they may cause changes in it which are beneficial for both the performance of the operation and for the probabilities of recovery after it.

I have been speaking lately of the plans for average or medium cases; and before, of the cases in which no attempt at reduction without operation should be made. You may ask, Are there any cases in which it is justifiable to wait longer after the warm bath, and rest, and chloroform, and a fair attempt at reduction, have been tried and have failed? I will not venture to say that such a case for waiting cannot happen; but I am clear that your rule, with barely an exception, must be that, when you are satisfied that a hernia is strangulated, and you have failed to reduce it with such helps as I have indicated, you should operate. While the patient is still under chloroform, the operation should be done; and you should prepare for it before giving chloroform. Of course, if you are satisfied that the hernia, though irreducible, is not strangulated, you may wait; but in this case you must watch almost impatiently, for an intestine or omentum that cannot be reduced is very likely soon to become strangulated, and so is one that is inflamed or blocked up in a hernial sac. Still, if no signs of strangulation, especially if no vomiting, should supervene, you may wait from day to day; but if the signs do occur, especially if there arise vomiting, or increase of pain, or increasing rapidity of pulse and breathing, then you must operate at once, and you had better not try again at reduction. The trial is much more likely to do harm than to do good: you had better operate at once. Nothing does more harm to a strangulated or nearly strangulated hernia than the force of an unsuccessful attempt at reduction.

While you are waiting, you may use, in different cases, ice or warm dressings, enemata, aperients, or opiates. I cannot tell you the indications for each of them. I am glad to have had no sufficient experience of waiting to have weighed the several values of these things; glad, because I have no doubt that, for every case in which an operation has been avoided by waiting, there have been two cases in which lives have been lost by waiting too long. But there are at least one or two conditions favourable for all cases in which you desire to wait; namely, rest in bed and very sparing food. Nothing should be allowed to disturb the patient's rest, and no handling of the hernia should be permitted. Part of the value of ice and poultices and other like applications is this, that they all keep hands off.

Of other supposed helps I will not speak—of tobacco, and curious postures, and shakings with the legs up and the head down, and cupping-glasses, and other like and unlike things. They are ingenious wrong-doings, more dangerous than the operation which they are intended to avert.

In speaking of herniæ as being reducible, I have had in mind only such as can be completely and certainly reduced. But it is not uncommon to meet with cases of strangulated hernia, in which the reduction is doubtful or partial. As I looked through my cases, I found many of these recorded, in which there was delay in sending patients to the hospital, because surgeons believed that part of the hernia was put back, and hoped that the rest would soon go; and some in which, even in the hospital, there was mischievous delay through the same fallacious hope. It is not easy to say what takes place in these partial or doubtful reductions. Some patients will tell you that the whole never did go back, and that what now seemed to be reduced might only be an additional protrusion. Sometimes, I think, air is pushed back from the intestine, or fluid from the sac; sometimes omentum is put back; sometimes, perhaps, part of the intestine; sometimes nothing—the whole notion of reduction being fallacious. The liability to deception is greater than you would imagine. You may feel a thrill of receding fluid, or a gurgling of air, which you may suppose to be what some describe as the characteristic gurgling (as if anything of the kind were infallibly characteristic), or something slipping back; but all may be fallacious. There is one practical rule for all these cases. If the symptoms of strangulation be not relieved by the supposed reduction; if the vomiting continue, or the pain, or the patient's sense of distress, or any other of the distinctive symptoms—then, without delay, you must operate. A partial reduction of a strangulated hernia, if it be not followed by a complete relief of symptoms, is in nothing better than no reduction.

I may add, that most doubtful reductions are not reductions; and of the partial reductions, none are safe except some of those in which intestine is put back and only omentum remains in the sac.

Be prepared also for cases in which reduction is, or seems, complete, and yet the signs of strangulation are not relieved. In these, a hernia may be returned *en masse*, or pushed into another sac, or between the peritoneum and fascia; or the case may be one of hernia complicated with an internal strangulation, or one of many other conditions so hard to discriminate and deal with, that I can give only one general rule for their management—viz., that if you can feel a lump at

or near the hernial ring, as if there were something which may be a strangulated hernia, then you must operate.

To end what I should say respecting the propriety of operating, I ought perhaps to speak of the condition of the patient as affecting the risk of the operation by reason of age and general health and various complications. Among my cases, I find not only many of the fattest and feeblest, but examples of complication with phthisis, acute and chronic bronchitis, aortic constriction, phlebitis, gastric ulcer, diseased bladder, intestinal disorders of various kinds, and internal strangulation. Patients such as these were one would not wound for any trivial good; but, with a strangulated hernia, the peril of doing the operation can hardly ever be so great as the peril of leaving it undone. Old age and feebleness, fatness, intemperance, or unsoundness of whatever kind, may add to the risks of this, as of any other operation; but all these risks must be accepted. A patient must not be allowed to die with a strangulated hernia, if by any means whatever the strangulation can be relieved; and you must not be averted from the operation by any consideration of the number of deaths that follow it. The deaths after the operation may be 50 per cent.; but the deaths due to the operation are not more than 2 or 3 per cent., and even these would probably have been deaths from the hernia if the operation had not been performed. The great proportion of deaths is made up of those in whom the strangulation has done mischief which the operation cannot remedy. It is not unfair to maintain that, speaking generally, the deaths after operations for hernia are only to be counted as failures to save life, while the recoveries are to be counted as lives saved from certainly impending death.

CROONIAN LECTURES ON DISEASE AND ITS MEDICAL TREATMENT.

Delivered at the Royal College of Physicians, London.

By JOHN S. BRISTOWE, M.D., F.R.C.P.,
Physician and Lecturer on Pathology at St. Thomas's Hospital, etc.

LECTURE II.—*Concluded.*

WITH reference also to *Morbid Conditions, due to the Retention within the System of Matters, originating in the Waste of Tissues*, and destined normally to be removed by the various excretory organs, the principles of treatment may be simple, but the means of carrying them out are by no means equally simple. The point of fundamental importance is, to endeavour to restore to a healthy condition the emunctory to whose failure the particular retention happens to be due. But this is often impossible; and the organ may remain permanently inoperative, or so much impaired as to be totally unequal under any circumstances to the full performance of its duties. In the case of the liver, this is, perhaps, less important than in that of the kidney, partly because the bile is less strictly an excretory fluid than the urine; partly because, being mainly, if not entirely manufactured by the liver, it is formed in less quantity by the diseased than by the healthy organ; and partly because the kidneys, to a considerable extent, act vicariously, discharging with the urine not only much bile-pigment, but other also of the constituents of the bile. The flow of urine should hence obviously be encouraged. The retention of urea is a much more important matter; for not only is it being constantly formed in the body, but no other organ can take on, in any adequate degree, the function of separating it from the blood, and consequently it tends to accumulate within the system. Some small quantity may pass off by the skin; but usually uræmic patients sweat little, if at all; some small quantity may pass off by the bowels, but certainly not much, and it may be added, that diarrhoea is not at all a necessary accompaniment of uræmia. A good deal no doubt passes into the connective tissue and accumulates there, the capillary system throughout the body acting as a kind of general excretory organ; and hence results general dropsy; and hence also, it is probable, that the smaller arteries are everywhere stimulated, as Dr. Johnson has demonstrated, into protective contraction. How far it may be desirable in these cases to promote the flow of urine, and what measures should be selected for the purpose, must of course depend very much on the nature of the renal disease which is present. But it must not be forgotten that urea is itself a valuable diuretic, and that undue stimulation of a diseased kidney by drugs is at best a questionable procedure. The only other secretions which can be promoted with advantage are those of the bowels and the skin. Profuse perspiration by means of

the hot air, vapour, or even Turkish, bath, I believe to be especially beneficial. I may add that it would seem theoretically, and I think that it is, of the utmost importance, in treating uræmic cases, to take measures to check the formation of urea in the system; and that to this end the diet should be carefully regulated, and not be allowed in quantity to exceed the actual requirements of the system.

I pass on to speak of those diseases which I have termed *Mechanical*. I have already explained, by reference to examples which I have quoted, how Nature acts to remedy such maladies, and how inefficiently she acts upon the whole. Of course I recognise the fact, that it is through her agency alone (by availing ourselves of those forces which it is her province to exert, by assisting and regulating the action of those forces) that we, as medical men, can hope to relieve and cure; but I assert that (at all events in reference to the affections which I have now under discussion) she acts, when left to herself, blindly and not beneficially.

Let us take the case of a calculus in the ureter. If that be so small and smooth as not to overdistend or to fret the ureter, it is probably gradually driven onwards by the force of the urine behind it, aided by gentle peristaltic movements of the ureteric wall, and so reaches the bladder; but if it be so large or irregular as to irritate the ureter in its passage, spasmodic contraction of the muscular walls takes place around it, the calculus becomes grasped, and its progress onwards is probably barred, in spite of the distension of the tube behind it by fluid and the powerful action of the muscular walls of this distended portion of tube. It is obvious that, under such circumstances as these, the more powerfully Nature acts, the less chance is there that the obstacle will be removed: and we know by practical experience, as well as on theoretical grounds, that our only efficient means of treatment is to allay irritation and spasm by the use of opium or other remedies of that kind, and so, in the dilated and comparatively flaccid tube, to allow the concretion to be urged gently onwards by that *vis a tergo* the nature of which has been already indicated. All that has been just said in regard to calculi obstructing the ureter, applies with equal force to the case of a small gall-stone seated in the common bile-duct and obstructing it; or to that of a large gall-stone, or a concreted mass of stones, which has escaped from the gall-bladder into the duodenum, and has become arrested in the lower part of the small intestine; or even to that of a solid body which has accidentally passed through the rima glottidis into one of the bronchi. And, indeed, the same remarks are in great measure applicable to all cases in which there is impediment in the course of tubular organs; to cases, therefore, of stricture of the urethra, or of the large intestine, or of the œsophagus; and to those of strangulation of the bowel, enteritis, and intussusception. In all of these cases, equally as in those of obstruction by the impaction of concretions, nothing but harm as a rule can result from the use of drugs or other measures calculated to increase the already violent but futile efforts of the muscular walls of the organs involved, and of those extrinsic muscles which co-operate with them. Our chief hope, medically, lies in the use of sedatives. I may add, in reference to stricture and chronic obstructions of the bowels generally, that it is of the first importance that the food administered should be as small in quantity as is compatible with the maintenance of the healthy nutrition of the body, as nutritious and as easily digestible as possible, fluid or well triturated, and as free as may be from seeds or stones, or other such hard and indestructible bodies. Now, so far as I can see, the condition of the heart in valve-disease is pretty nearly the same as that of the bladder in urethral stricture, or that of the upper part of the bowel in intestinal obstruction. Its muscular walls, in health, act with exactly so much force as is required to effect the circulation of the blood; but when valve-disease is present, causing actual or virtual impediment to the passage of the blood, then hypertrophy and dilatation of cavities gradually take place, the mechanical fitness of the organ (which hypertrophy alone might possibly have maintained) becomes gradually impaired, it acts more and more powerfully, but with less and less effect, and soon, under the least additional excitement, runs into purposeless violence, palpitates, and acts irregularly. Of course I am here speaking only of those hearts the muscular tissue of which remains of healthy quality; where the tissue degenerates, or becomes enfeebled from any other cause, complications are introduced—complications of sufficient frequency and importance, but which it is not my present purpose to discuss. In the case, however, to which I refer, it seems obvious that, as in that of the obstructed bowel or the obstructed bladder, our aim must be not to stimulate the heart to exertion (for that a sufficient stimulus already exists in the obstruction which is to be overcome), but rather to allay the irritability which tends so soon to supervene; and for this latter purpose, quiet of body and peace of mind are probably more efficient than any medicines. When an artery or vein becomes obstructed by a thrombus or an embolus, it is somewhat in the condition of the ureter or bowel or biliary duct obstructed by a

calculus: the passage of blood along it is prevented, and the district of its distribution is, as the case may be, either deprived of its due supply of healthy blood and undergoes necrosis, or becomes unduly full of stagnant blood, livid, and œdematous. There is this difference, however, that the removal of the impediment is either impossible or undesirable—impossible in the case of an artery, undesirable in the case of a vein, because its dislodgment and conveyance onwards would probably be followed by symptoms of still greater gravity than those to which it has already given rise. We have not, therefore, the desire, nor, if we have it, have we the power to render the obstructed passage permeable; and all our efforts must, therefore, be directed to the maintenance of the part whose circulation is affected, in as healthy a state as the unhealthy circumstances admit of, until collateral circulation is established.

I have assumed hitherto that the cause of obstruction does not admit of disintegration or absorption; that, if it be a concretion, it must be expelled; if it be a stricture or any such kind of impediment, it must be coaxed into letting the contents of the canal pass by or through it. And that assumption I conceive to be justified by facts. We have no sufficient evidence, I believe, that any concretion beyond our direct reach admits of being reduced in bulk, or dissipated, or disintegrated, by medicinal substances taken internally; and all evidence with respect to strictures is, that they tend to become tighter, partly by cicatricial contraction, partly by the increase of whatever adventitious tissue forms their basis; and that this progress cannot be arrested by a course of medicine. No amount of iodide of potassium or mercury will cure or even relieve a stricture of the urethra, or of the anus or œsophagus, or remedy the contractions which follow upon the cicatrization of an extensive burn; and of course such drugs are equally inoperative (although the truth of such inoperativeness is then less evident) in respect to strictures which are more deeply seated. In justice to nature, it must be admitted that she occasionally makes a sort of compromise with disease in the patient's interest, as when, in rare cases, a gall-stone becomes lodged in a pouch which it makes for itself in the intestinal walls, and so does not obstruct the bowel; or when a stricture is partly destroyed by ulceration. But these are processes which we can neither imitate nor aid.

The last of the mechanical diseases to which I shall refer is aneurism. It is admitted on all hands—and, indeed, the matter is beyond doubt—that the deposition within an aneurism of layers of fibrine is remedial; and consequently various methods have been recommended for the purpose of promoting this deposition. It has been thought that this might be accomplished by keeping the patient in a feeble or exsanguine condition, according to Valsalva's method, or by the opposite plan of permitting him to live generously, or by the administration of substances tending to increase the quantity of fibrine (or rather of fibrinogen) within the blood. But it is admitted now, I believe, generally, that none of these methods has any special influence. No doubt there are certain conditions of system in which there is a marked tendency to the formation of coagula within the vascular system; and, if these conditions could be induced at will, it is by no means improbable that we should possess in them the means of obliterating more or less perfectly aneurismal cavities. But, while thus plugging aneurisms, we should probably—nay, certainly—be plugging at the same time arteries and veins and the cardiac cavities with thrombi of a somewhat similar kind. While pretending to cure our patient of one disease, we should be manufacturing others at least equally dangerous to life. The conditions of which I speak are those that are met with in the prolonged cachexia of tubercle and cancer, and in the latter stages of renal, cardiac, and pulmonary diseases associated with anasarca. I do not myself believe that any medicine is capable of bringing about, or of assisting in bringing about, this result, except in so far as it aids in promoting tranquillity of circulation; and I certainly cannot think that any other plan of treatment, which might tend to promote coagulation at the expense of the patient's general health, would be admissible.

Parasites affect different parts of the body, and necessarily, on that account, need different kinds of treatment. Wherever we can reach them we have, for the most part, the power of destroying them. Phthiriasis and scabies are hence easily curable, and so is the irritation dependent on the presence of thread-worms. For similar reasons ringworm can generally be eradicated within a limited period; and so, I suppose, it will be admitted, can favus and tinea versicolor, although, according to my own experience, both of these latter (and more especially the last of them) cling to their subjects for years, notwithstanding the most careful treatment. And for similar reasons tape-worms and lumbrici may, I presume, be generally expelled; yet these again sometimes oppose very successfully all our efforts to dislodge them. I recollect very well finding three or four large lumbrici in the small intestine of a man who had died of cholera, attended with copious purging.

But in all these cases we rely, for the destruction or removal of the parasites, on medicinal or other agents applied directly to them. Can we, however, act upon them through the system by medicines received into the circulation? It is maintained by many authors that all the diseases which I have just enumerated are peculiarly apt to occur, and to become inveterate, in persons who are suffering from some form of cachexia, or are otherwise out of health; and with such views they are quite consistent in believing that a cure will be aided, if not effected, by improving the general health of the patient. I venture, however, to doubt very strongly whether these views are correct; at all events I must declare that, after having had a large experience of some of these diseases, and more especially of such as affect the skin, I have altogether failed to recognise any such necessary connection, and that I utterly disbelieve that any of them can be cured either under the influence of tonics or by any other forms of medicine operating indirectly. But this, which is a comparatively unimportant question as regards the parasites which have been named, is of great importance in reference to those animals which revel in our livers, brains, and voluntary muscles. And in fact many remedies have been recommended and employed for the cure of the diseases dependent on these latter parasites. Those which have been most lauded are, I believe, such as are known to be most efficacious in expelling tæniæ; and, recently, Dr. Hjaltelin of Iceland has extolled the success of kamala in the destruction of echinococci. But here once more I must express my entire disbelief in the destructibility of all such parasites, excepting by agents applied directly to them, and my entire disbelief, therefore, in the cure by means of drugs of the diseases due to the different forms of echinococcus and of trichinæ. I acknowledge that my own experience of the treatment of hydatids has been somewhat limited; but I have seen them treated, and have myself treated them in former years, both with iodide of potassium and koussou, without the faintest trace of benefit. When I have seen itch cured by a course of brimstone and treacle, and ringworm by one of sulphurous acid or creasote, or either of them eliminated by diaphoretics, I shall be ready to believe in the curability of hydatids and their congeners by like means.

The *Treatment of Inflammation* is a subject of high interest and importance; and there are no affections in respect of the treatment of which fashion has exercised so great an influence. At one time bleedings, frequently repeated, were the unfailing specific; at another time, calomel and antimony or aconite or digitalis; at another, alcoholic stimulants in large quantities; at another, heat; at another, cold; at another, counter-irritation applied to the surface. But, besides these, innumerable drugs are, or have been, regarded as of peculiar importance in the treatment of certain kinds of inflammation: thus bark and port-wine in tonsillitis, a combination of liquor potassæ and hyoscyamus (in which the hyoscyamus is chemically destroyed) in gonorrhœa, confection of pepper administered by the mouth in piles, arsenic in psoriasis and pemphigus, lemon-juice or potash in rheumatism, and so on. But in truth, although we talk of inflammations as if all the varieties of inflammation which we name or recognise were essentially the same disease, there is no doubt (as I have previously remarked) that under the head of inflammation we include a large number of diseases specifically distinct; distinct as to their causes, distinct as to many of their symptoms, and distinct as to their progress, yet linked together by the common occurrence of the general phenomena of inflammation. Pneumonia is an inflammatory disease, so is rheumatism, so is erysipelas, so is carbuncle, so is psoriasis, and so is nettle-rash; but can anything be more obvious than the dissimilarity between them, or than the absence of any tendency of one of these affections to merge into another of them? It must, therefore, be admitted that there is no necessary absurdity in the belief that different kinds of inflammation may need different kinds of treatment; and that inflammation of one organ may require different remedial measures from those which inflammation of another organ requires. For these reasons, it will be well to consider *seriatim* the treatment of several more or less typical forms of inflammatory disease.

Pneumonia is a malady of frequent occurrence; one, with the symptoms and progress and morbid anatomy of which, all reasonably well educated practitioners are thoroughly familiar; and one which has been subjected to all varieties of treatment. In this, as in all inflammatory affections, the general tendency is undoubtedly towards recovery; although here, as in other similar cases, it certainly sometimes happens that, owing to the intensity of the attack or to the feebleness of the person attacked, or to the coexistence of complications, the patient dies before the malady has reached its turning point, or it may be even during the period of convalescence. The most opposite plans of treatment have been found by their respective advocates successful in curing the disease. For a long time bleeding was regarded as the only effectual remedy; and, in this belief, large quantities of blood were removed daily by phlebotomy, until, in fact, the patient was reduced to a condition of extreme debility and anæmia; and yet (and I think I may venture to say,

notwithstanding all this) pneumonic patients seem to have recovered in large proportion. More recently the mercurial and antimonial method of treatment was largely followed. I recollect that my teachers at St. Thomas's Hospital, as a rule, carried it out thoroughly; and in my own early days of practice, in the hospital and elsewhere, I acted under the influence of their teaching. To the best of my belief, and I could support my belief by figures, this practice was again so far successful that the great majority of patients recovered. Dr. Todd undoubtedly deserves the credit, which his pupils claim for him, of having proved that alcoholic stimulants may be administered freely in inflammatory diseases, without adding to the fever present, and without other injurious consequences; and he, and his followers, treated pneumonia mainly by the administration of alcohol in large quantities; and here again, if we believe the testimony of witnesses and of statistics, the percentage of recoveries was high enough to satisfy the most sanguine. But surely it is not unnatural to suppose, when so many plans of treatment, essentially different in their principles, have all appeared to accomplish the same end, namely, the cure of a very large proportion of the patients who have been subjected to them severally, that the essential course of the disease, its progress towards recovery, is independent of the specific measures employed, or, at least, so far independent that the remedies effected nothing beyond a little delay or a little hastening of that progress—differences which would scarcely leave their mark in statistical tables, and would be still less obvious in comparisons made between our simple recollections of what have been the results of different plans of treatment which we have witnessed or carried out. And, indeed, so much has this sceptical view of the treatment of pneumonia found its way, during the last few years, into the minds of medical men, that a considerable number of them have adopted the expectant or (so far as medicines are concerned) the do-nothing treatment. Dr. Hughes Bennett especially has made himself the champion of what he calls the "Restorative treatment of this disease"; a treatment which consists in the abstention from drugs, and the administration of food and stimulants; and if his statistics mean anything, they mean that his success has been something quite remarkable.

I have myself, during the last ten years, given up all special medicinal treatment of pneumonia, all treatment, that is to say, aimed at cutting short or curing the disease, and have contented myself with giving such occasional doses of medicine as some urgent or distressing symptom has required for its relief, and sustaining the patient with so much easily digested food as the stomach could be got to retain, and such an amount of alcoholic stimulant as seemed to me to be admissible or desirable; and certainly, to the best of my belief, my latter patients have got as rapidly well as my former patients did, and have recovered in as large proportion. I may add that, during the last year or two, I have in many cases, experimentally, refrained altogether from alcohol, excepting during the period of convalescence, and, so far as I can judge, without any diminution of success. Of course stimulants are sometimes essential, and in large quantities. I recollect a young man who was under my treatment with double pneumonia, in the year 1862; and who, early in the disease, presented exceptionally severe symptoms, such as delirium, feebleness of pulse, subsultus, and other indications of extreme prostration, and whom I at first treated simply with soda-water and a small amount of stimulus. I found out soon, however, that he had been a stoker at one of our large breweries, and had habitually drunk five gallons of beer daily; and I ordered him at once, not five gallons, but still a considerable quantity of beer and some gin, with (as might be supposed) immediate beneficial results. But such cases are, of course, exceptional, and exceptional cases need exceptional treatment.

A special interest has attached to the treatment of *acute rheumatism*, ever since the well-known paper of Sir W. Gull and Dr. Sutton was read, three or four years ago, before the Royal Medical and Chirurgical Society, in which they endeavoured to show, by comparing the results of a series of cases treated, by themselves, with peppermint-water, with series of cases treated in other ways, that a mere expectant treatment was quite as successful in its results as other plans of treatment of greater pretensions. Rheumatism, like pneumonia, is a disease which arises from exposure to cold, or perhaps rather cold and damp; but it differs from pneumonia in this important respect, namely, that while pneumonia tends to run a certain definite course, marked by the morbid processes which are going on in the lungs, rheumatism begins with inflammation of a joint, and is continued more or less indefinitely by successive outbreaks of inflammation in other joints, and in the fibrous tissues of other parts, including that of the heart; often breaks out anew when it had appeared to have subsided; and often assumes a chronic character. Indeed, this is a strong reason to believe that in this disease each local inflammation (which is rarely very intense, rarely much prolonged, and rarely suppurative) acts as the starting point of new complications; that at each such spot some poisonous material is developed, which,

being absorbed by the lymphatics or veins, is received into the circulation, and then infects remote susceptible parts. It has been assumed, though apparently on insufficient grounds, that there is some peculiar relation between rheumatism and the development of lactic acid in the system; and that the continuance of the disease, the acidity of the urine, and the acidity of the perspiration are all connected with this development; notwithstanding (as Dr. Garrod shows) that the blood continues alkaline in rheumatism, and that the perspiration is less acid than in health, and notwithstanding that the urine is only in a comparatively small degree more acid than in its normal condition.

Now, for rheumatism, even more than for pneumonia, numerous drugs, and numerous plans of treatment, proposed on theoretical grounds, or accepted in deference to experience, have been used, are still in use, and are relied upon by medical practitioners. Which of these remedial agents is the most efficient, on which can most reliance be placed? Bleeding, mercury, iodide of potassium, colchicum, opiates, lemon-juice, nitrate of potash, bicarbonate of potash, hot baths, blisters and other forms of counter-irritants applied to the inflamed joints, these and many other drugs and other remedial measures may be enumerated; and, so far as I know, a fair average of success has attended the use of each; each, at certain times and in certain hands, has certainly been accredited with so-called "successful" results. But it is even more difficult here than in the case of pneumonia to judge fairly of the success of treatment, partly because of the great irregularity which marks the usual course of the disease, and partly because we are apt to judge of our success or non-success according as the heart escapes or becomes implicated. One claim asserted on behalf of the alkaline treatment by Dr. Fuller, and supported statistically by Dr. Dickinson, is that heart-disease rarely, if ever, comes on after the urine has been rendered alkaline; but that fact is not admitted by other observers, nor ought it to be admitted except as an accidental feature in a certain series of cases, unless it can also be shown that all tendency to the migration of inflammation ceases at the same time. But that is not asserted, and is most certainly not the fact. A similar claim, with regard to the prevention of heart-disease, is put forward by Dr. Herbert Davies on behalf of his blister treatment, and the claim is, of course, of equal value. And, if my memory serve me, nearly every other plan of treatment which has been fashionable of late years, has been attended by the same happy result. Another claim asserted in favour of the alkaline treatment is, that it shortens the total duration of the disease; a claim which has equally been asserted in favour of all other plans of treatment. And here again statistics are brought forward in proof; statistics which would show that the duration of the disease, under any one plan of treatment, is shorter by a few hours or a few days than its duration is under all other plans of treatment. Of course we must all admit that, if any remedy curtail the duration of rheumatism, and prevent the occurrence of rheumatic heart-disease, that remedy is curative and acts as a specific against the disease. But I fancy that very few of us, looking back to our experience of rheumatism, will concede that any such remedy has yet been found; that very few even of those amongst us, who believe we have some control over rheumatism through the agency of medicines, will pin our faith to any one drug; and that most of us will entertain a very wholesome suspicion of the value of statistics in settling the point at issue. It is curiously illustrative of the value of statistics, that the cases which Sir W. Gull and Dr. Sutton have published and tabulated, in order to show the success of peppermint-water, have been re-analysed and retabulated by Dr. Fuller to prove that they are wrong. For my own part I may venture to say that I have, for some years past, been slowly arriving at the same opinion in regard to rheumatism as I have expressed in regard to pneumonia; and on looking over long series of cases treated on different plans, I have been struck with the little essential difference of result which they manifest. I believe, in fact, with Sir W. Gull and Dr. Sutton, that we have no remedy which is capable of curing rheumatism. I may add here, that the observations on the treatment of acute rheumatism and acute gout by rest and ease, made by Dr. Sibson, in his address on Medicine, delivered before the British Medical Association in 1870, seem to me most valuable. He shows conclusively, I think, by a large array of facts, the insignificant importance of drugs, the immense importance of hygienic measures in the treatment of the former disease; and, in reference to this matter, he arrives at conclusions in which, for the most part, I cordially concur.

Most of us, I presume, have suffered from common *catarrh*, and know well how it generally commences with some little stiffness and tingling in the soft palate, whence the inflammation (attended with febrile symptoms) passes, on the one hand, into the nose; on the other, down the larynx into the bronchial passages, and perhaps along the oesophagus to the stomach, or along the Eustachian tubes into the ears, or involves the periosteum of the jaws, causing face-ache. Most who are liable to the disease know, also, I suspect, that different attacks differ

greatly in their severity, and in the degree in which the tendency to spread and to persist is present; and know also how, as a rule, it takes its own course, and how chronic it is apt to become when it involves the jaws and the ears. Now I do not, for a moment, pretend to deny that many of the discomforts of this affection may be soothed by various measures; that the febrile symptoms may be relieved by diluents and perspiration, that the sore-throat may be rendered tolerable by the application of black currant jelly and other demulcents, that the bronchial irritation may be relieved by the inhalation of steam, and that all the discomforts may be assuaged under the influence of opium; still less do I presume to deny that a cold may be aggravated by the renewed exposure of its subject to the conditions which primarily caused it. But, presuming the patient is removed from all the conditions capable of aggravating catarrh or of re-exciting it, and that his various symptoms are relieved or rendered tolerable to himself, I must confess that I have still a doubt whether by any of these means, or by all of them, you actually cut short the duration of his disease.

Psoriasis (using that term in its widest sense, as inclusive of lepra) is, I believe, generally regarded as a disease which it is within our competence to cure. Many internal remedies and many external ones have been employed against it with apparent success. Of remedies taken internally none is more generally relied on than arsenic, in one or other of its medicinal forms; and I am not going to deny its utility. But you know that a very large proportion of cases of psoriasis are intermittent in their course, the eruption coming out once a year, in the spring or autumn, remaining out for a few months, and then disappearing totally or in great measure, until the time for its periodical recurrence comes round again. Now I am quite sure that many reputed cures of psoriasis by arsenic and other remedies are due to the fact that the patient is put under a course of treatment, and kept under it, until the normal time for the disappearance of the rash has come; and I can assert that I have over and over again treated patients during the period in which the eruption was in abeyance, in the hope of eradicating the disease; and have then been disappointed to find it come out with all its former severity, even while the patient was fully under the influence of the remedy which had formerly appeared to cure it. There is no doubt, I think, that patches of psoriasis may be cured by various local applications.

Does any one believe that a boil, a carbuncle, or an attack of shingles is curable by medicine? does any one trust even to any form of internal treatment to curtail the duration of these or any such affections? Can we even cure them, or cut short their progress by local measures? I think the general answer to each of these questions would be "No;" and this, notwithstanding that in every one of these cases the patient may be much relieved as to his symptoms and benefited as to his general health by various means. Thus it is certain that incision of a boil or carbuncle procures in most cases immense relief from pain, and that the irritation which attends the appearance of the herpetic eruption may be much soothed by various applications, and that the pain which is often associated with it may be kept in check or removed by several forms of treatment. But it is none the less certain, I believe, that in each case the disease (however vigorously treatment may be pushed) still runs its course unchecked, and essentially unmodified. In confirmation of the truth of these statements, so far, at all events, as they relate to carbuncle, I may appeal to the recent writings and the practice of Sir James Paget.

Now, although from the remarks which I have made it will be gathered that my belief is on the whole against the curability of inflammation, I am far from going the length of asserting that all inflammations are incurable—far less of asserting that we cannot in most cases relieve the patient, and aid in his final recovery. And indeed here, as everywhere else, if the cause of disease can be eliminated or rendered inoperative, the disease subsides.

In all cases in which, owing to the situation of the inflammation, remedies can be applied directly, it is certain that more or less relief may be afforded by their application; and in some cases in which the inflammation is entirely superficial, an actual cure may thus be effected. Thus there is no doubt that we can by various local measures—sometimes by the use of cold, sometimes by soothing applications and the prevention of irritation, sometimes by mere cleanliness, sometimes by acting on the affected surface by astringents, or by destroying it by caustics—cure various superficial inflammations of the skin and of those parts of the mucous membranes which are accessible; among them may be enumerated erythema intertrigo, the results of scalds and of irritant applications, patches of eczema, lichen, and psoriasis, and some catarrhal inflammations of limited extent. But although we may thus relieve, we cannot thus cure, the majority of inflammations which involve the same tissues more deeply; such, for example, as boils and carbuncles and erythema nodosum. I think it must be admitted to be

at least equally certain that, although we may by such measures relieve the discomforts that attend an ordinary catarrh, or assuage the pain of a rheumatic joint, or of an inflamed serous membrane, we can in no sense arrest the progress of the general malady by effecting these local benefits. And surely it is beyond all doubt that we cannot thus cure, or even aid in the cure of, deep-seated inflammatory processes; those, for example, which take place in the substance of the lungs, or of the heart, or of the brain, or of the liver.

Again, it is unquestionable that we may, by remedies given internally, assuage many of the symptoms under which patients who are suffering from inflammation labour, and under some circumstances cure the patient or his disease. We may thus by opium, or other narcotics, relieve pain, soothe irritability, and give sleep; and no one will deny the value of these effects in the treatment of acute rheumatism, of inflammation of serous membranes, of carbuncle, of ear-ache, and of numberless other like affections; and we may by the cold bath, or by digitalis, or by aconite, reduce temperature, and there is good reason to believe that such reduction of temperature is occasionally of the highest importance in reference to the preservation of life; or we may even in certain inflammations, as psoriasis, which have something of a specific character about them, effect a cure, at all events a temporary cure, by medicines.

But it is worth while to ask those who believe in the general curability of internal inflammations by the administration of drugs, how many external inflammations are obviously benefited by them. To adduce, in the first place, some of the inflammations which I have spoken of over and over again: Who has seen a boil or a carbuncle, herpes zoster, or erythema nodosum cut short in its progress by means of drugs? Who trusts to internal remedies for the cure of a chilblain or of a whitlow, of a housemaid's-knee, or a group of inflamed lymphatic glands? and who, I think I may venture to ask, has seen erysipelas subside under the obvious influence of some special drug? Yet if so many inflammations which lie under our eyes are manifestly uninfluenced in their progress by internal remedies, and if others of them, which we have been apt to think we can cure by such means, turn out on closer scrutiny to be doubtfully influenced by them, that surely supplies a strong argument against the general curability of internal inflammations.

I fear I shall hardly have conveyed my real meaning if I fail to explain what it is that I understand by the word "cure" in reference to inflammation. Inflammation consists in a certain group of phenomena taking their origin from some special exciting cause. This cause may be exposure to weather, or some irritant acting locally or through the system; the resulting phenomena include congestion and proliferation of cells, which may go on to suppuration, ulceration, and gangrene, and restoration to health by the cessation of these processes, and the absorption or removal in other ways of the products of inflammation. Further, inflammatory diseases have, unlike the exanthemata, a remarkable tendency to recur on the slightest provocation; and undoubtedly their course and severity are largely influenced by the condition of health, or by constitutional peculiarities of the individual attacked.

Now I assume that, when a patient contracts pneumonia, or rheumatism, or erysipelas from exposure to atmospheric influences, the disease may be kept up or increased, and thus soon rendered fatal, if such exposure be continued or repeated; and I assume, on the other hand, that by the adoption of suitable hygienic measures such maintenance or growth of the disease by successive increments may be prevented. Again, I assume that, if two persons who have contracted either of the above diseases be at once put under the same hygienic and remedial treatment, the disease will probably take a different course in each, that difference being dependent either upon the degree in which the primary cause operated in each case, or on differences of antecedent health. Again, I assume that, if the disease be severe, the patient's life may, on account of this severity, be in danger or lost; but that death is due, in many cases, less to the direct influence of the inflammatory process, than to certain secondary complications or accidents which may be arrested, or obviated, or cured. Thus, long-continued want of sleep, or deprivation of food, or excessive temperature, may each of them prove the immediate cause of death; or in erysipelas, œdema of the larynx; or in rheumatism, compression of the heart by excessive accumulation of pericardial fluid. And further, I assume that, when an abscess forms, or gangrene takes place, these may be directly benefited or cured, if near the surface, by local measures. Now, in all these different points there can be no doubt that we have it in our power to influence very materially the course not only of the three diseases which have just been named, but that of many other inflammatory affections. If we do not cure the patient, we have it at least in our power to keep him from exposure, to maintain or to improve (within certain limits) his general health, and to relieve

symptoms and complications which distress or threaten. And thus it may be admitted that we can, by the judicious exercise of our art, give relief, hasten convalescence, and save life, and thus, in a loose sense, cure the disease. But what I understand by the cure of disease is, the stamping of it out—the bringing it to an end before its time. Inflammation has a tendency to take a certain course, and then to die out spontaneously; and unless in any case this normal duration be anticipated by our treatment, we fail, as it seems to me, to cure the disease; and it is in this sense that I doubt whether we have any curative influence over many inflammatory affections.

OBSERVATIONS

ON

SEA-SICKNESS, AND ON SOME OF THE MEANS OF PREVENTING IT.

By SIR JAMES ALDERSON, M.D., D.C.L., F.R.S.,
Consulting Physician to St. Mary's Hospital.

II.

SOME remarks which I published lately through the medium of the JOURNAL were not intended to be an exhaustive statement of my views on the subject of sea-sickness. I propose, therefore, to consider a few points which I intentionally postponed for a future occasion; and I may at the same time briefly allude to certain objections, which, though courteously offered, have, as I believe, been made without a sufficient apprehension of my view of the subject, and of the scientific basis on which it is founded.

The chief points which I omitted to consider are, the immunity of certain individuals from sea-sickness, and the relief which is usually experienced after a certain time spent at sea. But, before entering on these special topics, I wish to enlarge a little further on the connexion of sickness and vomiting with brain-disturbance. It is well known to pathologists that, of all the causes which act sympathetically to induce vomiting, affections of the head are the most common: nothing can affect the sensorial functions without in some way disturbing the stomach. The experience of all practical men testifies constantly to this; but if reference to published authority be desired, I may quote undeniable testimony in a valuable paper in the *Transactions* of the Royal Medical and Chirurgical Society, by Sir Benjamin Brodie (vol. xiv, p. 339). In treating of concussion of the brain, he says that "sickness and vomiting, for the most part, are early symptoms". Now there can be no doubt that the effect on the brain produced by the blood during the descent of the ship is of the character of a slight concussion. It is in the less severe forms of concussion that sickness and vomiting most certainly occur. In the graver forms of head-affection, such as apoplexy, it is not so often found, because it requires a certain degree of excitability of the brain to induce the act of vomiting, and that excitability has been destroyed by extreme pressure.

I pass now to the immunity of certain individuals from attacks of sea-sickness. This at first sight seems an anomaly, and presents a difficulty in accepting the theory which I propose, as well as every other theory which may be brought forward. I can only suggest, as a solution of the difficulty, that there are constitutions so formed as to be very slightly subject to sensorial impressions. The same inequality subsists between different individuals in their capacity of being emotionally affected: some are unmoved by the most distressing subjects; some have their feelings easily excited. It must be borne in mind that the argument as to exceptional immunity is equally applicable to all methods of explaining the existence and causes of sea-sickness.

In regard to the recovery from sea-sickness, there is much to be said without abandoning the theory of the original cause. A wonderful and instinctive power of accommodation to circumstances is possessed by the human frame. In the course of time, the sensorium is able to adapt itself to unusual circumstances; and on this point again we may refer to the same paper by Sir Benjamin Brodie, who, to his statement, "that sickness and vomiting are early symptoms", adds "that they seldom continue after the patient has recovered from the first shock of the accident". I may add to this, that, when organic change has taken place in the brain, as in the presence of a clot of blood, from the rupture of a vessel, there is, on reaction taking place, a cessation from sickness and vomiting.

That the brain has an extraordinary power of adaptation to circumstances, is evidenced by recovery from hemiplegia, as well as by numer-