dr. John

It is important to recall the fact that without a trained technician, with only a part-time demonstrator and with no secretary, no reserve of postgraduate students, it was not possible to pursue a charted course of research. Thus I made the best use of any opportunity for investigation that offered. I used the U-V spectroscope as Baly had done, seeking for some

evidence of a relationship between molecular structure and physiological action. This I could pursue alone, or as it happened, with a Cambridge colleague H.F. Holden of the Walter and Eliza Hall Institute, Melbourne. At the same time I collaborated with Brailsford Robertson whose interest in nucleic acid coincided with my own. Work done in Cambridge on rats with induced polyuria taking advantage of an experimental study by Needham, led me to encourage a science student and two medical students to take up the subject of water balance and kidney function. This was done under my personal guidance during the thirties for the degrees of M.Sc. and M.D. Dr. W.J. O'Connor became an authority on this in U.K..

Having attended a German Medical Congress at Wiesbaden, I was fortuitously able to recognise ^a similar case of Pituitary Cachexia in the care of Drs. Frank Hone and E. Britten Jones and from that, to successful therapy. The Addison's Disease study was a consequence of this. *I was invited to address the Royal Soc. Medicine in London on this work.*

This opportunity to study oxygen transport in an adult with congenital circulatory defect was followed up vigorously after practising radial arterial puncture on ourselves. I was stimulated to do this because I had seen similar work done in Vienna. In a like manner, having seen the Ascheim Zondek test done in Berlin by Zondek, I introduced it here and R.F. Matters and I followed this up with experimental studies which formed the basis for a thesis for M.D. by Matters and the ~~re~~ establishment of pregnancy tests in Adelaide, ^{but} not without opposition.

With the advent of Donald Sinclair, a physical chemist, we successfully erected a prototype of artificial kidney to continue the renal and water balance studies when the outbreak of war interrupted the work.

From a survey of work conducted during those years, two points deserve emphasis. Opportunities had to be siezed as they arose and refresher visits to Europe were necessary to restore self-confidence owing to isolation in time and space. It would have been easy to allow oneself to yield to apathy. My overseas refreshers were made at the cost of three fourths of my annual salary, and of much hard work as a ship's surgeon ^{where} of whose services *by the availability of* a free dispensary *converted* into a form of clinical slavery. Moreover, even

though life in Vienna was relatively cheap for an Australian, I had neither the cash nor the time to take advantage of it. Clinic and laboratory occupied my time from 7.00 a.m. to 7.30 p.m.. In fact, I was so inured to long working hours in my Adelaide Department that it was natural to continue ^{thus} in Vienna. Incidentally, I gave three university lectures in Vienna, two in Basel, one each in Zurich and Heidelberg in the German language, and by invitation also a scientific broadcast about my aboriginal experiments over the Austrian Broadcasting station "Urania".

During those years there was no change in staff or routine within the Medical School; Brailsford Robertson and I ^{had been} ~~were~~ enough to assimilate! The Medical Faculty was dominated by Honorary Surgeons and Physicians, excellent practitioners who ^{had been} ~~were~~ in practice before World War 1 and the same ^{being} domination was bequeathed to their sons. Looking back, it seems obvious to me that "Medical education" has over-compensated in the other direction. Clinical observation has been sacrificed to laboratory tests and to over-specialisation.

I have lived through ^{the} a strange period of medical education in which the pre-medical studies have been revolutionised, but not assimilated by medical faculties labouring under the illusion of "academic freedom." The subjects of Physiology, Biochemistry, Bacteriology, Pathology, and even Anatomy should form, so far as the medical student is concerned, an integrated whole taught by medically qualified Professors. I say Professors because I believe most firmly that such students should be taught by the most experienced and qualified teachers and that appointments should emphasise this teaching qualification and responsibility. Those who wish to study these subjects as part of a science curriculum should be taught by the first assistant to the Professor.

in handover, 16.

When I delivered the CIBA lecture for 1950, Lord Horder, the Chairman of the Foundation introduced me and said that the Adelaide Dept. of Physiology as a teaching department was the finest he had seen during his travels about the world. This opinion was ~~expressed~~ ^{confirmed} also by Prof. Pickering who expressed astonishment that medical students could be capable of performing such delicate operations on the living animal. Prof. McLean of St. Mary's could not believe, except that he saw it with his own eyes, that medical students could perform metabolism experiments without breaking the gas analysis apparatus! Prof. Best (Toronto) expressed similar opinions. Best also discovered that I encouraged my students to write questions and place them on my lecture bench for answering before the class. He said that he couldn't possibly do that! I said that the effect was twofold. I learned more about my subject and the students learned to ask the right questions. Moreover, I pointed out, the students learned to treat me as I had told them, namely, as an older, perhaps wiser, fellow student, not as an authority who knew everything.

Until after the 2nd World War, there were the following professorial chairs: Anatomy, Physiology, Biochemistry and Pathology. The other basic medical subjects - Bacteriology was taught by the Hospital Clinical Laboratory under Dr. L. Bull, a veterinary surgeon. All the clinical subjects were taught by the Honorary Medical and Surgical staff. The Faculty meetings therefore were gatherings of the leading medical fraternity, and of necessity, extremely variable as to composition because of irregular attendance. The Dean was always one of these part-time teachers. In this way, the "medical families" (for their sons always studied medicine) maintained a monopoly of the practice of specialities and of Hospital appointments. Because of this, pre-medical training was downgraded. I was quite used to this during my own medical training in Dunedin, and it was just the same in England, Holland, Switzerland and Austria, excepting that on the continent, Professors and Extraordinary Professors brought the clinical teaching into the University sphere. This I discovered to be the case in U.S.A. after the establishment of Johns Hopkins, which set the pattern for the A class schools at a time when medical schools varied from excellent to very bad.

At the time of my appointment I found that the Medical School was still suffering from the traumatic effects of a "strike" by the Honorary staff of the hospital. The full story is unknown to me, but through their refusal to attend the hospital, medical students were forced to proceed to Sydney to complete their clinical training, and the Government were forced to import medical practitioners from U.K. to attend the patients.

This manifestly led to a rift in relations between the imported medicos and the Adelaide medical establishment. Even today, I am informed, traces are still felt of this influence in the clinical sphere.

My practical class aimed at teaching disciplined team work, respect for living tissues, reasoned deduction from observation, and careful record-keeping. Again it is obvious that my background played its part. I designed apparatus including the kymographs, and supervised ^{their} construction in the Physics workshop. I costed the work and proved to a sceptical Finance Committee that the cost was some 30% less than the imported equivalent, (if obtainable) ^{n appropriate} and thus secured a raise of pay for my man in the workshop. All this seems like the do-it-yourself science of the turn of the century, and in fact, it was ^{at} ~~I have no doubt.~~

In all, some forty papers were published over the ~~whole~~ ^{to 1939} period, several of them being quite significant, e.g. Vitamin D, cholinesterase and peripheral nerve transmission. Blood chemistry in Ductus Arteriosus defect. Climatic adaptation in the Centralian Aborigine. Identification and synthesis of nor-nicotine in Duboisia Hopwoodii and its toxicology and molecular structure. Ultra violet absorption spectral studies of molecular structure using the earliest and most laborious techniques and even Raman infra-red spectroscopy, possibly the only work of its kind in Australia. Some pioneering work on adrenal cortical ^{the} hormones and the pituitary created interest in U.K..

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All this and more was done at a time when there were no research funds,
it was only
and when they did appear post war, /in a meagre way.

Just prior to the war Dr. T. McLarty, assisted by Dr. Robt. Condon, using a technique devised by me, produced some beautiful work on intraocular pressure. The thesis, which is in the library was never published however.

At the outbreak of hostilities I sought permission to enlist. This was granted me by the then Vice-Chancellor Sir Wm. Mitchell, who remarked that it would set a good example, so I put my name down for the A.I.F. However, when the army began to have feeding problems and it was discovered that they had a tame professor of human physiology and pharmacology in their lower echelons, I was quickly seconded as an Inspector of Catering.

As an Infantry Lieutenant, A.I.F., I was instructed to examine troop feeding and established the fact that cooking and food preparation destroyed 50% of protective factors leading to rejection of food by troops and gross wastage of 40% of foodstuffs supplied. Scientific reforms were proposed and I was sent to Army H.Q. to formulate an organisation to implement these proposals. The result was that I reorganised Army feeding, established training schools, designed special ration packs for jungle warfare, and steam cookers for conservation of food values and rapid feeding of troops on the move. The whole resulted in ^{the} organising ^{of} a Catering Corps which is now an integral part of Army logistics in its own right. An example of human nutritional research and its application on a vast scale.

³⁰⁰⁰ ¹⁰⁴ As a "by product" of this came the opportunity to examine the secret records of daily food intake of the Australian 2/21 Battalion during 3. ^{1/2} years captivity on Hainan Island. Never before had such detailed records of almost 3,000 men been kept for so long. One account was kept as a basis for argument with the Japanese, the secret copy written in microscopic script on tissue paper was kept safely until the Armistice and placed at my disposal. I had the records enlarged under U.V. light and using accurate analytical data of the Japanese food supplies made by a captive British officer in Japan, I was able to establish (1) that based on Japanese body weight the food was inadequate for the larger Australians who slowly suffered under-nutrition, (2) the ascorbic acid supply was adequate, (3) owing to infection and low intake Thiamine was inadequate

but intestinal flora, except in cases of enteritis etc., prevented general Beri Beri by thiamine production. This is published in World Review of Nutrition.

The ~~proposal~~ ^{proposal} to use anti oxidants in army rations led me to advise long term animal studies, the first of their kind. A team was recruited and a special section equipped in the physiology department. ^{This was perhaps the first time that slow changes were} *
Owing to an attempt to have funds for this work diverted to another defence department, this research was transferred to an Army dehydration plant at Scottsdale in Tasmania. The team working on this research comprised ^{Darwick Brown,} Drs. A.R. Johnson, F.R. Hewgill, with assistant M. O'Halloran and i/c of animal diets and the recording of results, Alan McNeil.

After the peace was declared and the forces were demobilised, it was the initiative of the Commonwealth Government to institute a Rehabilitation program whereby ex-servicemen and women with appropriate educational qualifications, could proceed with finance assistance through tertiary education. This led to a significant rise in the number of medical students, and departmental staff had to be increased to cope with the numbers. These returned men and women became excellent students, and their greater maturity benefitted the younger ones coming straight from school. The greater numbers also necessitated increased accommodation, and a new medical school was built in Frome Road, adjacent to the dental school and the Institute of Med. and Vet. Sciences, all forming one complex with the Adelaide Hospital. The move took place in Nov. 1949.

This was the Indian Summer of a compact family relationship between members working in a small department which was destined to change under the impact of large Government grants and Government control of expenditure. No change is ever completely for the good, and the excessive importance placed upon research (publish or perish) is to my mind lowering the academic value both of research and teaching.

The staff, including technicians, all took tea breaks and lunch together around a big table in the common room. These periods were valuable as discussion groups, and the talk could be on a broad scale or concentrating on a particular research problem, when ideas would be thrown into the pool. The fundamental point being that there was no distinction between technicians and research workers.

Contemporaneous with the development of high speed high altitude aircraft was the Being in a position to take advantage, I had the low pressure chamber transferred need for aero-medical research.) from R.A.A.F. at Victor Harbour to the Medical school, and set up an aviation

medical research unit in the physiology department, and for many years was Chairman of the Aviation Medical Research Centre in Melbourne. The chamber and operating plant cost £50,000 in 1930 money. Dr. Hugh Le Messurier/headed this unit and engaged in studying the quantitative effect of supersonics on the auditory nerve and area, and the neuromuscular transmission in anoxic states. Pre-war he had worked with Able at Baltimore.

In 1948 Dr. W.A. Dibden, with Drs. M.H. Draper and R.S. Edgar investigated successfully into the cause and cure of certain war neuroses formerly grouped as "shell shock", and was able to apply the principles of Pavlov's conditioned reflex and to show the part played in the syndrome by altered Hering Breuer reflexes. Dr. Dibden is now the Director of Mental Health.

Drs. A.J. Day and E.R. Threthewie of the Med. and Vet. Institute, collaborated in work on snake venoms and the influence of Heparin and the immunological response. Dr. Day continued his work on atherosclerosis and cholesterol content, with Dr. Colin Schwartz and G.K. Wilkinson assisting.

Drs. John Covernton and M. Draper (1949) did a study of myotonia, and Dr. Alan Kerr-Grant took up renal research where Hicks and O'Connor put it down prior to the war. O'Connor had proceeded to Cambridge as Beit Memorial Fellow, and became Prof. of the war. Dr. Covernton wrote a thesis for M.D. on the study of a family of Thomsen's Disease.

Dr. Neil Crosby (1951) a busy medical practitioner in a large country centre took a year off to pursue the problem of nocturnal enuresis, and succeeded in curing this by means of an electric stimulus which was patented by the University. He also assisted Dr. D.I.B. Kerr in comparing the effects of nicotine alkaloids upon respiration, and was appointed Sen. Lecturer in clinical physiology.

Dr. D.I.B. Kerr devised an electronic method of absorption spectrometry to trace the cholinesterase hydrolysis down to its actual neuro-muscular transmission velocity. He was invited by H.W. Magoun to Los Angeles where a team were working along similar lines, but he returned to Adelaide as Reader in Physiology.

Dr. Donald Cheek (1950) then a house physician at the Children's hospital was concerned about the cases of Pink Disease, and brought his problems to the department. In discussions on the aetiology it was suggested that there was a salt/water imbalance. Following this line, successful cures were made corroborating Selye's 'stress syndrome'. Cheek is now Prof. of paediatrics at St. Louis, USA.

Drs. Threthewie, Carman and Day also investigated Thrombosis and the anticoagulant effect of extraction from perfused mammalian liver. (1950).

Dr. Day became Professor of Physiology at Melbourne, and Dr. M. Draper

went to Edinburgh as Professor, but is now with the British Council.

Robert Porter who worked with Dr. Ross Adey on the anatomy and physiology of the nervous system took his Ph.D at Oxford, and is now Prof of Physiology at Monash University
Dr. Colin Schwartz who worked with Dr. A. Day is now Prof. of Pathology in London, Ontario.

Drs. D'Arcy Sutherland (surgeon) and J.H. Stace (anaesthetist) worked on dogs in preparation for modern surgical techniques in heart/lung surgery. Their success is now well known.

Stuart Butler who took an M.Sc. in physiology became subsequently Prof. of Physics in Sydney.

Barbara Dennis who came onto the staff as demonstrator took a Ph. D., worked in U.S.A. for a period but returned to the department as a Lecturer.

D'Arcy Webling who was a demonstrator in 1950-51 went to Switzerland to do further research and finished as a Prof. physiology in London, Ontario.

Tim Quinlan-Watson who helped in the research into kidney function for his thesis for M.Sc. is now Director of the Agricultural Research Station at Kybebolite.

ProfesDr. Ivan de la Lande joined the staff as Reader in Pharmacology when that discipline was raised to a full Chair in 1958.

The move into the new Medical School in 1949 seemed to be the starting point of a more integrated school, and with the establishment of study leave periods for staff to acquire overseas experience and the chance of comparison of standards, with the faster international communication, by the time I retired in 1956, medical teaching and practise was only a matter of years behind, not decades behind more advanced schools. In apology for this statement, it must be remembered that Australia is still a young nation with a limited population, and public finance must be spent on a very broad programme of national development. General education gets its share and medical education is just a small part of this. We have also been able for some years to import as migrants doctors already trained overseas, especially the U.K.

It is only now in 1975, with great birth pangs, that an integrated medical school and hospital is being built at Flinders University - just 12 miles south of Adelaide, which, together with other regional hospitals, will help relieve the pressure on the Royal Adelaide Hospital, and strengthen the general practise of medicine.