very quickly is from decongestion of the prostate, but the continued improvement and return of voluntary power of micturition may be associated with a decreased size of the prostate by atrophy.

Now, having admitted so much in favour of the operation, we may ask, Why does it sometimes fail, or why does complete recovery not ensue? I think that the explanation is due to the varying state of the prostate in different patients. Mr. Reginald Harrison classes the enlargement shortly under three heads which I may adopt: (1) enlargement mainly proceeding from blood engorgement (the prostate equal to a mass of erectile tissue); (2) the fibrotic prostate, consisting of a mass or masses of degenerated connective tissue and muscular fibre; and (3) an enlargement due to prostatic tissue resembling an adenoma. Where decongestion alone can diminish the size of the prostate sufficiently then castration should *a priori* be successful. This may apply to the first of the varieties which I have enumerated, but if either of the other two conditions be present then it is not reasonable to expect relief after castration except in so far as decongestion alone can bring it about. It is in this, then, that the great difficulty in treatment of prostatic enlargement obtains. How can the prostatic condition be judged? Rectal examination does not suffice; measurement of the length of the urethra is fallacious; indeed, I know of no plan short of cystotomy by which we can even approximately gauge the state of the prostate. The cystoscope is no doubt often valuable as an aid to diagnosis in prostatic cases, but we cannot even with this tell how much of the enlargement is due to actual increase of prostatic tissue and how much is due to vascular engorgement. We are therefore driven to this position: that in a case of prostatic enlargement requiring something more than catheterisation castration or vasectomy may give benefit but not necessarily, and if we fail to give relief by such means further treatment is required. In giving advice to a patient we are hampered by sentimental considerations which, however, have a distinct influence with the patient and his friends. No doubt this difficulty decreases in proportion to the patient's age, but I have known a patient well over 70 years of age run away from hospital when the suggestion was made that his testes should be removed. This difficulty does not militate against vasectomy so much, but in spite of Mr. Harrison's views on the subject I think we must admit that hitherto success has attended castration more frequently than it has attended vasectomy. Regarding the cases in which it may be unhesitatingly recommended I would say that it is in old men who have had relief by using a catheter for a time but in whom the difficulty of passing an instrument has become increasingly greater. In such cases congestion has probably increased and produced the greater obstruction. Castration, or it may be vasectomy, by causing decongestion may make catheterisation easier and if in addition atrophy of glandular tissue results we may have the need for catheterisation overcome. On the other hand, I am satisfied that in many cases such operations can scarcely be expected to be successful—namely, in those where the fibrotic prostate exists, or where a real tumour, though benign, of the prostate is met with. I show you three specimens removed from such cases, and very briefly I will detail the history of one of the patients.

CASE 4.—The patient, who was 58 years of age, had had for several years difficulty in, and frequency of, micturition. A double vasectomy was performed in November, 1897, but the symptoms increased rather than diminished. He could pass no urine at all naturally. The catheter was required both day and night. The left lobe of the prostate was small, but the right per rectum was very large. I performed a suprapubic cystotomy in November, 1898, and I found a very large right lobe. It was a fibro-adenoma which was easily enucleated. The patient made a good recovery and he does not now require the catheter.

No previous operation had been performed upon either of the other patients, but both had used a catheter for many months and in each hæmaturia was a prominent symptom. In both of these cases a suprapubic prostatectomy was carried out and both patients made excellent recoveries.

I do not dwell upon the advantage of this method in cases complicated by the presence of stone, but I wish to mention certain advantages which to my mind are of importance. 1. The operation immediately relieves the symptoms. 2. By resting the bladder the bleeding ceases and the patient gains much benefit from relief to pain and sleeplessness. 3. It admits of washing out the bladder very efficiently if that be

needed. 4. The bladder being at rest it contracts and regains its contractile power and capability of expelling the urine.

The objections which have been urged against the method are: (1) danger to life; (2) imperfect drainage; and (3) that the prostate cannot be wholly removed. Regarding danger to life, such has not been my experience. I have only lost one case—the patient died 19 days after operation, death being due, as far as one could judge at the necropsy, to old-standing nephritis. It is necessary to judge whether when suprapubic cystotomy is performed it is right to proceed to remove the prostate at once or whether it should be done at a second operation when the patient is stronger. There is a disadvantage in delay, as the bladder contracts and operation is not so easy. Still, for the time the patient is relieved from dangerous symptoms and he regains strength.

With Cathcart's adaptation of the Sprengel pump drainage can be most satisfactorily attained. The bladder can be kept empty and the patient be kept dry. It is not necessary to remove the whole of the prostate. The chief cause of difficulty in micturition and in the passage of an instrument is as a rule either an enlarged middle lobe or a lateral lobe irregularly enlarged. Such masses can be removed.

The risk of permanent fistula after suprapubic cystotomy is because the natural canal has not been rendered patent. If the obstruction be removed fistula will not persist.

Lastly, in removal of a mass of the prostate it is in many cases easy, if you cut through the mucous membrane covering it, to enucleate a mass with the finger-nail or a periosteum-detacher. The pedunculated middle lobe may have a firm attachment at one point, and this can be safely cut through, as it is fibrous and does not bleed to any extent.

[The lecture was illustrated by numerous specimens, naked-eye and microscopical, and also by instruments.]

DIAGNOSIS AND TREATMENT OF TYPHOID FEVER.¹

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I MUST in the first place express my appreciation of, and thanks for, the honour you have conferred upon me in requesting me to open the discussion this evening upon two important aspects of a very important subject. It is not necessary to mention the endemic prevalence of the disease in large communities, nor its special fatality amongst the adult population, nor its constant epidemic prevalence under certain conditions, in order to emphasise its importance. The advances which have recently been made in our means of diagnosis, in the bacteriology and surgical treatment of some of the complications and sequelæ, in the discovery of an unexpected association with other ailments, as cholelithiasis, and I might add in the attempts to break through an irrational and stereotyped method of treatment,—these facts constitute a sufficient reason for an interchange of opinions and experiences. Unfortunately, I must at the outset apologise for limiting my remarks to certain points in the diagnosis and treatment. The subject is so extensive and affects so many branches of our profession that the reason for limitation is self-evident, and, moreover, the prime object of this evening's meeting is to obtain a discussion participated in by all. I am obliged, therefore, to leave out of consideration that part of it which deals with the differential diagnosis of the various diseases which have been, or are likely to be, mistaken for enteric fever, as well as the specific treatment of many of the complications or sequelæ which are not peculiar to this disease. Taking, therefore, the diagnosis of cases of enteric fever we may divide them into two classes, according to whether it is certain or uncertain. A certain diagnosis may be arrived at either by (1) physical signs or (2) specific tests.

1. I take it that a recent acute illness commencing with lassitude, headache, chills, then epistaxis, enlargement of the spleen, a continuous pyrexia, rose spots appearing in successive crops for several days, and each lasting three or four days, with an absence of leucocytosis, and terminating

¹ A paper read before the Manchester Medical Society on March 6th, 1901.

by a remittent temperature gradually subsiding after, say, 17 or 18 days, and accompanied by sudden profuse intestinal hæmorrhage or undoubted symptoms of intestinal perforation, or further followed by a definite relapse, such a symptom-group may be with certainty diagnosed as due to enteric fever. I am aware that no symptom is pathognomonic, but such a combination is, I think, about as certain as anything in our uncertain life well can be.

2. As regards the specific tests the isolation of Eberth's bacillus either from the stools or from the rose spots is as yet impracticable, whilst tapping of the spleen is unjustifiable. On the other hand, the serum reaction, performed under definite rules of dilution and time, may be as convincing and certain as any imaginable symptom-group. It cannot be said that the degree of dilution and the rapidity of agglutination forming the time limit, and acting as a basis for such reports as incomplete or doubtful reaction, are matters of no moment to the physician. It has, I believe, been shown that occasionally with a dilution of 1 in 10, and a time limit of one hour, blood from individuals in whom no history or suspicion of enteric fever could be obtained will produce an agglutination sufficient to warrant the report that the reaction is present. In considering a series of about 200 of the most recent cases examined, in 12 instances a doubtful reaction was reported, in all of which the evidence for the possibility of enteric fever was of the most meagre description, and I am strongly of opinion that some of these cases may be examples of the danger I have just mentioned. But unfortunately this is not the only difficulty, for after the discovery of the reaction it was soon observed that the time of its appearance and the rapidity and completeness of agglutination vary enormously. It is therefore necessary to discuss the possible difficulties in its application.

The blood-serum may retain its peculiar power for so long a time that the attack of enteric fever having passed undiagnosed the reaction is obtained and first noticed during a subsequent ailment. In addition, since the injection of typhoid vaccine has been done on a large scale it is well to keep in mind that the reaction has been obtained as long as two years after its administration. In any case one must exclude the possible previous introduction of typhoid toxins into the blood unless the agglutination occurs very rapidly and with a high degree of dilution, though failing this the most certain test is, I think, to obtain an agglutination increasing in rapidity and completeness as the disease progresses. Thus I recently obtained three reports as follows: (1) incomplete reaction on the tenth day; (2) somewhat incomplete on the seventeenth day; and (3) Widal reaction on the twenty-ninth day. The exact terms are, of course, the result of a standard set up by the manipulator, but the course undoubtedly shows a gradual change in the power of the serum, which is only consistent with the view of a progressive introduction of typhoid toxins into the system, and under such circumstances, where the disease is under observation and the change well marked, warrants with certainty the diagnosis of typhoid fever.

I have already mentioned that the time of the appearance in the serum of the power of agglutinating recent cultures of Eberth's bacillus varies greatly, but it was early discovered that the later in the attack the blood is examined the fewer are the percentage of failures, so that if it be taken at the end or during early convalescence this percentage is reduced to a minimum. I might here refer to the results of the blood examinations made during the last 12 months. Out of 214 cases a positive reaction in 149 instances was obtained at periods varying from the fourth day to the fifth or sixth week, and in this series the clinical evidence was in agreement, whilst in other 11 cases the illness was quite over. In nine instances the clinical evidence was indefinite and the serum reaction doubtful. In 22 instances the clinical evidence negatived typhoid fever and there was an absence of the Widal reaction. Of 18 cases in which the clinical course or a post-mortem examination allowed one definitely to diagnose typhoid fever, in 10 an incomplete reaction was obtained at periods varying from the eighth day to the end of the third week. In another of these cases the reaction was incomplete on the fourth day of illness, two others gave a negative reaction on the sixth and tenth days, whilst other two were incomplete and doubtful after nine and 12 days. The last case died a few days later from intestinal perforation and at the post-mortem examination there were several typhoid ulcerations in the ileum. Completing the 18 cases, there was one instance in which an incomplete reaction was obtained after 10 days and a positive after

16 days, whilst there were two cases where the reaction was negative at the end of two weeks and positive at the end of a month, but in each instance the patient was through the primary attack at the time the positive reaction was obtained.

To complete the 214 examinations, on two occasions a positive reaction was shown which referred probably to a previous illness, since one of these was proved later to have been a case of typhus fever, and in the other there was no acute fever at the time, but seven weeks before the patient had suffered from "influenza." There were also three instances in which the blood gave an incomplete reaction, but the clinical examination was against the view of an attack of typhoid fever.

Summarising the results one finds that of the 214 cases in 209 instances the clinical evidence and the blood reaction were either in agreement or not at variance. It must, however, be noted that in the 149 cases first mentioned where the reaction was positive there were a large number in which the clinical symptoms or history would not have allowed a definite conclusion, and it would be difficult to over-estimate the value of the serum reaction in recognising the existence of a previous or recent attack of enteric fever, as well as its co-existence along with symptoms of another ailment. On the other hand, one has to admit that an incomplete reaction is not unfrequently most confusing and should, I think, be appreciated at something like the value of Ehrlich's diazo-reaction—that is to say, belief or discredit must be attached to it according to the clinical manifestations. Then, again, it is found that in a fair percentage of the cases the serum reaction may remain indefinite or even negative for the first 10 days or fortnight, and that occasionally the positive result may not be obtained until towards the end or possibly after the completion of the primary attack, so that, as in the case I have cited, it is quite possible for death to take place from enteric fever without having obtained a definite Widal reaction, though the blood has been examined only a short time before. Still, if the blood be examined periodically throughout the course of the disease one has to admit that the greater danger is not that the reaction will not be obtained in a case of enteric fever, but rather that evidences of its presence will be obtained in people suffering from other ailments. In most cases this is evidently the least important or least serious of the difficulties. I think one may therefore summarise the position of the Widal reaction somewhat as follows: a negative result, say, at the end of the first week of illness indicates at most a probability against the diagnosis of enteric fever, whilst each succeeding negative result increases this probability, so that by the end of the attack a negative result almost amounts to a certainty against the diagnosis. In fact, in all cases in which, as convalescence from the illness is approaching, or in which, after there has been a continuous fever of about three weeks' duration, the serum reaction is still found negative, I should say that the diagnosis of enteric fever must be abandoned unless the physical signs form such a symptom-group as I have already referred to. I would even go further and include the occurrence of an incomplete and doubtful reaction obtained by the serum from a case at the beginning of convalescence, especially if the same result had been noted during the acute stage of the illness.

Except in very unusual circumstances the diagnosis of enteric fever must always be uncertain during the first week of illness. However strong the probability may be until the presence of a well-marked Widal reaction, when the serum is tested for the first time, or the development of such signs as rose spots, intestinal hæmorrhage, &c., it is evident that without such indications the essential element for making a certain diagnosis must be considered as wanting. Of course, strong probabilities may be obtained where it is known that the individual has been exposed to infection or where other undoubted cases have arisen in the same family. I trust that I shall not be convicted of platitudinism when I lay stress on the necessity of suspecting enteric fever in any acute illness of a week or more in duration which is characterised by indefiniteness of symptoms to physical examination, and especially if the onset is gradual and is accompanied by headache, weariness, languor, feelings of chilliness, and enlargement of the spleen. In this connexion I should like to lay special importance upon the enlargement of the spleen, as in the few cases in which I have been consulted at the beginning of the attack it has seemed to me that the chief increase in bulk has

already taken place before the illness is far advanced. Then, again, with the presence during the early period of Ehrlich's diazo-reaction in the urine, one may in conjunction with symptoms excluding measles, acute tuberculosis, &c., infer the probability of enteric fever, though I think the fact that this reaction can be obtained in other diseases will always militate against any reliance being placed on the test, no matter how regular may be its appearance, in the earlier stages of enteric fever.

The indefiniteness of the symptoms which characterise the early stage of the disease and which is responsible for the frequent diagnosis of influenza and bronchitis may, on the other hand, be continued even throughout the attack. A person complains of lassitude, disinclination for work, and loss of appetite. The thermometer reveals the presence of a pyrexia which may be slight and either remittent or continuous. There may or may not be abdominal symptoms and signs of bronchitis. Such a condition may last for a variable number of days, and even weeks, and be unaccompanied by more definite indications. Attacks of this description may, and do undoubtedly, occur in adults, constituting such types as mild, abortive, or ambulatory. They are, however, relatively more common in children. In them also not only are the rose spots absent in a larger proportion of the cases, but even when they occur they are often smaller and more readily overlooked; and again shorter attacks with the temperature of a remittent type are more frequent, or cerebral symptoms, such as irritability, possibly with retraction of the head, somnolence, constant moaning, and even temporary strabismus, may overshadow everything else, the abdominal or respiratory symptoms being so slight that the thought of enteric fever is never entertained. In all such cases, and speaking generally in all doubtful cases, it must be admitted that the value of the serum test, with the reservations which I have already mentioned, cannot be too highly rated. Again, there is the exceedingly interesting apyrexial form of enteric fever, but here I think that unless rose spots were present in crops over a number of days, or unless the Widal reaction were found to increase in power as the disease advanced, an unquestionably positive diagnosis would not be justifiable without post-mortem evidence. Then there is the condition of typhoid septicæmia, when the exact cause can only be determined by bacteriological investigation.

Difficulties in diagnosis may arise, however, not from the indefiniteness of the symptoms, but from their special localisation when the brunt of the infection has an unusual disposition. Thus the symptoms may be clearly and solely those of meningitis, pneumonia, nephritis, cystitis, or cholecystitis until the further progress of the disease brings with it more characteristic signs, or the development of the Widal reaction or a bacteriological examination discloses the exact cause. Occasionally under these circumstances a suspicion of enteric fever may arise where a connexion with previous cases can be traced, but in the absence of such indications the true nature may never be suspected until the bacteriological investigation reveals the etiological factor, for even post mortem the distinctive signs of enteric fever may be absent.

Concluding, then, the question of the diagnosis of enteric fever I would say that clinically, despite the occasional record of rose spots in other diseases, such as the case of acute tuberculosis recorded by Fagge in his "System of Medicine," the only sign upon which absolute reliance can be placed is the appearance of rose spots on several consecutive days either during a primary attack of a continuous fever at least 17 or 18 days in duration, or during a relapse, though I think that an otherwise similar attack accompanied by temporary enlargement of the spleen or intestinal hæmorrhage, &c., would require very clear bacteriological evidence to prove its non-typhoid nature.

Treatment.—Turning now to the treatment of enteric fever I do not propose to question the advisability of confining the patient to bed from the earliest possible moment and keeping him there throughout the attack and for the first 10 days of convalescence. Nor do I think a difference can arise as to the importance of the general hygiene of the sick-room or the necessity for good nursing. Again, I shall not attempt to discuss such very questionable treatment as the use of antiseptic and even vaunted specific drugs, and my own experience has unfortunately caused me to conclude that no benefit is to be expected from the typhoid antitoxin hitherto prepared. Further, we shall probably gain nothing by considering the various remedies which have been used in the treatment of the different symptoms.

On the other hand, not only is diet admittedly a most important factor, but comparatively recently attempts have been made to modify or to alter long-established rules.

Even in the most recent text-books we are taught that irrespective of the condition of the patient and his apparent fitness, and ignoring the type of his attack, we must not allow solid food, not only during the acute stage, but till his temperature has been normal for 10 days. Personally, I think it is impossible to have treated many cases without being struck with the hardships of such a rule in some and with its impracticability in others. In the first place we shall doubtless agree that so long as the tongue is dry or the patient from his mental or general condition has no inclination for, or ability to consume, anything beyond fluids, we cannot do better than keep to a rigid liquid dietary, and the advisability under these circumstances of regular administration, limited quantities, and free dilution, along with periodical inspection of the stools, is not in question. But apart from such cases I wish to maintain that each patient should be dieted according to his condition, so that it may happen that one will receive a limited supply of solid food of a specified character almost throughout the attack, whilst another may not be able to resume solid food until convalescence is far advanced. But as an attempt at construction is often more serviceable than detailed destructive criticism and since I firmly believe in maintaining a definite plan, it will, perhaps, suit our purpose better if I lay down a few rules. I should say, therefore, that (1) in every case the patient is to be put on fluids alone at first and (2) that known easily digestible food only should be allowed until at least three weeks after the termination of the attack. Personally I like very much the gradual change dictated by old-established usage, such as fish, chicken, mutton, and beef, in this rotation and allowing time to observe the effect, or want of effect, of each change. Practically, therefore, the change consists solely in altering the time at which a trial with solid food may be made from the tenth day of convalescence to any period during or after the attack when the patient's wishes and our own examination or experience convince us that the attempt may with benefit be made. A third rule which I would make is that until the end of three weeks of convalescence precautions should be taken to prevent the swallowing of large imperfectly-masticated pieces of meat, &c., and here it must be admitted that the absence of ravenous hunger, from the earlier satisfying of legitimate cravings, is itself one of the best safeguards against an inclination to bolt food. At the same time if the habit is firmly rooted I have combated it by having the meat previously minced or chopped.

The objections raised against any deviation from a fluid diet are as follows:—1. The physiological processes are so altered during the febrile state as to interfere with the proper digestion of other forms of food. In practice I think the patient's wishes can with advantage be consulted to a very large extent, and if one carefully watches the effect I think one has to admit that the presence of fever *per se* does not necessarily prevent the assimilation of various solid foods. 2. A second objection urged is that any other form of diet may irritate the ulcerated and catarrhal bowel. But here again one finds numerous foods, such as underdone or raw meat, white fish, boiled chicken, rice, &c., which are more readily digested and assimilated than the precipitated casein of cow's milk, and it is a matter of experience that in some cases with diarrhoea an improvement is best obtained by an alteration in the dietary. Of course, when speaking of any food given during the acute or early convalescent stages it is to be understood that special attention is paid to the cooking and to freedom from small bones or large pieces of skin, cartilage, tendon, &c. 3. But perhaps the most constant objection urged is the danger of producing perforation of the bowel owing to undigested particles increasing the catarrhal state or mechanically causing a rupture as they are forced along the intestine. This fear is, I think, always uppermost in the mind of the medical practitioner treating the case, and I can affirm that it is a *bête noire* to the initiated who are under treatment. I am aware that authentic instances are on record, and I well remember the case of a youth who shortly after a Christmas dinner showed symptoms of perforation. But it must be remembered that with one or two exceptions perforation has always been found to be the result of a progressive ulceration or necrosis, and the case which I have just mentioned was no exception to this rule, since at the laparotomy the bowels were free from solid particles, so far as could be seen, and the faeces exuding from the perforation were of the usual soft yellow character, whilst

there was nothing suggestive of mechanical injury. Moreover, it must be evident that such an occurrence cannot be brought in question under the rules which I have mentioned. A fourth objection to the too early administration of solid food is the possible production of a temporary fever or even of a definite relapse. I am so heretical as to doubt the existence of a *febris carnis*, and in this connexion I should say that as sudden and at present unexplained rises in the temperature are relatively common during early convalescence from enteric fever there is always the possibility of their relationship to food being *post* and not *propter*, and I have for some time ceased to make any alteration in the diet solely on account of the supervention of pyrexia, with the result, as I think, that the fever has immediately subsided or pursued its further course quite independently of the food given. Then as regards a relapse. This is generally admitted to be the result of a reinfection, so that it is not rational to assume a definite causal relationship where it occurs immediately after. A more likely suggestion is that since during or after an attack of enteric fever the bile may not unfrequently be crowded with the specific bacilli the administration of solid food by increasing the flow of bile may as a result increase the number of bacilli poured into the intestine and thus be instrumental in producing a relapse. Such a suggestion is, however, purely theoretical, lacking confirmation, and from our ignorance of the nature and causes of a relapse difficult to prove. I am, therefore, of opinion that the adduced objections are not a sufficient warrant for the rigid rules which have till recently fettered us. On the other hand, the increased mental satisfaction and improved bodily constitution which follow a more liberal and varied diet demand that some such alteration as I have suggested should be made. In hospitals, certainly, when dealing with a large number of patients, the change is even more essential, as it tends to do away with that deceit and the smuggling of possibly harmful foods which is only too commonly practised or attempted when the restrictions are rigidly enforced.

In conjunction with an altered dietary, a system of regular immersion in a cold or rather a tepid, bath during the acute stage of illness is capable of greatly altering the aspect of a ward containing a number of enteric fever patients. At the present time antipyretic drugs for the reduction of temperature are advisedly discouraged, and usually one resorts to some form of hydrotherapy. Cold or tepid sponging or douching and cold or ice packs can be used under all circumstances; the essential factor, however, is that they must be regularly and thoroughly administered. But whenever possible a tepid bath of from 75° to 85° F. is to be preferred. It must be distinctly understood that beyond acting as an excellent hypnotic and stimulant in delirious or wakeful cases no immediate benefit is to be looked for, whilst to be of any real value they must be given regularly as the pyrexia again reaches a certain maximum. A short experience of their use will convince one (1) that they are potent factors in reducing temperature with the diminution of its attendant evils; (2) that the intellect keeps clearer during the disease and stupor is lessened; (3) that they exert a general tonic action on the system—the skin keeps softer and more healthy, the circulation is improved, bronchitis, hypostatic pneumonia, and broncho-pneumonia are less prominent features, whilst headache and insomnia are much relieved, the patient frequently sleeping between successive baths; (4) it will be seen that they retard emaciation and maintain the muscular strength and nutrition; and (5) that they diminish the percentage of patients who die from cardiac failure and asthenia. It has further been shown that by their use the toxicity of the urine is greatly increased. To obtain the best results it is necessary that the baths should be commenced early in the attack.

The next point which I wish to discuss is the question of surgical interference. I shall not refer to such operations as tracheotomy for obstructive laryngitis, drainage of an empyema, &c., but limit myself to the consideration of the abdominal complications. Until comparatively recently it seems to have been fairly generally held that the presence of a severe ulceration of the bowel negatived a laparotomy or operation upon the intestines during the ulcerative stage. Enteric fever has, however, benefited from our advances in surgical procedure, so that one may now say that the existence of ulceration *per se* adds very little risk, and that the chief factor to be considered is the general condition of the patient. Such complications as perforation of the intestine,

of the appendix, or of the gall-bladder may and do occur in patients who at the time are not in a fit condition to stand an operation. On the other hand, in a large percentage of the cases at the time of the onset of the symptoms the patient's state is sufficiently satisfactory, but from inability to diagnose these complications until the supervention of more pronounced indications they quickly become hopeless. On this account, though experience on the part of the surgeon is very important, the most essential requirement is an improvement in our means of diagnosis. With this end in view Professor Osler, in an article in THE LANCET on Feb. 9th of this year (p. 386), summarises the various points which should be noted and carefully looked for during an attack of enteric fever, and one cannot help feeling that an accurate record on the lines suggested will furnish most valuable evidence for future guidance and will be the means of materially altering the prognosis of these complications. If one waits for vomiting, or loss of liver dulness, or alteration of the pulse, or any other symptom-group diagnostic of perforation, in a large number of instances the time for operating will have gone by, but by keeping a detailed record of the condition of the abdomen during the attack and carefully noting the onset of the slightest abdominal pain, its character, situation, or persistence, the accompaniment of a rise or fall in the temperature, and the alteration in the distension, rigidity, or mobility on respiration of the abdominal walls, I think one need not despair of yet obtaining very much greater success than has been our lot in the past.

Manchester.

THE TREATMENT OF BRONCHIECTASIS AND OF CHRONIC BRONCHIAL AFFECTIONS BY POSTURE AND BY RESPIRATORY EXERCISES.

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THE principle of treatment to be described has a wide range of application in obstructive affections of the respiratory tract. The symptoms produced by a pedunculated growth in the larynx or by a long uvula may be mitigated by position, though the patient is seldom aware of the relief to be obtained from posture alone. In severe chronic catarrh with tenacious expectoration or in the later stages of pulmonary excavation nothing will restrain the patient from flying up into the sitting posture and bending to the struggle as the first of an endless series of coughs ushers in the paroxysm. In phthisis this commonly ends with vomiting as the only adequate mechanism for complete expectoration. Much of all this is avoidable by suitable posture. It is, however, in bronchiectasis that the postural method has its chief application and that its most striking results are witnessed. The following cases, which were exhibited before the Medical Graduates' College and Polyclinic on June 4th, 1901, will speak for themselves.

CASE 1.—The patient, a well-developed and fairly strong girl, 20 years of age, had been subject for many years since an acute pulmonary attack to cough and expectoration which had been worse during the winters, and presented the general characteristics of bronchiectasis. She had never been under any treatment even as an out-patient until she was admitted under my care into St. George's Hospital 18 months ago, having got much worse though she was not bedridden. The complexion was, and had been, habitually slightly dusky, the subcutaneous tissues were full, the veins, particularly in the hands, were lax, the finger ends were considerably clubbed, thickened, and dusky, and the nails were much incurved. Dyspnoea on exertion had been a marked symptom. The secretion had been of the typical bronchiectatic character, at times fetid for long periods, its expectoration becoming then more and more difficult and occurring with a gush. Considerable improvement resulted from general treatment in the ward and from the relative luxury of rest and hospital treatment. The catarrh and expectoration diminished and she was ultimately provided with an automatic respiratory jacket which she wore with comfort, and at first, she thought, with advantage. This