

system of dispensing adequate sanatorium benefit to insured persons will be established until there is a differentiation of the function of the tuberculosis officer as the expert in the treatment of tuberculosis in all its forms from that of the medical officer of health, whose business it is to administer measures for the prevention of tuberculosis.

The great danger of bureaucratic government is that systems become stereotyped, and the danger is the more serious when sanatorium and medical benefits have to be administered, because the treatment of disease must fail to give good results as the machinery becomes obsolete and the systems and ideas antiquated. The panel system is on its trial; it must profoundly modify the relations existing between the medical man and his patients. It may tend to convert a large section of the medical profession into dispensary doctors, who may be able to earn a livelihood without ensuring to their patients satisfactory treatment. Some few may be strong enough to rise above such conditions of service, but the tendency will be to convert the medical man into a machine producing work of an inferior quality. I should fear that this must tend to degrade the medical profession as a whole. Legal work of this sort would hardly be worth paying for.

By the scheme evolved by a special committee, which gives the first place to sanatoriums as the chief channel for dispensing sanatorium benefit, failure seems inevitable on financial grounds alone, but perchance the officers of approved societies may save the Act, so far as medical benefits are concerned, from sharing in this failure. While, therefore, there may be no urgent reason for investigating the general results of the Insurance Act, it would be wise, even now, to constitute a commission of inquiry into the results of sanatorium benefit. Such an inquiry would show how many cases have sought relief, how many have been treated, by what system of treatment, and with what success.

I still think it is unwise to enter upon a policy of constructing new sanatoriums until there is evidence from the results obtained in the last year that they have yielded something approaching to "the amazing number of cures" obtained, according to Mr. Lloyd George, in German sanatoriums. At the same time the results of dispensary treatment might be investigated, and although tuberculin has not been used in any uniform way, and too often favourable cases have been transferred from a tuberculin dispensary which is not popular with the Insurance Committee to dispensaries where tuberculin as a remedy is for the most part ignored, still inquiry may show that tuberculin dispensaries have yielded some satisfactory results.

A CONTRIBUTION TO THE CRITICAL STUDY OF THERAPEUTIC STATISTICS.

BEING CRITICAL REMARKS ON THE LECTURE OF DR. BATTY
SHAW ON THE PRESENT EVIDENCE FOR AND AGAINST
THE USE OF TUBERCULIN AS A SPECIFIC CURE.

BY

HERMANN SAHLI, M.D. BERN,

PROFESSOR OF MEDICINE AND DIRECTOR OF MEDICAL STUDIES IN BERN.

In his lecture, published in the BRITISH MEDICAL JOURNAL, May 3rd, 1913, directed against tuberculin treatment, Dr. Batty Shaw starts with a comparison of the serum treatment of diphtheria, the value of which is generally recognized, with tuberculin treatment, of which, in his opinion, the value is less generally known. To me this comparison is altogether inadmissible. No well-informed adherent of tuberculin treatment sees in it anything comparable with the serum treatment of diphtheria, and I have constantly had occasion myself to utter a warning against the popular error that tuberculin treatment is equivalent to antitoxin treatment. This inapt comparison is a constant source of error and confusion and of the incorrect use of tuberculin.

It is also very surprising to me that in combating the value of tuberculin the author shows not the least chronological sense, but reverts time after time to the experiments of Koch. That Koch made a mistake in the

interpretation of the results which he then obtained is quite generally recognized by the adherents of tuberculin treatment, and no one bases himself now, as Dr. Batty Shaw appears to assume, on these far-distant experiments. On the contrary, it was the absence of any confirmation of these experiments which was the cause of the almost complete disappearance of tuberculin from medical practice in the Nineties. Dr. Batty Shaw's attack on these experiments and on Koch's teaching seems to me like tilting at windmills. The fact that in spite of Koch's failures tuberculin was not entirely set aside in the practice of scientific physicians, and that with radically altered method and essentially different theoretical conception it is coming into increasing use at the present time, is undoubtedly due to the existence in tuberculin treatment of a useful kernel, which Koch himself was unable to isolate, but whose isolation, as I have shown in my monograph on tuberculin,¹ has since been obtained by an immense outlay of clinical and experimental work. It is not admissible to ignore this work in the way that Dr. Batty Shaw does, especially after the translation of my monograph has made these researches accessible to the English reading public.

It must excite the greatest surprise when the author points in all seriousness to quack medicine, with its reports of all manner of cures and remedies, unable for a moment to stand criticism, as an argument for the assumption that the whole results of modern tuberculin treatment rest only on the confusion between *post hoc* and *propter hoc*. No branch of medical treatment deserves this comparison with quackery less than tuberculin. I contend that no section of therapeutics is better grounded in science and reason, and that this is because this method of treatment has rightly forsaken the ground of rough and deceptive statistical empiricism which, as is evident from the whole train of thought of his lecture, determines Dr. Batty Shaw's attack. By this rational basis and by the careful criticism which is demanded by each individual case, tuberculin treatment is honourably distinguished from quack medicine, as well as by the fact that its reintroduction after the *débâcle* of the Nineties was exclusively due to progress in scientific knowledge. There is therefore a grave injustice in Dr. Batty Shaw's comparison between the quack doctor's lack of criticism and the similar lack which he assumes in the tuberculin therapist, and this reply will show on which side the deficiency really lies.

The theoretical remarks of the author are concerned with things as they appear on the surface, and are very one-sided. The detailed discussion of these would only be possible on a far broader basis, and I refer the reader who wishes seriously to form his own judgement on the subject to my monograph. I will content myself here with taking up a few points which seem to me specially questionable.

In the first place, the author's argument against tuberculin treatment is quite unintelligible when he states that it is improbable that the amboceptor action can make itself felt on the bacilli and on the toxin of the tuberculous nodules, because these nodules are shut off from their surroundings. It would be indeed a boon if such an exclusion existed. There would then be no need to bother our heads any longer about the treatment of tuberculosis. That the action of tuberculin, and therefore of amboceptor, really extends into the interior of the remotest tubercles, has been clinically and anatomically established (the latter especially at the time of Koch's era of tuberculin treatment) by the clinical and anatomical proof of the so-called focal reactions. That the author shares the incorrect view that anaphylatoxin is a combination of complement, amboceptor and antigen, I will be content with just mentioning. His view that in anaphylactic phenomena, which are also at the basis of modern teaching on tuberculin action, no curative side is to be recognized, should find little approval, and is a part of a generalization hardly in unison with physiological thought. The more favourable course of relapses in pneumonia, erysipelas, etc., cannot be considered simply as evidence of immunity in the face of the frequency of such relapses; must it not rather be attributed to the curative action of anaphylaxis, or better of hypersensitiveness (allergy)? And are not the rapid and abbreviated course of revaccination in comparison with primary vaccinia (von Pirquet), and the stormy but curative course of tuberculous infection in the case of cattle previously treated by Behring's method (Römer), or of the reinfection of already tuberculous animals (Koch, Römer) incontro-

vertible examples of the curative action of allergy? This use of the anaphylactic hypersensitiveness in activating the protective mechanism of the organism is so generally recognized to-day that it would be timely if the misleading name, "anaphylaxis," which is properly a "phylaxis" and has evidently misled the author, were to be replaced by the more correct term "allergy."

When Dr. Batty Shaw sees only harmful action in these anaphylactic phenomena, and argues again from the out-of-date experiences of the first tuberculin era of Koch, we must reply that to-day we are treating on quite different principles, and therefore with an entirely different method, and I have taken much trouble in my book to afford a proof that by rational dosage the curative element may be isolated from the harmful in the allergic phenomena and used in the interests of therapy.

The significance of the modern reactionless tuberculin treatment appears to be little recognized by Dr. Batty Shaw. In the experiments instituted by him, and published by Dr. Rigg, in order to test tuberculin treatment, ten patients treated with tuberculin were set against nine treated without tuberculin. The treatment was carried through with febrile reactions contrary to our present principles. Instead of drawing the conclusion that this method of treatment as we now know was incorrect from the fact that under these circumstances the treated did worse than the untreated, which seems to us natural; and the further conclusion that such experiments are unsuited to afford grounds for any conclusions at all; the author only imposes on himself in his unfavourable conclusions a curiously disarming reserve when he says that the unfavourable effect of the treatment might also be ascribed to the confinement to bed by the febrile reactions (not to the reactions themselves?) as the controls were able to go about. Dr. Shaw, however, appears to have had a certain feeling that not merely the confinement to bed, but also the technique in these experiments might excite comment, as later on he published, through Dr. A. C. Watkin, statistics of 22 cases in which the treatment was carried through as far as possible without reactions; these statistics, however, are just as scanty and as little free from objection as the other.

With what little depth the author conceives the whole problem of tuberculin treatment, and with what little thought he criticizes the adherents of tuberculin, is apparent from the fact that he thinks it necessary to defend his negative view of the therapeutic value of tuberculin against the old argument, which is scarcely used nowadays, that tuberculin treatment is an immunization, and that from the possibility of a protective inoculation against typhoid (the author might as well have said any bacterial inoculation in general), it might be concluded that such an inoculation must also succeed in the case of tuberculin. To refute this argument, which is nowadays hardly employed, he calls attention to the essential difference in regard to immunization which exists when the individual in question is healthy and when he is already infected. These are such well-known matters that here again the author pushes an already open door. I do not think that any thoughtful adherent of tuberculin treatment can nowadays be found to contend that tuberculin produces immunity against tuberculosis, since I have shown again and again that the therapeutic action of tuberculin is fundamentally distinct from immunization, and is indeed in a certain sense the converse of it, and that there is in point of fact no actual immunity against tuberculosis, although this does not in any way minimize the value of tuberculin treatment.

I will now touch shortly upon the chief point of Dr. Batty Shaw's statement—namely, on the question whether by his miniature statistics he really refutes the value of tuberculin treatment, and the further question whether I am not right in looking to the study of suitable cases for the proof of the value of tuberculin.

In opposition to his former view, Dr. Batty Shaw now denies the conclusiveness of the older statistics, favourable to tuberculin treatment, stated some time ago by himself in agreement with Turban, Trudeau, and others. He has, as he says, become sceptical because many cases in which he would have expected to see favourable results from tuberculin nevertheless became worse after its use. In addition to this, he has radical doubts regarding the older statistics because in them the cases specially suited for

this treatment were selected, although this was indeed a necessity inherent in the nature of the case, and in the limited clinical applicability of tuberculin treatment.

In order at once to clear up these objections against the older statistics, I must take up their defence, although in the tuberculin question I have myself given up any statistical proof of this kind for reasons which I will adduce later on. The selection of cases is a *conditio sine qua non* for rational tuberculin treatment, and this condition cannot be set aside for the purpose of a paltry simplification of the statistical basis.

The recurrence of relapses after tuberculin treatment is naturally no evidence against the value of tuberculin. The opposing view of Dr. Batty Shaw is obviously based on the fact that he is completely dominated by the old view that tuberculin, when it helps, can only help by immunizing for all time. This view has, as I have said, been given up by all tuberculin experts because it has been long disproved by experience. I have, indeed, always opposed this view, and, as soon as it is surrendered, relapses no longer prove anything against the value of tuberculin. Spontaneous recovery is also very frequently followed by relapse, and in this respect nothing more can be demanded from tuberculin than from the spontaneous cure of tuberculosis, especially as tuberculin action is nothing other than a natural process of healing with assistance, a fact which I have always emphasized.

Having in mind this hesitation regarding his own early statistics, Dr. Shaw himself undertook a statistical test of the value of tuberculin treatment in the following manner: Out of a number of cases which had been noted by "tuberculin advocates" as suited for tuberculin treatment, he treated the one half with, the other without, tuberculin; the separation of the two groups, as the author expressly emphasizes, was left entirely to chance. I intend to consider, later on, whether the value which the author sees in the last point, and in the omission of any rational selection of cases suitable for tuberculin treatment, is of real value for the elucidation of the truth.

Even apart from the question of principle, as to how statistics should be drawn up in order not to obscure the truth, both sets of statistics obtained by this process are altogether useless.

In his lecture Dr. Batty Shaw himself mentions the first of these sets of statistics. This was obtained by Dr. Rigg at his suggestion, and has been already mentioned above.² It is actually based on nineteen cases. The result of these statistics was to the detriment of tuberculin. Quite apart from the fact that the number of cases is far too small to avoid the probability of a great inequality in the two groups, these statistics are, as I have already stated, totally worthless for the decision of the question of the value of modern tuberculin treatment, because reactions were allowed to occur.

Besides this, Dr. Batty Shaw sent me later a second series of statistics drawn up on his suggestion by Dr. Watkin,³ in which, if I correctly follow the wording, reactions were avoided but which for the greater part were febrile, and therefore cases neither fairly nor suitably chosen for testing the action of tuberculin. These statistics also are limited to the minute number of twenty-two cases; a limitation which, in accordance with the fundamental law of statistics, should never occur in such calculations, and which is least of all admissible in a disease like pulmonary tuberculosis, so difficult to classify and assess, and so irregular in course. Further, the period of observation—three months—was altogether insufficient in order to assess the cases. The author, indeed, contradicts himself, and admits that from such observations not much is to be deduced, and that this is not the object of the communication. But, if not, I must then ask him, Why make the communication at all? But the author finds that a "crying need" exists for the carrying out of similar tests on a larger scale, and he, as well as Dr. Batty Shaw, lays special stress on their method of simultaneous observation of untreated control cases, the neglect of this comparison being, according to them, a chief source of error in earlier statistics.

In opposition to this, I must remark that I cannot recognize the value, the necessity, or the relevance of such statistics at all, and that I hold this assumed control of tuberculin results by simultaneous observation of

untreated cases of similar category as altogether deceptive and misleading, for reasons which will now be stated.

I have in my monograph emphasized the uselessness of the statistical method in general for judging of the value of tuberculin, and given reasons for this view. This judgement is not altered in any way by Dr. Batty Shaw's statistical method, which consists in determining by chance out of the category of cases suitable for tuberculin treatment which shall be treated by tuberculin and which not.

This method suffers from various errors in principle. In the first place, such a determination of treatment by lot would only be permissible if every particular case in the whole statistics (the treated and the untreated) were stringently chosen so as to offer some prospect for tuberculin treatment. For if this condition be not fulfilled, there will be no guarantee, even with far larger numbers than these on which the authors base their statistics, that chance had not brought more unsuitable cases into one or other of the groups. But in this case the statistics give altogether false results. Now, the authors tell us nothing about the fundamental point how the total group of cases (treated and untreated together) was chosen. I have, indeed, some reason to assume that the cases were chosen very unfavourably, since no fewer than twelve out of the twenty-two cases of Watkin were febrile, and were, therefore, in any case, little suited for tuberculin treatment.

A second no less grave error in the miniature statistics published by Dr. Batty Shaw and his pupils lies in the fact that it is wrong to determine in advance a case for tuberculin treatment, and then to carry through this treatment, as it were through thick and thin, with no consideration for the condition of the patient. The foremost principle of tuberculin treatment is that it should be at once stopped if it is seen not to be helping the patient. Now in no single statistical table given by Dr. Batty Shaw is the statement made that tuberculin was omitted when the patient became worse, and since the statistics show that several cases became worse under tuberculin treatment, it must be assumed, so long as the individual cases are not published *in extenso*, that errors in method occur in the materials used for the statistics in no inconsiderable number. It is difficult not to assume that many cases were only treated to the end with tuberculin in the interest of completing the statistics, although an expert in tuberculin treatment would have felt obliged to give up this therapy, after observation of the unfavourable results, it may be even after the first injections. Material containing errors of method cannot be employed for statistical use in a therapeutic question.

Dr. Batty Shaw will probably reply that with such a rigorous use of logic any statistical method such as his, involving division of the cases by lot, is impossible, since many cases will fall out in the course of treatment, and also that in the same way as he questions the correctness of the older statistical methods so in this case also a statistical treatment of the question is altogether impossible. With this view I am in entire agreement, and came indeed in my monograph to the conclusion that the statistical method is one unsuited for giving a clear answer to the question of the value or uselessness of tuberculin. Tuberculosis material is less suited than any other clinical material for statistical use because the cases are so extraordinarily unlike, and cannot satisfactorily be classified in any way, and because in tuberculin treatment everything depends on the individual capacity of reaction, which in any particular case cannot be foreseen.

To prove that this difficulty exists not merely in my own brain I give the following examples: It is a matter of common knowledge that though two cases of tuberculosis may be apparently precisely similar in regard to physical signs, general condition, temperature curve, etc., yet even without any kind of treatment they may run an entirely different course. Under exactly the same external conditions the one may rapidly get well, whilst the other becomes progressively worse, and has a fatal issue. No one who knows anything about tuberculosis will deny this. When, therefore, a therapeutic statistical table is composed of such cases, which are outwardly similar, but in fact quite different in their intimate nature, the results obtained are useless because differences inherent in the material are referred to the treatment. This mistake is,

in fact, again and again made in therapeutic statistics, and nowhere does it play a larger part than in tuberculosis.

The possibility of obtaining statistics regarding tuberculin is, therefore, like an attempt to estimate the action of digitalis statistically, an idea which no one has been unreasonable enough to entertain. Since also, in the case of cardiac disease, functional diagnosis in regard to the recognition of the power of recovery is attended by great difficulties and may often, indeed, be impossible, even if not quite so difficult as is the case with tuberculosis, nevertheless, the result of therapeutic statistics of the action of digitalis would be quite uncertain, and we should be altogether in a bad way if the value of digitalis had to be first demonstrated by a statistical method. It would depend on many chances whether, from a statistical comparison of heart cases treated with and without digitalis, the potency or the inertness of digitalis were shown, and different statistics would probably afford diametrically opposite results. In working up digitalis treatment into a great statistical hotch-potch, the danger would be present of deducing the most extraordinary results—such, for instance, as that digitalis has a better action in cases of mitral stenosis than in mitral insufficiency, better in men than in women, better in Englishmen than in Scotsmen, and similar absurdities. Furthermore, it is not only in respect of tuberculin treatment that many a deduction in medical statistics is of such a character, because the statistical method in medicine is often employed by those who have not the most elementary knowledge of the correct principles and of the precautions which must be observed if it is to be freed from the reproach that it is *le mensonge en chiffres*.

If the question be asked, How, then, under such difficult conditions, is any certainty of therapeutic conviction possible? the answer must be that digitalis itself teaches us how the practitioner may make correct therapeutic deductions. If in a certain number of cases from the moment of application of the therapy, whether digitalis or tuberculin, improvement sets in, the practitioner soon comes with good reason to the conclusion that this therapy is useful. On these lines, and only on these lines, can we come to a conviction of the value of digitalis, and on these lines also the usefulness of tuberculin in a certain category of cases of tuberculosis may be deduced. Even in serum therapy, any practitioner of independent judgement bases himself more on the experience attained in particular cases—for which purpose even a modest clinical material suffices—than on the published statistics, which are, with more or less reason, contested again and again on account of the variations in the separate epidemics. And in serum therapy there are obvious reasons for the choice of the statistical method, because here it is a question of a direct antitoxic treatment, which needs no particular selection of cases and no particular intelligence to carry it out, and is therefore much more appropriate for statistical analysis.

There is innate in this kind of deduction, based on a number, though small, of accurately observed cases, also a kind of statistical method, if one may call it so, which affords a much greater guarantee of correctness than the method recommended by Dr. Batty Shaw, which I should call crude statistics. In the clinical method of therapeutic observation of a particular case the statistical element consists in the fact that in a certain number of cases after the treatment has been commenced a change was noted which was not to be expected, and further that this occurs so frequently that a chance coincidence is excluded. The real guarantee of the correctness of the conclusions lies in the fact that the conclusion is based, not merely on the fact of the employment of the particular therapy and of the improvement occurring, but also—and this is far more vital—on the fact of the coincidence in time of the improvement with the commencement of the treatment, whereby the certainty of the elimination of chance increases according to the laws of probability in powers.

This kind of statistics, which is not commonly called by that name, and which might be called *fine* statistics in contrast to the therapeutic *coarse* statistics which I should discard, is the only kind which is admissible in therapy. It permits of the correct application of the principles of probability on which every statistical conclusion is really based, by taking into consideration the factor of coincidence in time. This method incidentally has the advantage

of giving reliable results even in a limited material. It is the method of the practitioner, and without it no rational therapy could have been developed. For even the largest coarse statistics, which answer merely the question of the course of a disease with and without treatment, fail and are deceptive when the conditions are at all complicated. For a decision on surface therapeutic action (such, for instance, as the effect of opening an abscess) the method of coarse statistics may indeed be used, but is as a rule entirely superfluous, because the method of fine statistics which we have indicated—that is to say, the observation of a series of cases treated according to the method in question—answers such questions at sight, and therefore much more quickly and with less trouble. To convince the reader by such a method of fine statistics there is no need to publish tables; it is not only sufficient, but imperative, to do what Dr. Batty Shaw has left undone—to give detailed and critically stated accounts of cases, out of which the reader can himself work out the fine statistics, exactly as he is in the habit of doing in his own practice. The difficulty of obtaining any firm basis for a therapeutic judgement from such published cases is that these often do not correspond to the standards which any critical reader imposes on himself, and that the standards vary largely according to the medical culture of the author.

On this is based the statement which I have often applied especially to the tuberculin question—that therapeutic conviction, in contrast to almost all other human knowledge, can hardly be transmitted, but is almost exclusively attainable by personal observation. In medicine that alone stands on a firm basis which has been clearly seen and experienced personally.

REFERENCES.

¹ *Ueber Tuberkulinbehandlung*. Von Professor Dr. Hermann Sahli. Basel: Benno Schwabe and Co. Vierte Auflage. 1913. *Sahli's Tuberculin Treatment*. Bale and Danielsson. 1912. ² BRITISH MEDICAL JOURNAL, February 1st, 1913, On the "Controlled" Therapeutic Use of New Tuberculin in the Treatment of Pulmonary Tuberculosis. ³ BRITISH MEDICAL JOURNAL, June 28th, 1913, The "Controlled" Use of New Tuberculin in the Treatment of Pulmonary Tuberculosis.

SOME OBSERVATIONS ON TUBERCULOSIS OF THE KIDNEY, WITH ILLUSTRATIVE CASES.*

BY

J. MILL RENTON, M.B., CH.B. GLASG., F.R.C.S. EDIN.,
SURGEON, OUT-PATIENT DEPARTMENT, WESTERN INFIRMARY,
GLASGOW.

ALTHOUGH the pathology of tuberculosis of the kidney is now fairly well understood, the subject is still a difficult one, as it is only by careful cystoscopic and bacteriological examination that satisfactory results can be obtained. In straightforward cases it is comparatively easy to come to an accurate diagnosis; but others are met with in which the symptoms and cystoscopic appearances are so indefinite that it is a matter of extreme difficulty to determine accurately the condition of the kidneys.

While tubercle of the kidney is not a purely primary disease, and there is always some other focus of tubercle in the body, in the bulk of cases this initial lesion is trivial, and the renal infection is by far the most serious condition. If it can be eradicated the patients are usually able themselves to overcome the initial focus.

It has been shown that at a very early stage of renal infection changes are manifest round the mouth of the corresponding ureter, and that before long definite deposits of tubercle are to be seen there. Gradually other parts of the bladder become infected, until finally there may be widespread tuberculous ulceration. The early changes round the mouth of the ureter are very characteristic of renal tubercle, and it is generally held that they are a certain sign of disease of the kidney; and, conversely, that if the mouth of the ureter is healthy the corresponding kidney is also healthy. This is obviously an observation the truth of which is of very great importance, but I will refer later to some exceptions to this rule.

It has also been shown pretty conclusively that if a tuberculous kidney is removed the tuberculous disease of the bladder will gradually heal, although it may be a considerable time before this occurs.

Tuberculous disease of the kidney is probably unilateral in the bulk of cases at the commencement, and may remain so up to a comparatively late date. On the other hand, a few cases occur in which the infection can be shown to be bilateral at what appears to be an early stage of the disease.

SYMPTOMS.

It is very important to recognize that the early and sometimes even the later symptoms are often entirely referred to the bladder. Probably the commonest initial symptom is frequency of micturition associated with a certain degree of pyuria. Frequency continuing with absolutely no pyuria is almost certainly due to some other cause. The frequency is soon associated with pain before, after, or during micturition. Haemorrhage occasionally occurs as an initial symptom, and may be the first thing to attract the patient's attention. These symptoms may continue, now better, now worse, over a very long period without any renal pain, tenderness, or rigidity, even although the disease has commenced in the kidney and is gradually progressing.

Several cases have been met with in which both kidneys were affected, as shown by ureteral catheterization, though the symptoms were entirely referable to the bladder, and there was nothing in the kidney regions to indicate disease. The following case is quoted as an illustration:

CASE I.

A man, aged 27, gave a history of severe frequency and pain, with occasional haemorrhage, extending over three or four years. He was only able to hold water about half an hour, and was wasted and obviously very ill. He stated that he never remembered having any pain in the renal regions, and on examination nothing could be detected beyond possibly a slight fullness in the left lumbar region. A cystoscopic examination of the bladder was made under chloroform with some difficulty, owing to its small capacity. The whole bladder was covered by a white caseous-looking deposit, and it was not possible to make out any landmarks. Tubercle bacilli could not be found in the urine. Ten days later he died suddenly.

At the *post-mortem* examination it was found that the bladder was completely covered by caseous tubercle. The right kidney was of very small size and converted into two or three cavities. The kidney substance was quite gone and the ureter only a fibrous cord. The left kidney was enlarged, but fixed high up under the ribs, which probably explained it not being more definitely palpable. Its pelvis was dilated and lined by caseous tubercle, and there was only a thin rind of kidney substance remaining.

I quote this case as an extreme example to show that even in advanced cases there may be no direct renal symptoms, and that their absence is no indication that the kidneys are not affected. A certain number of cases give rise to renal pain and tumour formation, but they are much less liable to cause difficulty in diagnosis. Suter¹ found that, out of 60 cases, 53 came on account of bladder symptoms alone, and Braach² gives the proportion as 85 per cent.

The next case is an ordinary straightforward instance of tuberculosis of the kidney.

CASE II.

A boy, aged 14, had a history of frequency of micturition of six months' duration. There was some pyuria and tubercle bacilli in the urine, but no renal pain, and nothing to be detected in the lumbar regions. On cystoscopic examination the bladder was found to be of good capacity, but definite tuberculous deposits were seen round the mouth of the right ureter. The left ureter appeared quite normal, and the rest of the bladder was healthy.

The right kidney was removed and found to contain a broken-down caseous area which had irrupted into the pelvis. The patient made a good recovery, and eighteen months later was perfectly well in every way. Unfortunately he has since been lost sight of.

This is the type of case most frequently recorded, and many of the writers give the impression that a diseased ureter mouth on one side and a healthy one on the other is invariable, and that unless that is found the case is unsuitable for operation. As Braach² and Thomson Walker³ have pointed out, considerable variation in the appearance of the ureters occurs, and it may be impossible to locate the diseased side by inspection alone. The following case is illustrative of this:

CASE III.

A man, aged 21, with a history of frequency of micturition for nine months, accompanied by severe attacks of pain. There was no renal pain, tenderness, or rigidity, but there was marked pyuria, and tubercle bacilli and a coliform organism were present.

* Read before the Glasgow Medico-Chirurgical Society.