On November 25th the condition was much the same. Ten c.cm. were injected at 1.45 P.M. The pulse was 116.

On November 26th the pulse in the morning was 112, in the evening 100. She seemed rather better. Five c.cm. serum were given at 3 P.M. Ehrlich's reaction was not present.

On November 27th the motions were formed; the pulse 120; the breathing quieter. Five c.cm. serum was given at 3 P.M.

On November 28th and 29th she was better.

The convalescence was almost uninterrupted. There were two sudden rises of temperature, which lasted only a few hours.

CASE III.—F. M., a male, aged 19, was admitted on December 16th, 1896. He had had diarrhoea, and had a few spots. The motions were loose and yellow. Ehrlich's reaction was present. The tongue was very furred.

On December 17th he was much the same, and 10 c.cm. serum were given.

On December 18th there was no change in the temperature, but he seemed better. The tongue was clearing in patches. The spleen could be felt. Five c.cm. serum were injected.

On December 19th the tongue was clean, temperature rather higher. Urticarial rash appeared on the right side. Five c.cm. serum were injected.

On December 20th he was about the same. The tongue was rather drier. Six c.cm. serum were injected.

On December 21st there was a good deal of tenderness round the seat of injection, but no deep swelling or glandular enlargement. The rash had extended over the body and itched a good deal, otherwise he was about the same.

On December 22nd the tongue was moist; he slept well, but was suffering from the rash. Ehrlich's reaction was present.

On December 23rd he was better, with the exception of the rash.

On December 26th he was about the same, but on December 29th he was better, and recovery after this date was uninterrupted.

CASE III.

CASE IV.—In this case, which came under treatment about the twelfth day of the disease, and which has not yet quite recovered, though progressing satisfactorily, no improvement was noticed between admission and the first injection. There was much general bronchitis. After each injection the temperature dropped nearly two degrees in the next twelve hours, but subsequently rose again. The patient, however, improved in appearance, the tongue became moister, and the pulse slowed.

REMARKS.

That the serum had a beneficial effect on the progress of the disease I have no doubt; the question remains as to the amount of such effect. In Cases I and II a steady defervescence took place from the date of the first injection, though both cases were, in common with others admitted about the same time, of a severe type, and in the ordinary course of events I should not have expected defervescence to commence for six or seven days. It will be noticed that defervescence took a somewhat long time, so that it is possible that the whole period of this disease was not much shortened, but still the general condition of patients I and II was much less serious during the third week than I have been accustomed to see in cases treated in any other way, not excepting the bath treatment, which I have followed for some years. The state of the tongue improved at once, and the general aspect of the patient changed for the better.

In Case III, and to a slight extent in Case II, some inconvenience was caused by an urticarial rash, which, starting at the seat of the puncture, spread to some distance on the surrounding skin.

No good end would be served by making any extravagant claim as to the value of the serum, and I am aware that these notes must be only of limited value, owing to the comparatively late period of the disease at which the treatment began in each case. I also will not enter into the stormy question of the mode of action of the serum, but simply contribute this paper as a clinical observation.

REMARKS

ON

THE EARLY FEEDING OF TYPHOID PATIENTS

By SAMUEL WEST, M.D., F.R.C.P.,

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THE management of the diet in typhoid fever is of the greatest practical importance, and it is well that from time to time the subject should be considered. Certain points have been raised by Dr. Barrs in his recent article upon the early feeding of typhoid patients with solid food, and it is because I think there is a great deal to be said against both his reasons and his conclusions that his article calls for criticism.

The usual practice of the present day is to place a typhoid patient, as soon as the disease is recognised, upon a fluid diet, and not to change it until all trace of fever has been absent for a week or more, and then to make the change to ordinary diet gradually and with great caution.

As to the best kind of fluid diet during the early stage of the fever, and the best method of changing the diet when the time has come, as well as the time at which the diet could be most advantageously changed, there may be considerable differences of opinion. The two first questions need not be raised in the present communication. It is the third question which is raised—namely, as to when the change in diet may be safely made.

The reason that the diet is not changed until the eighth or tenth day after the fever is that there is a general belief that to do so sooner introduces a risk, and many think a considerable risk, of relapse. Dr. Barrs, on the contrary, advocates an early change from liquid to solid food: first, because he does not share this belief; secondly, because he cannot understand the reasons for it; thirdly, because his own experience is against it; lastly, because the patients are underfed and require more food.

In respect of the last objection it may be asserted that no one intentionally starves a fever patient nowadays. On the contrary, as much food of a suitable kind is given as the patient can digest. Nor is milk, if taken in sufficient quantity, a starvation diet. Other patients may live for months upon a milk diet only, and be capable of a considerable amount of active exertion and mental work; and why should typhoid patients starve on it? The wasting and anæmia and debility of a typhoid patient is not the result of his diet, but of the fever, and is similar to that seen in tuberculosis, in which disease the appetite may be even large and yet the wasting continue. What is called a low diet, that is, a fluid diet, is not necessarily a starvation diet. It is often the patient that is low, not the diet, just as asthenic fever describes the patient and not the disease. If a typhoid patient requires more food it can be given him by increasing the quantity of the liquid diet, whatever it is which is being taken, without necessarily making any change in it, and still less putting the patient upon a solid diet.

Nor are the patient's desires necessarily a safe guide in this respect; so far as there is a mere feeling of hunger this can be easily dealt with by increasing the amount of fluid food he is already taking. So far as the desire is for different food, the propriety of yielding to it must be determined by other considerations, for the cravings in disease are sometimes after what is harmful and not after what is good.

After all, then, we come back to the question of fact: Is it,

or is it not, a fact that there is a risk in changing the diet too soon?

The general opinion is that too early change in diet introduces a risk of relapse. This is a risk only, and not a certainty, of course, and how great a risk it is impossible to express in figures; but as the dangers of relapse are so considerable, the general opinion is that this risk should not be run.

Against this opinion Dr. Barrs quotes 31 cases; but it is to be observed that out of these 31 cases 2 had relapses, that is, 6.2 per cent.

In typhoid fever statistics are notoriously unreliable. I dealt with this question some years ago, in a paper read before the Medical Society of London, and showed what utterly fallacious results can be drawn when the number of typhoid fever patients is small, and that even when the numbers are comparatively large, conclusions may be arrived at which are not borne out by bedside observations.

In questions of the kind we have now under consideration the general floating opinion of the profession is much more likely to be correct. The only doubt which might be thrown upon such an opinion would arise if the question at issue were one to which the attention of the profession at large had not been clearly directed, or upon which there had been a very strong tradition. In the present case neither of these objections hold, for typhoid fever is an extremely common disease, and one of which every practitioner, whether in hospital or family practice, has experience, and the practitioner is called upon in every individual case to decide this most important question of diet, and above all things he has to consider when and how he shall make any change in it. He could not avoid the question if he would, for it is sure to be forcibly brought before his notice, if not by the patient, at any rate by the patient's friends. If, then, the general experience has led to a strong opinion that the diet should not be changed until the eighth or tenth day after the fever is past, depend upon it it is an opinion that is worthy of respect. It should not be lightly set aside because it cannot be actually put into a statistical form, still less must it be upset by appeal to a small number of cases, or because the *rationale* of it is not obvious.

A priori considerations should not, as Dr. Barrs himself says, influence our practice too much, but that is equally true whether they appear to support or to oppose any special line of treatment. One of Dr. Barrs's chief objections to the present mode of treatment is that the explanations given are to him unsatisfactory. They are equally unsatisfactory to me, and I believe them to be incorrect; but still a wrong explanation does not affect the value of a fact.

I am confirmed by my own observation in the opinion generally held that too early and incautious a change of diet from liquid to solid is attended with a considerable risk of relapse. I have seen relapse follow so often that I cannot question the fact; and the relapses have occurred not only in cases in which solid food has been given somewhat prematurely, but even in cases where the change of diet has been from one kind of liquid food to another. Thus I have seen it follow, and I believe result from, the taking of orange-juice or grape-juice without the pips, just as I have seen it follow a plum bun or piece of Christmas pudding smuggled in. The risk that premature change of diet introduces is of a relapse—that is, of a recrudescence of the disease—with all its consequences, whether it be perforation, hæmorrhage, or what not. There is no evidence to show that the food itself induces perforation or any other morbid process except the relapse, and then all the accidents may occur which are met with in the original attack.

If it be a fact, as I believe, that a too early change in diet may lead to relapse, and the explanations suggested are unsatisfactory or wrong, what explanation is there? We cannot answer this question until we know why it is that any fever ever comes to an end spontaneously. We can at the present time only speculate as to the answer. As far as we can judge, the spontaneous limitation of these infectious fevers seems to be dependent upon very subtle changes produced in the body of a chemical or physical character by which either the germs are prevented from developing, or the poisonous products they produce neutralised. If it depends upon the production of antitoxins, bodies of very complicated chemical

composition, it is quite a conceivable possibility that very slight chemical changes may so far modify them as to lead to their complete transformation or neutralisation, or actually to stop their production, in which case the original toxins would produce their symptoms all afresh, and a relapse would occur.

There is some evidence to be produced in favour of these chemical changes to which I am referring. Some years ago, in the course of some observations upon the excretion of urea, I noticed the remarkable influence which an abrupt change of diet has upon the amount of urea passed. For instance, in a patient who had been placed upon a fixed diet for some time and was passing an average percentage of about 1.5 of urea the diet was changed, as, for instance, by the administration of a single egg, or something of that kind. The percentage ran up at once to 3.5, or even 4, and remained at that height for some little time. This was quite independent of the new article of food given, for if the same diet was continued the percentage of urea fell in the course of time to what it was before, namely, 1.5; or, if the offending article was withheld, so that the diet continued what it was before, the percentage still remained high for a time, so that the total excretion of urea amounted to many times the equivalent of the nitrogen contained in the article of diet which had been added. This is a very instructive and suggestive fact, and shows how considerable the chemical changes may be which slight causes can produce.

Diabetes mellitus affords another illustration of the same kind, for the ingestion of a small quantity of sugar or starchy food is followed by a very considerable increase in the amount of sugar eliminated, and this may last many days, and be altogether out of proportion to the actual amount taken, so that sugar acts upon a diabetic patient as an active poison.

In another disease of an entirely different character from typhoid fever, namely, rheumatic fever, I have similar observations upon the excretion of urea to show what profound effect is produced by slight alterations in diet. But rheumatic fever is of especial interest in relation to the question we are discussing, for it has been long accepted as a clinical fact that it is very easy to determine relapse by changes in diet. Rheumatic fever is no doubt a germ disease, just as typhoid fever is, and no doubt the true explanation of relapse is much the same in either case.

If this conclusion to which observation has led me be true, we have a sufficient explanation of relapses occurring in various diseases as well as in typhoid fever, as the result of comparatively slight disturbing causes, of which a change in diet may be one. In typhoid fever the physiological equilibrium is unstable. In all the metabolic processes the same instability is seen which is so obvious in the temperature. How easily this is upset by intestinal disturbance we have daily clinical experience.

If what I have said is true, it is change as change which does the mischief in typhoid fever, and not so much the alteration from one kind of diet to another, although it is quite clear that the more abrupt the alteration the more likely is it to produce these effects. This is, I believe, the essential fact in typhoid fever, and it so far influences my practice. In the febrile stage it is important first of all to find out a diet which suits the patient. Milk is the simplest diet, and fortunately it suits most people, but it does not suit all. If milk cannot be taken the diet must be modified, and this or that form tried until one is found which agrees with the patient. As soon as the suitable diet is found, it should be continued and not changed without good reason. "Leave well alone" is an excellent maxim in medicine, and it is one of constant use in typhoid fever. If there are exceptional cases, or exceptional circumstances arise in any case, exceptional treatment may be necessary. So with the diet: if change is necessary, change must be made, but it should be made as it were under protest, and only when distinct necessity arises. Opinions differ greatly as to what is absolutely the best diet for typhoid patients. Some lay great stress upon the feeding, the giving of milk only, and the avoiding of beef tea. Others do not object to a combination of milk and beef tea or prepared foods. Some like eggs added to the milk, and so forth. To my mind it matters little so long as the diet is liquid, sufficient in quantity, and agrees with the

patient, if it be not changed. If I come to a patient and find that patient upon a diet which suits him, although it might be one which I should not myself have originally selected, I should not change it if the patient were doing well.

In regard to the other question—namely, at what period of convalescence it may be safe to pass from the diet of fever to a more ordinary diet, I believe the best rule is that which is laid down by most authorities in the present day, and which I always practise—namely, to make no change whatever in the food until the temperature has never risen above the normal for ten days. I have never seen any harm done by keeping a patient a few days longer on the ordinary liquid diet, especially if its quantity has been increased, and if it is not necessary why should any risk be run at all, especially when it is remembered how serious a relapse may be? During the first day or two the risk of a relapse being produced by a change of diet is considerable, and it becomes less and less with every day of the post-febrile stage; it is less at the eighth day, and still less at the tenth.

As a rule I think it may be safely said that if the patient passes the tenth day the risk of relapse is extremely small. This, however, is only a general rule, and there are exceptions to it; thus I have lately seen a case in which the relapse occurred on the fifteenth day.

I do not think this question should be treated as an open one, as if it had not been seriously considered hitherto, for the current opinion is really the outcome of a multitude of observations conducted without bias over a series of years, and all the more trustworthy because no attempt is made to prove it by figures. So far as my own personal experience is concerned, I am led to endorse the current opinion with emphasis. I cannot but believe that if change of diet from liquid to solid food in the early days of convalescence, and still more before the fever has ended and convalescence commenced, become the routine practice, some of the individuals upon whom the experiment is tried will suffer, while in the end the conclusion will be that the current opinion is correct.

GRASS SPIKELET IN THE CONJUNCTIVAL SAC OF A CHILD: SUPPURATION OF THE CORNEA: COLLAPSE OF THE EYEBALL.

By H. PERCY DUNN, F.R.C.S.,
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THE following case is remarkable, partly on account of its rarity, and partly because of the sad result.

A child, aged 3, was brought by her mother to my outpatient room early in January with the following history. Four weeks previously its right eye began to become inflamed. At about that time the child had had a fall, striking the side of its head forcibly on the floor, but it was not thought that the fall had had anything to do with the condition of the eye. The eye was bathed from time to time with warm water, but gradually became worse. Ultimately some lotion was obtained from a chemist, and with this attempts were made to wash the eye. By this time, however, the patient had begun to resent all interference. Whenever any preparations were made to apply the lotion the child commenced to scream, and, in short, resisted so forcibly that the parents ceased for some days to disturb it by attempting this measure. A medical man was next consulted, and on his advice the child was brought to the West London Hospital.

When I saw her there was much oedema of both lids, and a muco-purulent discharge freely oozing from the inner canthus. An attempt was at once made to examine the eye, but this was productive of violent screaming, so that it was necessary to administer chloroform. Preparatory to examining the eye under the anæsthetic the parts were thoroughly cleansed with a 1 in 1,000 solution of chinolol. After the lids had been retracted, and the eyeball exposed, it was seen that the cornea had sloughed, and that the globe was partially collapsed. While, however, the chinolol solution was being used to wash out the sac, a linear body about $\frac{3}{4}$ inch in length came into view, lying over the front of the remains of the cornea. It was thickly covered with pus, and at first it was taken to be a brush bristle to which some thickened pus had

adhered. I passed the foreign body over to Mr. M'Dougall, one of the members of the post-graduate class, and after careful washing he discovered that it was a grass spikelet, the full size and appearance of which are depicted in the accompanying figure.

On inquiry from the mother it was not possible to obtain any history showing how the piece of grass could have gained entrance to the child's eye. Judging, however, from the facts that the cornea had sloughed, and that the globe was partially collapsed, there is every reason to suppose that the foreign body had been present for some time acting as an acute source of irritation in the conjunctival sac. The case also possesses additional interest, inasmuch as experience teaches that foreign bodies upon the surface or within the eyes of children are rare. For example, I have never had to remove a foreign body from the cornea of a child, such as might have been forcibly blown there by a high wind. Again while particles of dust are commonly enough seen under the lids of adults, I cannot recall any instance in which I have been called upon to evert the upper lid of a child for the purpose of removing any such foreign substance. The significance of the rarity of these cases in children is self-evident. Moreover, an adult usually volunteers a statement that "something has gone into the eye," whereas, under similar circumstances, such a hint as to the condition of affairs could scarcely be forthcoming from a child. In the case above narrated, although the foreign body must from the commencement have been the cause of the whole trouble, its presence appears never to have been suspected. The inflammation, of which it was the source, was attributed to other causes, and even an ophthalmic surgeon might easily have been deceived at the beginning of the case, unless while examining the eye very carefully some portion of the foreign body had come into view.

From an examination of the literature of this subject I cannot find any record of a case similar to that narrated above.

In conclusion, I may add that in this case I used the new antiseptic, chinolol; for some time now I have discarded all other antiseptics in my work. Chinolol has many advantages over the various other antiseptic drugs: it is non-toxic, non-caustic, and does not coagulate albumen. These are valuable qualities which have commended it to surgeons. But, among other things, it possesses the advantage of not causing any ill-effects to the skin—for example, of the hands when used in strong solutions. It is, of course, impossible to employ a 1 in 1,000 solution of carbolic acid without the skin being much damaged; whereas a 1 in 1,000 solution of chinolol, while acting as a powerful and perfect germicide, has no deleterious effect whatever upon the skin. Chinolol is now, I believe, coming into much use in general surgery and gynecological practice; hence I have thought it of interest to give the new antiseptic a trial in ophthalmic work, and so far my experience of it is very satisfactory.

THE INTERNATIONAL MEDICAL CONGRESS.—Professor W. K. Roth, General Secretary of the Twelfth International Congress to be held at Moscow this year, requests us to state that the titles of all communications intended to be presented at the Congress, together with a summary of the conclusions, must be sent to the Executive Committee not later than June 1st, 1897.

At the meeting of the subscribers to the Liverpool Northern Hospital, held on January 20th, the Secretary stated that Mr. Levy, the Chairman of the David Lewis Trust, had decided to give another £20,000 towards the rebuilding of the hospital, thus raising this generous gift to £80,000.

AN IMPROVED TUBERCULIN.—The *Médecine Moderne* states that news has reached the Pasteur Institute in Paris to the effect that Professor Koeh, before his departure for the Cape, left in the hands of one of his assistants the formula of an improved tuberculin. The new preparation is said to be in course of manufacture in Professor Behring's establishment for the supply of therapeutic serums.