

SOME LESSONS LEARNT IN FIFTY YEARS' PRACTICE IN VENEREOLOGY*†

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In preparing this lecture I had great difficulty in deciding what could most profitably be included and how to present it. I decided that, to some extent at least, it must be historical and autobiographical, though I recognize that at my time of life one runs a great risk of becoming boringly reminiscent.

My first active interest in V.D. began approximately 50 years ago when I had charge of a cantonment hospital in an Indian military station. There I considered it part of my duty to prevent V.D. being conveyed to the troops by the ladies of the oldest profession in the sadar bazar. I found that the time-honoured examination practised in regulationist countries was useless owing to the difficulty of establishing the fact of non-contagiousness and the impossibility of detecting venereal diseases in their incubation stages. Fortunately, the ladies all lived in one quarter, a row of little houses, so, with the help of funds contributed by military units in the cantonment, I took one of the houses and in it instituted daily disinfection of the ladies by the matron of the cantonment hospital, and thereby abolished venereal infection by inmates of the bazar's brothels. I may add that I was warned by the senior medical officer of the station that I was imperilling my commission and that he would not protect me if any feminist organization started a row at army headquarters. Instead of being "broke", however, my tenure of the appointment was prolonged by Lord Kitchener, who was a decided realist in these matters, when he learnt from the P.M.O. why there was no V.D. case in the station hospital at the time of his inspection. After all, I could not see how anyone could object to the daily disinfection. It was carried out by a female nurse and we did not advertise the fact that it was

being done; furthermore, I would not allow the police to interfere.

I had nothing to do with the management of V.D. in the troops until 1909 when I was posted to the Military Hospital, Rochester Row. That was a black day in my life. I had hoped to go to the R.A.M. College, to teach bacteriology, but the pathologist at the Rochester Row hospital (incidentally a specialist in hygiene) had most inconsiderately thrown an epileptic fit in public and I was the only labelled military bacteriologist available to relieve him.

At that time the late Col. F. J. Lambkin was in charge of the Rochester Row hospital. It had been a hospital of one of the Guards regiments, and Lambkin had persuaded the War Office to have it converted into a hospital for research and instruction in V.D., a complement, in fact, to the Queen Alexandra's Hospital, Millbank, in the Royal Army Medical College. I am glad to have this opportunity of testifying again to the debt which venereology in this country owes to Lambkin for his enterprise in systematizing the treatment of syphilis in the army and in the establishment of this teaching centre, almost on the eve of the revolution in the management of syphilis of which we are seeing the fruits today.

The hospital was well placed for taking advantage of new discoveries in diagnosis and treatment in this field, and between 1909 and 1920 it became an authority for the guidance of civilian as well as army venereology.

Col. Lambkin left the hospital a few months after I was posted there and was succeeded by an ophthalmologist with no special experience of V.D., but there was no check to the impetus in teaching which had been started by Lambkin.

At the time I joined, no Wassermann test had been done in the hospital and there had been no demon-

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stration of *T. pallidum* by the dark-ground method. The only literature in English on the Wassermann test was contained in articles by Browning and Mackenzie and by Henderson Smith and Candler in this country and by Noguchi in the U.S.A. None of the published methods, either in English or German, suited my circumstances, because I was not then allowed to kill a guinea-pig and had to rely for complement on the blood I could get from its ear, which was about enough for one test by any of the described methods. So, on the principle that what mattered was not the absolute quantities of the reagents but the proportions which these bore to the total, I worked out a method in which the reagents were measured in a capillary pipette and the proportions were made by appropriately varying their dilutions. The volume was empirical, about 100 ml., and at first I used one pipette throughout any series of tests. That was a source of anxiety, because if the pipette was broken during the testing of a batch of sera, I had to start all over again. Accordingly I had to learn the art of making capillary pipettes which would deliver the same volume; in this I found useful guidance in Almroth Wright's book "The Technique of the Teat and the Capillary Glass Tube". That work, like others by Almroth Wright, taught me much that I have found invaluable; I would always recommend any medical worker whether in venereology or otherwise to practise glass-blowing until he can make capillary pipettes of whatever capacity he requires, and I do not believe that one should be in the hands of technicians for such work. Further, I believe that one should practise and practise until with a teat he can draw a liquid up to a given mark on a capillary pipette and hold it there quite steadily. The apparatus for dark-ground microscopy at the Rochester Row hospital was quite unsuitable for this work and there was no literature on the subject, so I had to worry out the principles in various books on microscopy to discover what was wrong with the kit which had been bought by War Office instruction. In all this I learnt much about the working of the microscope that I had never bothered about before, and I would always recommend anyone practising venereology to learn how to use the microscope to the best advantage. Again I would say: "Do not lean on the technicians".

When we began to demonstrate *T. pallidum* by the dark-ground method, word of it soon got around, and for many months it was a common experience to receive a message from the C.O.'s office to the effect that this, that, or the other V.I.P. was on his way up for me to show him the germ of syphilis. So I kept specimens in capillary tubes and could

usually have the beast in the middle of the microscope field by the time the visitor had climbed to the top floor. The storage-in-capillary-tubes method was much later proved to be quite practicable for transmitting serum through the post for dark-ground examination.

The treatment of syphilis in the army at that time was by injections of mercurial cream in courses of varying length throughout a period of not less than 2 years. It was common to see clinical relapses in men during the time they were receiving these injections, and it was interesting to note that the character of the relapses then depended very much more on the age of the infection than we later found in relapses after arsphenamine treatment. I judged that this was because the mercurial treatment did not put a dead stop to the activity of the spirochaete but left the infection simmering so that the disease was constantly progressing towards the stage of tertiary manifestations. This idea was supported by the fact that I found in men who had undergone the routine 2 years' treatment by mercury that 42 per cent. were positive to the Wassermann test by the original method and 75 per cent. to a modification which, although more sensitive, was still almost as specific. Judging by an analysis of 378 cases which I made at a later period, under regular courses of mercury injections, 83 per cent. of the cases relapsed at least once during the first year of treatment, and the total time lost by a soldier whether in hospital or in attending for injections in the first year was just under 84 days.

In contrast to the relapses under mercury treatment, those under arsphenamine treatment occurred as if the arsenical remedy had stopped the clock, so to speak, so that even many months after the last treatment, a relapse would very often be of the same character as the condition when the patient first came under treatment. In a paper written on this subject in 1911 I recorded some interesting repetitions, such as the same type of rash in eight out of eleven relapses from 3 to 12 months after treatment, and in one patient who relapsed twice, at 3 and at 6 months, the same roseola, sore throat, and synovitis of one elbow joint.

In the treatment of gonorrhoea, the hospital had just advanced from the astringent lead acetate, silver nitrate, or other devilish lotion given with a small hand syringe to the Janet form of irrigation with potassium permanganate, but the strength then was about twice what we later thought advisable when we became convinced that the essentials were encouragement of drainage and the acquisition of resistance. In this I think we owe much to the teaching of Almroth Wright, and I am fond of

quoting a dictum of his that the Ministry of Health should issue an edict forbidding the use of anti-septics. I don't suppose he meant that to be taken literally; probably he was taking a leaf out of the book of his friend, Bernard Shaw, in making a rather extravagant statement to make people sit up and take notice, but throughout my connexion with venereology I have found it paid never to forget Almroth Wright's and other savants' insistence on the importance of drainage and the antiseptic value of plasma and leucocytes.

The third venereal disease of those times, soft chancre, was then far more troublesome than it became in later years, and much of the fault lay in bad treatment, but first I must mention that when I started work at Rochester Row large numbers of syphilitic chancres were diagnosed as soft chancres and that about that time one Hunterian lecturer calculated that the ratio of soft to hard chancre was as two to one. Very quickly, after better methods of diagnosis became general, that ratio was reversed. Now, although the figure for chancroid treated in the centres in England and Wales is less than half that in 1925, the ratio of chancroid to syphilis seems to be approaching unity.

The treatment of soft chancre when I joined at Rochester Row seemed to me quite terrible. Buboec were a very common complication and I am sure that an important cause was inattention to drainage of the sore; at any rate I have seen dangerous threatenings in the groin after the use of drying powders on chancroids. The treatment of the bubo was by incision from end to end, scraping with a Volkmann spoon, and thereafter daily wiping out the cavity with a gauze swab. I had nothing to do with it at that time and all I can say about the lesson I learnt in that branch in the years 1909 to 1914 was how *not* to treat chancroid and complicating bubo. When I had the direction of that treatment I used the lesson learnt in India in the treatment of liver abscess, and relied on repeated aspiration, with greatly improved results. As in the case of liver abscess, laying the cavity open or connecting it with the outside by a drain seemed to invite secondary infection, since the opening was largely out of control, whereas repeated aspiration required only simple precautions to prevent secondary infection and yet ensure drainage and flushing of the cavity with antiseptic plasma. Anyhow, the methods of dealing with chancroid and its complications in No. 9 Stationary Hospital during the war of 1914-18 reduced to a bagatelle what would have become a serious problem if we had adopted the fierce "spring-cleaning" methods which I had witnessed at "The Row".

To revert to the other two diseases: as you know, the discovery of "606" was published in March, 1910, and the drug was tested in a number of clinics, including Rochester Row, before it was put on the market under the name of "Salvarsan" at the latter end of the same year. At Rochester Row we carried out a systematic investigation of the effects of increasing courses of salvarsan and mercury, and in our investigation we were greatly helped by a W.O. directive that the blood of any patient we had treated for syphilis should be sent to us whenever we called for it, no matter where the man was stationed; under this order I received specimens from many parts of the world. We learnt in the years 1910-14 pretty well what to expect from given amounts of treatment and that knowledge was put to good use in the war of 1914-18. The treatment given to soldiers overseas during that war mostly lasted only a month, but in that month we gave to each case 0.6 g. more Salvarsan than we had ever given before. In compressing the treatment into such a short period, I knew that if we gave the "606" in what we called full doses, 0.6 g., we should have a percentage of encephalitis which would cause alarm and despondency, but I calculated that if we gave the 2.4 g. in doses of 0.3 g. we should have very little. The event proved this to be correct as in over 70,000 cases of syphilis treated during that war, we had only eight deaths from encephalitis. The most dire late effects of syphilis were prophesied from our "war course", as it was called, but inquiries in neurological clinics and mental hospitals which were instituted by the Medical Society for the Study of Venereal Diseases and supported by the Ministry of Health in the '20s gave us some evidence that these gloomy prognostications were not borne out by the event. That evidence is supported by the Registrar-General's mortality tables relating to late effects of syphilis for 10, 20, or more years after the war of 1914-18. I have mentioned that in that war more than 70,000 soldiers were treated for syphilis, and, if our critics had been right, we could have expected by the years 1934 to 38 or so a great increase in deaths of males from tabes, G.P.I., and aortic aneurysm. Instead of this, in 1936 the standard mortality rate for G.P.I. was approximately one-third what it was in 1914, for tabes it was just over one-half, and for aneurysm about the same. These figures give me great satisfaction because I had not liked being lectured on the treatment of syphilis by foreign critics, who I knew had not investigated the effects of salvarsan treatment so systematically as we had done at Rochester Row.

I think that we can gather from our experience on the grand scale I have mentioned that a good initial

course of salvarsan, even though backed only with mercury, was a pretty good preventive of late effects of the disease. Of course, after the war of 1914-18, the treatment of syphilis in this country was lengthened considerably, but I have always thought that an improvement of results over those produced by say a 10-week course of arsphenamine and bismuth is analogous to the small increase over a ship's optimum speed in response to a disproportionately great increase in fuel feed.

That is not to say that I do not advocate aiming at 100 per cent. cures out of hand, but sometimes circumstances, such as a war, may prevent application of the ideal treatment and then one may gain some comfort from the records of results gained with less than the ideal.

In the days when arsphenamine preparations were our sheet-anchor, I was a determined opponent of the school which relied on arsenic alone or gave arsenic courses alternating with courses of heavy metal. We learnt very early at Rochester Row that reliance on the arsenic alone invited neuro-recurrences in the form of paralyses of the cranial nerves, and, seeing such complications in patients treated elsewhere with arsphenamine alone, our patients having escaped, we congratulated ourselves on having early followed the advice of the great Neisser to use the combined arsenical and heavy metal treatment.

In this also we accepted a principle enunciated by Ehrlich when he invented copper-salvarsan. In introducing this compound to his disciples, Ehrlich likened his chemotherapy to a poisoned arrow aimed at the parasite, the amino-benzene being the arrow tip and the arsenic the poison with which certain natives anointed their arrows. He said he had noted that savage tribes, the more certainly to destroy their enemies, tipped their arrows with more than one poison, and he proposed to apply this principle to his attack on *T. pallidum* by combining salvarsan with copper. That compound was found too toxic, but Ehrlich's successor, Kolle, made silver-salvarsan. Poulenc Frères made Luargol, a combination of salvarsan with silver and antimony, and Lehnhoff-Wylde in France made zinc-sulpharsenol, having found that the curative dose of an arsphenamine preparation was lower if another metal was present in the circulation when the arsenical remedy was injected. None of these compounds achieved any great popularity, but the principle of exploiting synergistic action in the defeat of the parasite of syphilis remains.

I hope that what I have said on this subject does not run counter to what your lecturer on the treatment of syphilis may tell you later, but I have

mentioned it to explain my own reluctance to rely on any one remedy for the treatment of syphilis, and it will take a lot of statistics to convince me that employment of the synergistic principle in the treatment of syphilis does not pay better than reliance on one remedy.

I should like now to tell you what I have learnt in the management of urethritis. Very soon after the introduction of "606", it seemed to me that the problem of syphilis was solved; one had only to see the effect of one injection on the spirochaetes to realize that the conquest of syphilis was just a matter of discovering how much of the remedies in our hands we must give. As I put it to my C.O., we need not worry our heads further on syphilis for years to come; all we had to do was to go on progressively increasing the treatment with the available remedies until we had the best results. The discovery of the optimum would take a number of years but the scheme of the investigation could be laid down and we must just pursue it systematically. I suggested then that we should get on with the problem of gonorrhoea, and I persuaded him to let me look after the gonorrhoea cases in the hospital, giving me an assistant for routine laboratory work and in the gonorrhoea ward.

In the period 1910-14 we tried a number of methods of treatment with mixed results. We very early reduced the strength of the routine solution of potassium permanganate to 1 in 8,000 and we dropped the pressure to a head of three feet. For a time I was attracted by the idea of killing the gonococci *in situ* by heat, knowing their sensitiveness to this agent, and we used bougies heated either by water or electricity with quite good results in a number of cases. We carried that treatment on throughout the period 1912-14 and even into the war, but eventually abandoned it as the results were not commensurate with the labour and individual attention required.

During the war of 1914-18 we noticed, that in patients who had both syphilis and gonorrhoea, the urethritis seemed to dry up rather more quickly than in patients with gonorrhoea only; we wondered whether this was due to the "606" we were giving or to the mercury and were quite ready to swallow the claims of an American worker that he could cure gonorrhoea in about 10 days with intramuscular injections of mercury succinimide. I got some of this chemical and injected it in doses of 100 mg. The results seemed at first almost miraculous and I am afraid I passed for discharge from hospital quite a large number of patients who had been there only 2 weeks. The medical officers of my hospital were so impressed that some of them vowed that at the

end of the war they would set up as specialists in V.D. and make their fortunes curing gonorrhoea in double quick time with mercury injections. Then I began to see disadvantages in mercury succinimide and thought that perhaps a good chemist could improve on it. So I got leave to come home and consult a chemist, Mr. J. E. Marsh, Lecturer in Chemistry at Oxford University. He very kindly prepared a lot of new mercury compounds which I first gave to rabbits. Some of them were fearsome preparations which knocked out the animal almost before one had got the needle out, but we tried quite a number of the less ferocious ones on human patients, with varied results. Altogether, however, when I came to review the results after treating about 7,000 cases on these lines, I concluded that, counting the time spent in hospital by relapse cases and cases whose discharge dribbled on, we had gained about nothing. The injections certainly made the discharge diminish and very often disappear so far as the outward sign of a bead or even moisture at the meatus was concerned, but when one looked closer one had to conclude that the infection was just simmering and, under application of tests of cure such as we apply in peace-time, I concluded that the mercury-treated cases actually spent longer in hospital than those treated with simple irrigations. The mercury merely suppressed the tissue reaction and, as I came to believe, interfered with the development of that resistance by the tissues on which we had to depend for the cure of gonorrhoea in pre-sulphonamide, pre-antibiotic days. Accounts which I have heard and read of the effects of cortisone in various manifestations of V.D. remind me very much of our experience with mercury compounds in gonorrhoea. I have sometimes wondered whether the action of mercury in syphilis was simply a suppression of symptoms.

So far, I have dealt with experiences in urethritis up to the end of the war of 1914-18. Before I continue with lessons learnt since that period, I should like to mention one other which I learnt very early: "Don't take 'No' for an answer". Venereology is now almost respectable, but it has been described by writers in the past as the Cinderella of all specialties and its practitioners have had to fight for a place in the sun. Before I joined the Rochester Row hospital, I would not have believed that I could ever dare to use such insubordinate language to my superior officers as I had to do to get what was necessary for the management of this group of diseases. I left the army determined that never again would I tolerate such conditions of diagnosis and treatment as I had struggled to remove whilst serving. I soon found myself expected to

manage in conditions less convenient than I considered necessary for treatment of civilians, and I asked the powers concerned to find someone else for the job. They didn't. The lesson is, make up your mind what is necessary and insist on having it, but be sure that what you ask for *is* the minimum. It means some sweating of blood but in the long run it pays so far as efficiency is concerned, though perhaps not so far as personal advancement is concerned. Just now I said that, in your demands, you should be sure that what you ask for *is* the minimum for efficiency, and here I would urge the duty of economy in prescribing. Shortly after I took up work in the Ministry of Health, I reviewed the requirements of all the V.D. clinics in England and Wales and concluded that a reorganization costing about £40,000 a year was necessary. The recommendations to this effect were received very coldly—it was the time of the Geddes axe—and I was told that no increase in expenditure on V.D. could be afforded. A feeling of acute frustration in which I seriously contemplated "throwing in my hand" engendered the thought that perhaps the money could be found by effecting economies in the details of expenditure in the various clinics, so I obtained annually an analysis of the items of expenditure in each of them. Meantime, with the valuable co-operation of the staff of my clinic at St. Thomas's Hospital, I effected some very large economies in drugs and dressings there. Thus, rubber gloves, formerly thrown away when torn or pricked, were repaired whenever practicable; for a pad of wool wrapped in gauze which was used to massage the area of an intra-muscular injection and thrown away, a glass slab was substituted; for a pot. cit. and hyoscynamus mixture costing 7d. for a week's supply, which was given to every patient with urethritis, was substituted education that drinking plenty of water was quite as good in most cases, and the patient who would not be happy without a bottle was given a very good substitute for the expensive pot. cit. mixture containing a little quassia coloured with cochineal or burnt sugar and costing ¼d. for a week's supply. By such means we reduced the cost of dressings and drugs other than arsphenamine compounds to an average of 1½d. per attendance, and I then argued that it would be reasonable to expect other clinics to keep down their costs under the same headings to an average of less than 2½d. I found in some clinics a cost of as much as 2s., and 1s. was a very common amount. Investigation revealed waste of the most idiotic kind very often quite unknown to the directors. Thus one clinic put a whole bandage over six layers of gauze and corresponding cotton wool on the

site of each venepuncture ; one had a stock pot. iod. mixture costing 1s. 10d. for a week's supply though the pot. iod. in it cost only 6d., one of the other ingredients being moreover incompatible with pot. iod. ; very many bought their distilled water in small ampoules bearing various fancy names ; and so on. By pointing out waste to directors during the decade up to 1930, when the Ministry controlled details of expenditure, we reduced the expenditure on dressings and drugs other than arspenamine compounds by more than £40,000 a year, though in the same time the attendances were approximately doubled. The principle observed in all arguments on this question was that if anything was really necessary and procurable it must be provided, but directors of clinics were begged to examine such recurring items of expenditure and ask themselves whether they could not be lessened without loss of efficiency. Today I should be tempted to ask : "Is an expensive antibiotic really necessary for every case of urethritis and every contact of such a case ? Wouldn't a few irrigations serve the same purpose in the male ? At any rate, wouldn't they be worth trying first ?"

To return to urethritis ; when, as adviser in V.D. to the Ministry of Health, I had an opportunity of seeing the work of others in this branch, I concluded that in most clinics the management of this condition was empirical, illogical, and altogether futile. Chronic urethritis, always called gonorrhoea, the common routine was prostatic massage without evidence as to infection of the prostate, passage of sounds, courses of vaccine which were usually stock, and irrigation with either potassium permanganate or mercury oxycyanide without regard to the urethral fauna and flora. I have always found it pays to discover by careful excluding tests the locality of the residuum of infection. Further, when conditions allow, a urethroscopic examination pays out of hand. It is interesting to think of the amount of sneering which has been directed against urethroscopy. I would agree that often the appearances are indeterminate, but it cannot be denied that a careful urethroscope, examining the mucous membrane millimetre by millimetre and preferably from meatus to bulb, often sees conditions which escape the eye of the in-and-out-in-a-flash performer. Also there are some conditions such as sago-grain or Waelsch urethritis which cannot be detected by any other means ; the appearances are characteristic, and it seems to me that, in the study of urethritis, cases with distinctive urethroscopic appearances should always be classed together and their other reactions such as skin allergies and serum reactions considered in a separate class.

In the examination of urethral secretions, I have learnt that it pays to examine centrifuged urine deposit and prostato-vesical secretions in the fresh state, under axial (*i.e.*, with nearly closed diaphragm) illumination. One may thus get the clue to a persistent discharge in a very severe oxaluria, and, if one finds no pus cell in the prostato-vesical secretion, it seems hardly worth while to go on to the making of films.

In dealing with dried films of urethral discharge, I have always made at least two, one to be stained by Gram's method (with decolorization by absolute alcohol, not methylated spirit) and the other to be stained by Loeffler's alkaline methylene blue, which has ripened sufficiently to become polychrome, or by Gurr's polychrome blue. I have found that a common fault in the examination of stained films is to pay too little attention to the cytology of the specimen. Almost the most glaring case of this kind I saw was not in venereology but in tropical medicine. When I became an assistant to the Professor of Military Medicine at the Queen Alexandra Hospital, Millbank, in 1908, one of the patients I took over had no fewer than six successive entries on his medical history sheet immediately before being invalided home from India which read : "malaria, no parasites found". The patient had been invalided home on account of the debility from which he was suffering, which had been attributed to malaria. A complete blood examination, part of an A to Z examination of the patient in question, showed such a degree of eosinophilia that one wondered how the succession of people who had examined that patient's blood could have missed it. A search of the man's stools very quickly revealed an ankylostomiasis. In our field it has always seemed to me profitable to note the kinds of cell, pus, epithelium, etc., and their proportions, and to try to classify those which do not appear to be typical epithelium or blood cells. Thus, I think, one will discover more trichomonad infestation in the male than if one relies on dark-ground examination. I know that the stain recommended for *T. vaginalis* is usually some modification of the Romanowsky, such as Leishman, Giemsa and so forth, but they are not really very satisfactory unless one takes great care about the pH of the secretion and the differentiation after first washing off the stain. On the other hand, by fixing the film by the classical three-times-through-the-flame method, one can slop on some polychrome blue solution, and, having washed this off after a few or many minutes (it does not matter which), one can sit down to quite an interesting search. One can learn the characteristics of *T.*

vaginalis when stained by this method by practising on some slides made from vaginal secretion known to contain many *T. vaginalis*. In spreading such films of vaginal secretion it pays to dilute the secretion and spread thinly. In such a film the pus and epithelial cells are easily recognized and are quite distinct from *T. vaginalis*, which has granular cytoplasm and a boat- or pear-shaped nucleus usually near to one end. If you look for these characteristics you soon become so familiar with the appearance of the cytoplasm that you need not see the nucleus to make the diagnosis. As for the flagella, they are no more necessary for recognition than is the flagellum of a cholera vibrio. If you are interested in staining flagella, Seller's method of staining Negri bodies is satisfactory.

Perhaps you will say that all this means a lot of time spent after clinic hours. Well, if a man wishes to become proficient in a sport, he does not mind practising for hours and hours; why should he not practise microscopy of urethral and vaginal secretions till he becomes so familiar with cytological appearances that he can spot such a body as *T. vaginalis* in a few minutes?

One other point in technique occurs to me as worth the telling. For many years, in doing lumbar punctures, I was troubled by the persistence of my needle in travelling to one side, and then the cause occurred to me. One puts in the needle with the bevel looking to one side or other of the patient, and, the bevel being chisel-shaped, the needle travels away from it. So I changed the technique by putting the needle in with the bevel looking towards the patient's head or towards his tail, and I was no longer troubled by the movement of the needle point to one side. It may be objected that the membranes should be punctured with the needle bevel looking sideways. Personally I do not think this matters with the fine needle recommended for our lumbar punctures, but if there is any doubt on this point it is quite easy to rotate the needle sufficiently when one judges one has reached the dura.

Finally, I should like to say a little about reading and writing. In my young days I was often led up the garden path by articles in medical periodicals and thus learnt to read critically. I would never swallow the conclusions of any author, however eminent, much less quote any, unless I felt they were justified by the evidence, and in weighing up statistical evidence I have been greatly helped by a study of the elements of medical statistics in Bradford Hill's book. It taught me how to work out statistical errors and the weight to be attached to them.

In writing, I have tried to keep the critic in mind and to ask myself whether the facts justify what I have said or want to say about them. And lastly about references. If the original article has not been seen, I cite the abstract in which I read the statement I have quoted. There is a great temptation, which I have felt often in my life, to trust the abstract or the *Index Medicus* for the reference. I am sure that temptation is felt by others, and this is my reason for mentioning it: I once read in each of the lists of references given by four different authors on one subject the same wrong page number. The author had been mentioned on that particular page as having read a paper on the subject in question, but no detail other than the title and a promise to publish the paper in the next number of the periodical had been given on that page.

In the early days of '606', before it was given intravenously, a lay translator for an enterprising firm of publishers quoted Wechselmann as having directed that when the solution of '606' had been made clear with alkali, sufficient white of egg should be added until an emulsion of the remedy had been formed. This was for deep subcutaneous injection. The original author's direction was to add glacial acetic acid.

I have tried to avoid an undue amount of egotism in this *pot pourri* of experiences, but some was unavoidable. I have learnt much by my (and other people's) mistakes, and if you profit in any way by my passing on the lessons, you may not have wasted your time.