AN ADDRESS ON SOME POINTS REGARDING ACUTE RHEUMATISM REQUIRING INVESTIGATION.

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By STEPHEN MACKENZIE, M.D., F.R.C.P.,
Physician to, and Lecturer on Medicine at, the London Hospital.

It may seem to some present that acute rheumatism is not a peculiarly suitable or interesting subject for collective study; they may think it is a common and fairly well understood disease. It will be my purpose to show that it is a subject standing peculiarly in need of such investigation. A large number of keen and accurate observers have devoted time and patience to its study; it is always with us, and yet little is known of its pathology. It is still unsettled whether it is a disease of the nervous system, or whether it is a poison circulating in the blood. We have accumulated by patient individual observation, and by the careful study of the disease in our hospitals, with all the advantages of modern appliances and means of research, an enormous amount of information concerning acute rheumatism; but it is admitted by the most thoughtful minds, that our knowledge is inadequate and disjointed. It is necessary to turn in a new direction for the information required to fill up the blanks in our knowledge, and to gather into some store-house, where it can be sifted and weighed, the great harvest of facts concerning the disease scattered over the country, in the case-books and minds of those who do not, as a rule, publicly record their experience. The information that is especially needed at the present stage of our inquiry, is that which it is not in the power of those who, in the main, have done most to investigate the disease to afford. The great bulk of our recorded experience of acute rheumatism has been, as I have already stated, derived from the study of the disease as seen in hospitals. Of the hospital-patient nothing is personally known by the observer as regards antecedents, and the accounts furnished by the patients themselves of their previous habits, health, and diseases, are at least shortcoming. Many engaged in general practice are as accurate and competent observers, and as close reasoners, as the hospital physician and teacher of medicine. They have, moreover, advantages not shared by him. They have known many of their patients all of their lives; they are acquainted with the family, and see how its tendencies manifest themselves in the different members; they are able to observe how any illness is modified by the known habits, previous illnesses, and idiosyncrasies of the patient; and, lastly, they see the patient after the illness is over, and are able to observe the residua of the disease, the manner in which these clear off, or the impress left on the constitution of the patient, permanently or for a time. The points, therefore, to which I shall direct attention, are chiefly those on which information is required at the hands of those who know most of the individuality of the patient, and who are able to look upon him more in the light of a person than as "a case."

There is one matter of investigation which ought to be entirely within our reach, and that is, the geographical distribution of rheumatism within the British Isles. Very little is really known on this point. Senator, on the authority of Hirsch, makes the statement: "In many parts, indeed, as in Cornwall, Guernsey, and the Isle of Wight, the disease appears to be either unknown or excessively rare." (Ziemssen's *Cyclopaedia*, vol. xvi, page 15). This was, of course, written some years ago. As regards the Isle of Wight, I recently applied to Dr. Sinclair Coghill for information on this point, and he has informed me that though, owing to the equability of the temperature and the dry subsoil, the disease is very much less common than in many parts of Scotland and England with which he is familiar, yet rheumatism, both acute and chronic, is neither unknown nor excessively rare either in the regular inhabitants, or in those who go to the Isle of Wight in search of health. I hope the day will come when we may be able to take a health-census; when we can get every practitioner in the British Isles to record, on a particular day, the number of cases of certain diseases actually under his care. Connected with the distribution of the disease is another point on which

information is needed—its seasonal prevalence. This has been worked out in particular localities (On the Seasons of the Year and the Prevalence of Acute Rheumatism, by Henry S. Gabbett, M.D., *Lancet*, October 20th and 27th, 1883), but requires to be corrected by the experience of different places where the conditions of soil, humidity, etc., are widely different. If the whole of the medical profession in the Principality would combine to answer these two questions, what a solid contribution it would be to medicine!

Those, again, engaged in family practice may see a different phase of the disease from that which is met with in hospital experience, for they see it in all classes of the community. I do not believe that the disease presents any important differences in the peer and in the peasant; but what is more important is, that those engaged in private practice see more of the milder phases of rheumatism, for it is obviously the more severe cases that seek and obtain admission to our hospitals.

Whatever view be taken of the nature of rheumatism—whether it be regarded as a neurosis, or as a poison introduced from without, or generated within, the body of the patient, or whether a mixed neuro-humeral view is held—all are agreed that there is some individual proclivity or peculiarity, inherited or acquired, which determines that, certain exciting causes being in operation, rheumatism occurs in some persons, and not in others. In what does this individual peculiarity consist? Are there any outward and visible signs by which this rheumatic tendency can be recognised?

I am afraid that any attempt to connect rheumatism with any physiological peculiarities or temperament has received its death-blow at the hands of Mr. Jonathan Hutchinson, who has shown what confusion has existed in the use of the terms diathesis and temperament, and has demonstrated to us that the criteria of temperament practically resolve themselves into a matter of complexion or colouring (*The Pedigree of Disease*, Churchill, 1884). I know no individual peculiarities which enable us to recognise an inherent tendency to rheumatism; but I think most will agree with me that persons of a sanguine type are much more prone to rheumatic manifestations than persons of dark complexion. That this individual peculiarity is usually inherited, I think we shall most of us agree; but whether the offspring inherit a direct tendency to acute and subacute rheumatism, or merely a basic arthritic tendency or diathesis (Duckworth), which may eventuate in rheumatic fever, osteo-arthritis, or gout, according to the circumstances in which the individual is placed, is a question on which probably opinions will be found to differ. If any criteria can be discovered whereby this arthritic temperament, whether in its limited or in its widest meaning, can be recognised, it will be a signal service to medicine; and it is those who can study families as a whole from whom we must expect help. In spite of the study of temperaments and diatheses being out of fashion, I cannot but believe that much gain will result from an attempt to connect family tendencies with personal peculiarities that reveal their existence.

There is another way by which we may possibly arrive at the conclusion that our patients have a tendency to acute rheumatism; and that is, by a study of the other illnesses from which they have suffered, and of the common ailments to which they are liable. Our conception of rheumatism is too apt to be narrowed to the type of rheumatic fever. Thanks to the writings of Dyce Duckworth, Ord, Barlow, and others, our conception is expanding. From acute to subacute rheumatism we pass without a gap, admitting that the two are the same in kind, though differing in severity. From subacute rheumatism, we pass, by easy gradations, to inflammations of single joints and myalgia. But there are a number of other conditions or symptoms which, whilst they sometimes occur with undoubted manifestations of rheumatism, sometimes occur alone, and may act as "revealing symptoms."

Take the case of a girl, aged 17, who came to my outpatient department last Wednesday. She came complaining of aching pains in her joints, which she had experienced for a fortnight. Examination showed no swelling or redness of any articulation, but I found, on exploration of the chest, mitral incompetency, with a dilated and hypertrophied left ventricle. In reply to my question, her mother said she had never had rheumatic fever, but she informed me she had St. Vitus's dance three years ago, and that she was now troubled with a succession of white bumps, "like nettlerash," in her skin. Who could doubt that the heart-disease, chorea, uticaria, and flying articular pains, were alike expressions of the rheumatic diathesis?

This case conveniently introduces the subject of the relationship of chorea to rheumatism. It is a matter on which opinions differ widely. That the relationship is a close one I have no doubt, and I think I have elsewhere brought forward some strong evidence on this point (*Trans. Intern. Med. Congress*, 1881, vol. iv, p. 97). Dr. Octavius Sturges has certainly done good service, by showing how

necessary it is to correct our preconceived opinions on this point by studying the incidence of rheumatism in our patients apart from chorea. But the relative frequency with which chorea develops in patients actually suffering from acute rheumatism, and conversely, where rheumatic fever develops during the course of an attack of chorea, has no parallel, so far as I am aware, in the history of other acute diseases. If we take inflammatory diseases, as pneumonia or bronchitis, or peritonitis (an inflammation of a serous membrane), or if we turn to the specific fevers, we find no such close association, unless, indeed, we except scarlet fever; but then, curiously enough, scarlet fever stands intimately related to rheumatism. Here let me emphasise the fact that rheumatic manifestations are often not nearly so well marked in the joints in children as they are in the adult, and that we often find endo- and pericarditis in children in whom only the most trivial arthritic affection can be detected. It is not unfrequent in chorea to find evidence of heart-disease where no history of rheumatism can be elicited; it may be that the child has been lame for a day or two, or has complained of pain in the limbs, but the mother has had her mind relieved by the old woman's theory that they were "growing pains," as if pain were a natural incident to growth.

Turning now to *minor ailments*, there is much to be worked out with regard to them. Let us take tonsillitis. It is a well known fact that acute rheumatism is often ushered in with sore-throat. Sometimes this is a more or less diffused inflammation of the fauces and pharynx, but often the inflammation localises itself in the tonsil. Some patients have a sore-throat with each succeeding attack of acute rheumatism. It is noteworthy that, in the "Preliminary Report on Acute Rheumatism," by our lamented friend, the late Dr. Mahomed, it is noted that, in the returns there analysed, in 25 per cent. of the cases tonsillitis preceded the acute rheumatism. Are there any peculiarities by which rheumatic tonsillitis can be discriminated from tonsillitis due to other causes? and are we justified in regarding our patient with tonsillitis as liable to rheumatism?¹

The association between migraine and rheumatism has been pointed out by the late Dr. Anstie, and by Dr. Hughlings Jackson. How far are we to look upon sick headaches, of a recurring kind, as expressions of rheumatism? and does this little "nerve-storm" indicate that rheumatic fever is a nervous cyclone?

There are certain skin-diseases which certainly stand in close relationship with rheumatism. Urticaria is one of these. Undoubtedly, urticaria owns other causes; but it occurs too frequently in association with acute rheumatism, as compared with other acute diseases, for this to be devoid of significance. The various forms of erythema, again, are more or less generally admitted to be associates of rheumatism. Erythema marginatum is seen not rarely with rheumatic fever, as, also, is erythema papulatum. I have known erythema papulatum to give rise to endocarditis without any joint-affection. Erythema nodosum differs so much from the other forms of erythema, as to be held by many as a distinct disease, and occupies a curiously unsettled nosological position. Having, a short time ago, a case of erythema nodosum under my care, in which subacute rheumatism supervened, and knowing the discrepant statements made by writers as to its association with rheumatism, I was myself led to make a collective investigation on a small scale. With the assistance of a gentleman connected with St. Bartholomew's Hospital, and the Registrars of Guy's, St. Thomas's, and the London Hospitals, I obtained particulars of 108 cases of this disease or symptom. Of these 108 cases, in 17, or 15.7 per cent., there co-existed acute or subacute rheumatism; besides which, in another 17 cases, or 15.7 per cent., there were arthritic pains, not the merely local pain of the eruption. If we include these in the rheumatic category, we have 31.4 per cent., associated with rheumatism. Further, sore-throat (including one case of tonsillitis) was present in 11 cases, though this symptom was not especially inquired for. Finally, in 19 cases, or 17.5 per cent., there was evidence of heart-disease, and in 5 of these the endocarditis developed during the attack. One patient had had three attacks of erythema nodosum, each coinciding with an attack of rheumatic fever. To my mind, this evidence points in the strongest possible manner to the inference that erythema nodosum is an expression of rheumatism. There is no other disease to which erythema nodosum stands in similar relationship. I admit, of course, that in many cases it will be the only evidence of rheumatism. But, if true, the knowledge must prove valuable for prognosis and prophylaxis; though, as regards treatment, owing to the nature of the cutaneous lesion, antirheumatic remedies cannot be expected to have the same striking results as are

seen with regard to the arthritis and pyrexia. Then, again, there is a certain form of purpura which, in my opinion, is certainly rheumatic—the peliosis rheumatica of Schönlein. I have collected notes of about twenty cases of this form of purpura, which I hope shortly to publish, and which, I believe, will show that it has features distinct from other forms of purpura. Some of these cases have occurred concurrently with acute rheumatism; some of them have been in persons who previously have had undoubted acute rheumatism, with resulting permanent heart-disease; but in others no definite rheumatism could be demonstrated. From the similarity of the symptoms in the last group to those in the first group, I have no doubt of their rheumatic nature; but nothing short of the continuous observation which can only be carried out by those engaged in family practice can conclusively settle this point, for the subsequent history of such cases is of equal importance to that preceding the attack. The value of the recognition of the conditions I have named as frequent, or, if you prefer, as occasional, manifestations of rheumatism, lies in this, that they act as "revealing symptoms" indicating diathesis or proclivities; they may be the shadows cast by coming events.

As regards the actual attack of acute rheumatism, there are one or two remarks I should like to make. In the first place, we must be sure of our diagnosis. To many, this, perhaps, does not appear to be a matter of pressing difficulty; and I admit that, in most cases, the recognition of acute rheumatism is an easy matter. Yet I am quite convinced, and others have made the same observation, that first attacks of acute rheumatoid arthritis, or rheumatic gout, are sometimes mistaken for rheumatic fever. I have seen several cases in which I, in common with others, have thought the disease to be rheumatic fever, but the patient has come under observation with a second or subsequent attack, in which the deformity characteristic of rheumatoid arthritis has been present, and persisted. Only last week, I saw a girl, aged 8, who was said to have rheumatic fever. But she had had the joint-affection five or six weeks; it had been remarkably persistent in the affected joints; the wrists had a pulpy feel; there was just perceptible swelling of some of the phalangeal articulations; there had not been the excessive sweating of rheumatic fever (though this is often wanting in children); and lastly, but most important of all, the heart was unaffected. I believe the case to be one of acute rheumatoid arthritis; and the appreciation of this is extremely important, both for its treatment and prognosis.² It is pretty frequently found in cases of pulmonary phthisis that the patient is stated to have had what is called rheumatic fever. That they have had some acute articular disease may be assumed, but that it was genuine acute rheumatism is negatived by the unfrequency with which organic heart-disease is found in such cases, either at the bedside or at the *post mortem* table. Dr. Sutton, who has drawn attention to this point, has further expressed a belief that rheumatoid arthritis and phthisis (and also insanity) are prone to occur in the same families; and thus it is rendered probable that the acute arthritis in these cases of phthisis is of the nature of rheumatoid arthritis, and not rheumatic fever. Dr. Laycock, it will be remembered, described an arthritic tubercular cachexia. On the other hand, it is not to be denied that cases which begin with genuine acute rheumatism, associated with heart-disease, do later pass into a condition of rheumatoid arthritis. Dr. Ord has recorded such a case (BRITISH MEDICAL JOURNAL, 1880, vol. i, p. 158). It is, moreover, believed by Dr. Sutton that there is a form of rheumatoid arthritis with which endocarditis does occur. The relationship of acute rheumatoid arthritis to acute rheumatism is one of the many points requiring further investigation.

In every case that we take to be acute rheumatism, we should carefully examine into the integrity of the nervous system. We are all exceedingly apt to overlook that for which we do not especially look. It has been shown by Ord, Dyce Duckworth, Barlow, and Hadden, that muscular paralysis, anaesthesia, glossy skin, and other changes of a neurotrophic character, are sometimes present in rheumatism. The facts are important, whatever interpretation we put upon them. It is uncertain whether they lend support to the neurotic origin of rheumatism, or whether they should be looked upon as quasi-accidental associations, similar to the nervous disorders occasionally met with in the exanthemata and in febrile diseases. We are all familiar with the vexed question of the arthropathy associated with *tabes dorsalis*. It is not improbable that some cases which appear to be acute rheumatism may really be due to lesions of the spinal cord. I have recorded a case which was sent into the London Hospital as one of rheumatic fever, and which proved to be an affection of the spinal cord, probably acute central myelitis (*Brain*, vol. vi, p. 95).

Passing now to the heart, we want information as to the causation

¹ A valuable piece of therapeutic evidence has been furnished on this point by my friend, Dr. Joseph Hunt (*Lancet*, March 11th, 1882), who shows the marked influence of salicylate of soda in cases of acute tonsillitis.

² The case under observation in the hospital proved a very intractable form of rheumatoid arthritis.

of the heart-disease which is sometimes present before the advent of the first attack of acute rheumatism. In some cases, as suggested by the late Dr. Mahomed, this may originate as a primary dilatation, and the valvular insufficiency may be caused by stretching of the auriculo-ventricular orifice, and a secondary valvulitis. This yielding of the muscular wall of the heart may possibly be due to a latent weakness of the organ connected with the rheumatic diathesis, or, more probably, is brought about by the debilitating influence of anaemia. On the other hand, there can, I think, be no doubt that, in the majority of cases, the heart-disease which is found before the first attack of acute rheumatism, or discovered when the patient comes under care for chorea or some other disease, is really due to a rheumatic endocarditis occurring in childhood, when often the arthritic manifestations are so trivial as to escape attention. Another point, as regards the heart, is the following. Is there any evidence that organic heart-disease develops, or rather manifests itself, subsequently to an attack of acute rheumatism in which no evidence of heart-affection was discoverable? We know that, in chorea, absence of murmur is no proof of absence of endocarditis. We have *post mortem* evidence that vegetations are present on the valves in cases in which no murmur was heard during life. We sometimes find heart-disease in patients who have had rheumatic fever, but whose hearts were not believed to be affected during the attack. Are we justified in assuming, at the end of an attack of acute rheumatism, in which, throughout, there has been no murmur, no pericarditis, and no enlargement of the heart, that our patient's heart has escaped? Can any who have had the opportunities tell us of cases they have watched throughout where there has been no sign of heart-disease during an attack of rheumatic fever, but in which, some weeks or months later, the signs of valvular disease, or of yielding of the heart's chambers, became evident, without, of course, the intermediation of a fresh attack or relapse of the rheumatism, or of severe post-rheumatic anaemia? Farther, we want information as to the proportion of cases in which a murmur, which becomes audible in the course of an attack of acute rheumatism, permanently and completely disappears. We know that murmurs which we assume to be due to endocarditis do disappear, but in hospital practice it is impossible to estimate with what frequency. Does a murmur in such cases ever disappear for a time, and then reappear and become permanent? We want to know, in fact, whether valvulitis undergoes a cure in some cases; and whether, in other cases, a valve which is swollen and gives rise to a murmur, in the course of its subsequent shrinking, ever reaches that stage where apposition occurs and no murmur is heard, before finally reaching that stage of permanent contraction where the segments of the valve can no longer meet, and a permanent murmur results.

A point to which attention has been directed in our inquiry is as to whether any permanent contraction, deformity, or disorganisation of joints, ever occurs as a sequel to an unequivocal attack of rheumatic fever. We all know that, in the overwhelming majority of cases, the joints are left free and supple after the attack; and those who are in the habit of opening and examining the joints of patients after death, agree in telling us that acute rheumatism leaves no structural changes by which we can ascertain its previous existence. We are apt to assume that, if ankylosis or disorganisation of a joint take place, the disease was not rheumatic fever, but pyæmia, or some other disease; just in the same way that, if a patient recover from an attack of tubercular meningitis, most jump to the conclusion that the meningitis was not tubercular. We want information about the joint-troubles which supervene upon acute rheumatism, if such be really the case.

Lastly, a few words with respect to the effects of treatment. The results of treatment ought to be most easily tested by those who see the patient early in the attack, and where the patient remains under the same hygienic conditions throughout. In hospital-practice, the patients are taken from one set of conditions, and placed under quite different ones, always probably a change for the better, even though it entails a certain temporary exposure. Moreover, in hospital-practice, we do not get the patient, as a rule, until the disease has lasted for at least two or three days, often longer, and treatment of some kind has in most cases been previously adopted. The cases, therefore, are not the most suitable to test therapeutic experience. The fact that at least several days have elapsed before the patient comes under treatment in hospital takes away, in a great measure, our power of testing the value of treatment in the matter most important of all, namely, that of preventing heart-disease. It is important to remember that the heart-disease in acute rheumatism, in the great majority of cases, develops very early in the attack, as pointed out particularly by Sir William Gull and Dr. Sutton in their valuable study of the natural history of rheumatism. Let me illustrate this by some

figures. In 1875, when medical registrar to the London Hospital, I carefully examined into this point. Of 133 cases of acute and subacute rheumatism admitted into the wards that year, in only 11 cases, or in 8 per cent., did a murmur make its appearance in patients whose hearts were entirely free from suspicion of disease on admission; and, of these 11, in 3 only did the murmur persist at the time of the patient's discharge; whereas, of the 133 cases, 116 had a murmur on admission. Of 68 first attacks of rheumatic fever (which are the most important test of this point), a murmur was present in 54 when the patient came into the hospital; that is to say, of patients admitted into hospital with first attacks of acute and subacute rheumatism, in 79 per cent. the heart was affected before the efficacy of any treatment could be tested. I think these facts are sufficient to show that the effects of treatment in preventing heart-disease will have to receive an answer by those who see the patient at home, at the very commencement of the attack.

In touching on these points, it must not be concluded that they alone require elucidation. Time prevents my dealing with more. Enough has been said, however, to show how incomplete is our knowledge, and I hope to indicate some directions in which our inquiries should be prosecuted. We ask especially those engaged in family practice to come to our aid; one and all can help. The individual observations and facts require collating, tabulating, and comparing; and, finally, careful inductions must be drawn, which, if each step in the collective investigation be honestly and carefully performed, cannot fail to advance our knowledge and benefit the community. We cannot take the excuse of press of work and want of time. Experience teaches us that it is the busy men who help us most in these inquiries. This is as it should be; the busiest men have the largest experience, and the busiest men are as a rule the best men; for in our profession, as in others, success generally comes to the fittest. But we want the help of all; of the young practitioner, with his few and well-observed cases, and of the old and honoured members of our profession, whose experience would be valuable, if it could only be collected.

THE EARLY TREATMENT OF CONVERGENT STRABISMUS.

Road before the Harveian Society.

By W. ADAMS FROST, F.R.C.S.,

Assistant Ophthalmic Surgeon, and Lecturer on Ophthalmic Surgery, St. George's Hospital; Assistant-Surgeon, Royal Westminster Ophthalmic Hospital.

THIS form of squint is such a common affection, its pathology has been so long known, and its treatment by division of one or both internal recti is so universally recognised, that a communication on the subject of its treatment might seem superfluous. Although, however, all are probably prepared to admit that convergent strabismus in young persons is nearly always the result of hypermetropia, and that it can be cured by division of the internal recti, I do not think that the following propositions meet with so universal an acceptance, although they are equally true, and of even greater importance—namely:

1. That concomitant convergent strabismus seldom recovers spontaneously.
2. That a squint which is constant leads in a few months to great and progressive impairment of the vision of the squinting eye.
3. That the majority of cases, if seen sufficiently early, can be cured by correcting the hypermetropia with glasses.

The two factors which seem to underlie the pathology of the affection are—first, that the acts of accommodation and convergence are associated; and can, therefore, be used together with a less expenditure of nerve-force than is required to use either separately. Secondly, that convergence is independent of the conjugate movements of the eyes.

In the natural condition, accommodation and convergence must go hand-in-hand, if binocular vision is to be maintained. Thus, in looking at a distant object, neither the one nor the other is used; but, in proportion as the object is brought nearer, so is a greater effort of both required, the two always increasing *pari passu*.

In hypermetropia, however (owing to the shortness of the antero-posterior diameter of the eyes), the accommodation is required even for distant objects; and, as the amount employed for this purpose only places the eyes in the condition from which the normal eye starts, it follows that this amount is always required by the hypermetrope in excess of that required by the emmetrope.

I presume we may take it as an accepted physiological law, that acts

which, under normal conditions, are always used together, can be so used with a less expenditure of nerve-force than if they be used separately. That this is the case with regard to the functions we are considering, can be proved experimentally; if an attempt be made to read, through convex lenses, a page of print placed at their focal distance (so that convergence is called into play without accommodation), fatigue is soon experienced; if, on the other hand, a similar experiment be made with a pair of prisms, of suitable strength, placed with their bases inwards (so that accommodation is required without convergence), the result is the same. If, however, the lenses and prisms be combined, so that neither function is required, no discomfort is felt.

The possessor of hypermetropic eyes has, therefore, three courses open to him. 1. He may put forth extra nerve-force, and use the amount of accommodation which is required to correct his hypermetropia, without using any excess of convergence; in this case he will experience fatigue. 2. He may not use the amount of accommodation necessary to correct his error, in which case his vision will be defective. 3. He may use the required amount of accommodation, but, in order to economise nerve-force, use his convergence at the same time; in this case he will acquire a convergent squint. It is with this third condition that we are now alone concerned.

The need for this saving of nerve-force will, of course, be most felt when the available accommodation is most nearly exhausted—that is, in near vision; hence, at first, the child only squints when the attention is directed to a near object. He soon learns, however, that, by squinting, he is enabled to see with less effort, and he then converges when looking attentively at any object. The internal recti, now almost constantly receiving nervous stimulation in excess of their opponents, become hypertrophied, and predominate over them, and the squint, which was occasional, becomes constant.

So far we have considered the squint as a binocular affection, as it really is; that it appears to affect only one eye at a time is the result of our second factor—namely, the independence of convergence and the conjugate movements. The connection of the muscles with the nervous system is such that an impulse can by no possibility be sent to a single muscle, but the two eyes must necessarily act together; thus, both may be turned to the right or left, or both may be turned inwards—convergence. If, now, a certain degree of convergence be produced—say, equal to a rotation inwards of each eye 15° —this will not in any way prevent the eyes being turned to either side while that amount of convergence is maintained; thus, if the eyes be turned to the left, so that the left eye is straight, the right will be rotated inwards 30° ; and, if the right be straight, the left will squint to the same extent.

If the hypermetrope were to allow each eye to be rotated inwards to the same extent, as the result of convergence-impulse sent to the internal recti, the object looked at would not be seen clearly, because its image would not fall upon the yellow-spot in either eye; to avoid this, without altering the amount of convergence used, both eyes are turned to one side, so that the corresponding eye has its visual axis directed to the object, and the other takes up the whole of the squint. If the eye which is directed to the object be covered, the other fixes it, and the covered eye squints. If it be a matter of indifference to the patient which eye he uses, when the covered eye is uncovered the eyes remain in the position which they occupied at the moment, and the squint is then said to be *alternating*. Usually, after a time, the patient develops a preference for using one eye (most frequently the right), and the squint is then said to be fixed in the other. When this is the case, although the working eye can still be made to squint by covering it, and making the other fix an object, yet, directly it is uncovered, the eyes return to their original position.

We may now leave the preliminary part of the subject, and pass to our first proposition, namely, that a convergent concomitant squint seldom recovers spontaneously. The *a priori* probability of this proposition is so great, and it is in such accord with experience, that it would be unnecessary to insist upon it, were it not for the widespread popular belief that children squint from imitation, caprice, naughtiness, or some such cause, and that they may “grow out of it”—a belief that, I am sorry to be obliged to add, is not always discouraged by medical men with that vigour with which all error should be opposed. Children never squint from these causes, and probably could not do so if they tried; and (setting aside a few possible cases of very young infants who develop a temporary convergence from intestinal or cerebral irritation) a squint always means that the subject of it is hypermetropic. A squinting governess, who is not unfrequently held to have produced squinting in her pupils from imitation, might be more correctly looked upon as a test to discriminate between those of her pupils who were and those who were not hypermetropic; for the

former would almost certainly imitate her peculiarity, while the latter would not.

Even useless and injurious habits acquired in childhood are not readily unlearned, but when the habit is formed for a distinct and definite purpose, and the conditions which led to its formation are still present, its abandonment becomes almost an impossibility. But even if it could be shown that spontaneous recovery did occasionally occur, it would be unjustifiable to wait for it if my second proposition be true, namely, that a squint that is fixed in one eye leads in a few months to great and progressive impairment of vision in the affected eye.

To ascertain the frequency with which amblyopia occurs from this cause, I have examined my case-books at St. George's Hospital for the last four years; a large number (41 per cent. of the whole) were too young for the visual acuteness to be estimated, and in a few the notes were imperfect; of the remainder, no fewer than 60.6 per cent. had amblyopia of the squinting eye. It is the occurrence of this amblyopia which renders the advice, which is so often given, to postpone treatment till the child is older, so dangerous. This procrastination leads every year to the loss of hundreds of eyes which ought not to be lost—a result the blame of which often falls on the wrong shoulders, for frequently neither patients nor friends are aware of the defective vision of an eye which no attempt is ever made to use; no sooner, however, has the squint been rectified by an operation, than an interest is taken in the long neglected organ, its powers of vision are tested, it is found with dismay that the eye is practically blind, and the operator is, not unnaturally, blamed for a result which his earlier aid would have prevented.

The amblyopia never comes on while the squint is alternating. I have seen a squint remain in this condition for seven years, with perfect vision in each eye; more rarely, a squint which is not truly alternating does not lead to amblyopia, because the one eye is used for near, and the other for distant vision; but, with these exceptions, a squint probably always leads to amblyopia. Hence the importance of ascertaining whether a squint is alternating or fixed, and of not postponing treatment when it is in the latter condition.

We pass now to our third proposition: that the majority of cases can be cured in the early stage without an operation. Broadly speaking, I should say that cases in which the squint is occasional, and many in which it is alternating, can be cured by correcting the hypermetropia. It is, of course, necessary that the glasses should be worn constantly, and care must be taken that they are so fitted that the patient cannot look over or under them, which children are very apt to do—a sufficiently obvious precaution, but one which is often neglected by the optician. A good indication as to the probability of a cure being effected without an operation, is afforded by the use of atropine. If, when all accommodation is rendered impossible by atropine, the squint diminishes or disappears, it will almost certainly cease when excessive accommodation is rendered unnecessary by correction of the hypermetropia.

Cases of squint naturally divide themselves into those who are, and those who are not, old enough to wear glasses. I think that glasses may with advantage be given much earlier than was the custom till quite recently. I have frequently ordered them for children three and a half years old, and sometimes for even younger ones, and have never seen any reason to regret having done so. When the child can wear glasses, I would order them for every case in which the squint is diminished by atropine; and in others when it is alternating, if it is considered desirable to postpone operating. When glasses cannot be worn, treatment may be postponed as long as the squint is really alternating. As soon as it ceases to be so, it must either be rectified by an operation, or the working eye must be kept covered for an hour or two every day, in order that the other may not become amblyopic. It must be borne in mind that this has no curative effect on the squint, for the covered eye squints behind its shield to the same extent that its companion did; it is merely intended to ward off the amblyopia, or to cure its slighter degrees. When a squint diminishes or disappears under atropine, but the child is too young to wear glasses, it has been proposed to keep the eyes constantly under the influence of a weak solution of atropine; there are, however, two serious objections to this course; in the first place, general toxic symptoms occasionally arise; and, in the second, accidents may easily arise from the defective sight. Especially is this the case among the children of the poor, who are often left to themselves or in the charge of a child not much older.

In conclusion, the points upon which I would particularly insist are the following. 1. The cure of a squint is important, not merely for cosmetic reasons, but because it is essential for binocular vision, and because, if neglected, the sight of the squinting eye will

be very seriously impaired. 2. Treatment must on no account be postponed when the squint has ceased to be alternating. 3. The majority of cases, if seen sufficiently early, can be cured by correcting the hypermetropia.

THE TREATMENT OF PROFUSE HÆMOPTYSIS.

Read before the Medical Society of London.

By SAMUEL WEST, M.D.,

Physician to the Chest Hospital, Victoria Park, etc.

In choosing the treatment of profuse hæmoptysis as a subject to bring before the Society, I was guided by the somewhat personal desire of gathering the experience of others, rather than by the thought that I could import into the subject anything new.

I have endeavoured, in dealing with this question, to state the problem as it presents itself to my mind, and I have avoided speaking of my own experience.

Even though there may be nothing new to say upon them, there may still be, I think, a use in considering now and then subjects of such great practical importance; and though profuse hæmoptysis is not, as diseases go, a very common affection, still it is one upon which everyone should have formed an opinion upon which he is prepared to act, when the occasion arises, for there is then but little time for thinking. I would ask, therefore, that this paper should be regarded rather as an introduction to a discussion, than as an expression of personal views.

Hæmoptysis in phthisis may be slight or severe. Of the severe forms, some are fatal. Of fatal hæmoptysis, we may say that the pathology is established. It is due to the rupture of an aneurysm of the pulmonary artery, or of an eroded vessel; and, although it is conceivable that copious hæmorrhage might occur from the bronchi from many small vessels,—as in the stomach, sometimes, without any lesion gross enough to be recognised by the naked eye,—still such cases are extremely rare. These aneurysms or ulcerated vessels are found in cavities, and hence severe hæmorrhage occurs but rarely, except in chronic forms of phthisis.

All profuse hæmoptysis is not fatal, and we may infer that profuse non-fatal hæmoptysis is due to the same lesions as the fatal, for these reasons: first, that no clinical difference, except in regard of the result, distinguishes those cases which die from those which recover; secondly, that pathological evidence proves, beyond doubt, that both these lesions (namely, aneurysm and erosion of vessels) may cure. The pathology of slight hæmoptysis is very obscure, and must, from the nature of things, remain so. Such cases do not die; but there is good reason for believing that even slight hæmoptysis must be referred to similar, though less extensive, gross disease.

In discussing the treatment of hæmoptysis, we need only consider the cases in which it is profuse, for slight hæmoptysis requires no definite treatment.

The pathology of these more serious forms of hæmoptysis is, as I have said, established. The question we have to discuss, then, is by what means we may best control bleeding from vessels diseased in the way described.

Whatever be the pathology, the general treatment of profuse hæmoptysis must be the same as that of profuse hæmorrhage from other parts of the body. Rest, absolute of the body as a whole, and of the part diseased so far as possible, is the main essential principle; and, with this object, the patient will be kept in the recumbent position, speaking will be prohibited, cough checked, and excitement avoided, or, if present, controlled by drugs. So far as these conditions can be fulfilled by drugs, they will, for the most part, be met by the use of opium, which is as indispensable in most cases of profuse hæmoptysis as in profuse hæmorrhage from other parts.

The action of the so-called hæmostatic remedies is, however, in the face of the present pathological views of hæmoptysis, most difficult to understand. The hæmostatics fall into two groups—the topical astringents, and the vascular constrictants. Chief among the former group are perchloride of iron, alum, gallic and tannic acid, and acetate of lead; but, powerfully as these remedies act when applied directly to the bleeding surface, it is difficult to see how they can produce the same local effect when administered by the mouth. To take perchloride of iron, for example, it is hard to comprehend how a few minims of a dilute solution introduced into the stomach can produce an effect which the undiluted solution can only effect when applied directly to the bleeding surface. If they really do act at all, it must be, like the remedies of the second group, by vascular constriction.

Of this second group, the prominent remedies are digitalis and ergot. Both of these drugs produce contraction of the peripheral arteries; and, if hæmoptysis were due to capillary oozing, they might, by contracting the vessels, cut off the blood from the capillaries, and control the hæmorrhage; but knowing, as we now do, that profuse hæmoptysis is not due to capillary oozing, but to gross lesions of fairly large vessels, the usefulness of these remedies becomes open to grave question.

The effect of constricting the peripheral arteries would be, in great measure, to increase the quantity and pressure of the blood in the arteries of larger size; but, as the lesion is nearly always in them, this is the very place where there should be the least blood and the lowest blood-pressure, and we might expect, *a priori*, that these remedies would increase rather than check hæmorrhage. Digitalis, at any rate, is admitted, I think, now to be of little or no service, unless it be given in doses large enough to make the patient sick; and then its action becomes that of a nauseant and depressant, rather than a stimulant.

The great efficacy of ergot in uterine hæmorrhage probably accounts for its introduction as a remedy for bleeding elsewhere; but ergot acts not only upon the vessels, but also most powerfully upon the muscular tissues of the uterus, in which the vessels run such a course that its contraction leads to their closure by mechanical compression. In the lungs, however, there is no contractile tissue so placed that it can act in this way; and if ergot act upon the smaller vessels only, the same objections arise to its use that have been already urged against the use of digitalis. There is great difference of opinion upon the question of the use of ergot, which it would be unwise to disregard. If ergot act not alone upon the small arteries, but also, as has been suggested, upon the veins, producing dilatation of them and a fall in blood-pressure in consequence, there may be an explanation in this action of the good which follows, as is stated in some cases, the administration of this drug.

In considering the treatment of hæmoptysis, this very important fact has to be borne in mind, namely, that hæmoptysis tends to stop of itself; and it is very difficult to make adequate allowance for this in estimating the value of drugs. If, then, the pathology of hæmoptysis seems to discredit the drugs upon which chief reliance is at present placed, it will be well to consider whether it gives any other more promising indications for treatment.

When hæmorrhage from a surgical wound ceases, the result is due, first, to closure of the vessels by contraction of the muscular coat; and, secondly, to the clotting of the blood, aided, and in the case of large vessels rendered possible only, by the great fall in the blood-pressure which profuse hæmorrhage induces. In the case of profuse hæmoptysis, the muscular coat in the place of hæmorrhage is so diseased that it has lost all power of contraction, and the effect of vascular constrictants acting upon the peripheral branches would be to dam the blood up, and so increase the bleeding. The first indication cannot, therefore, be met. Nor can the second, for there is no drug which can be relied on to increase the clotting power of the blood; for though most of the astringents have this effect when applied directly to the source of hæmorrhage, we have no evidence that they do so when administered by the mouth. Hence we must trust to the intrinsic clotting power of the blood determined in the required place by the lesion of the vessel, and by contact with the surrounding tissues. The third indication we may, however, endeavour to fulfil in various ways by imitating those effects upon blood-pressure and circulation which severe hæmorrhage naturally induces. What is required is, that the circulation in the affected portion of the lungs should be as slow as possible, in order to give the blood time to clot, and that the blood-pressure should be as low as possible, to give the clot formed time to consolidate *in situ*. These effects we may endeavour to obtain by acting upon the vessels, or upon the heart, or upon both combined.

As the chief danger of hæmoptysis is due to local effects of the hæmorrhage, rather than to the loss of blood, and as profuse hæmorrhage leads to the conditions of circulation most favourable to its cessation, it would seem that, in free blood-letting from artery or vein, we possess a means of rapidly inducing the condition we require. Accordingly, copious bleeding was a much vaunted remedy of past times for all severe internal hæmorrhage; and, though probably rightly but little employed in the present day, still it may not be out of place where the dangers are as imminent as in profuse hæmoptysis or in apoplexy, for by no other means can the same rapid and sure result be produced. But without free blood-letting, which can only be available in such very special cases as those just indicated, an attempt may be made to reach the same end, not by removing the blood from the body, but by detaining it in some part of the body distant from the

seat of hæmorrhage. Junod's boot, by gigantic dry cupping, provided a mechanical means of doing this, though it is probable that its action is not simply mechanical, having regard to the advantage which is often derived in the slighter degrees of hæmorrhage or congestion of the lungs and other viscera by the use of a few dry cups. In the vessels of the cutaneous and abdominal systems, we have reservoirs large enough to contain a great portion of the blood of the whole body, and capable, therefore, of producing a considerable effect upon the blood-pressure and circulation, if we possess the means of calling them into action. This may be the explanation of the use of free purgation, for the mere loss of fluid is not sufficient to account for the benefit which frequently results from it. I do not know that diaphoretics have been much employed in hæmoptysis; but it is possible that they might be of advantage in some cases, though, with the exception of pilocarpine, their action is, as a rule, too slow.

In pilocarpine and nitrite of amyl, we have drugs which produce a very considerable vascular dilatation throughout the whole body; and these remedies may be found of service, though I do not think they have been thoroughly tried. Nitrite of amyl is a cardiac stimulant, and its action is very transient, and on both these accounts it would appear to be inferior to pilocarpine.

The objection which might be urged against both of these remedies, namely, that they dilate, not the vessels of the abdomen or of the skin only, but of the whole body, and therefore of the lungs as well, is not, I think, insuperable; but the value of these drugs must be settled by actual observation.

In counterirritation, we have a means of producing vascular dilatation in the skin; but, to be effectual, it would seem necessary that it should be very extensive, unless its action be not mechanical, but reflex.

By the remedies above referred to, an attempt is made to fulfil the conditions indicated upon the circulation by means of the vessels. We may now turn to those which produce the same effect through the heart—to the group of cardiac depressants as well as to those of nauseating or depressant emetics.

Of the cardiac depressants, the only one that is freely used is tartar emetic. This must be administered until marked depression is produced, and it is a remedy, therefore, which requires to be carefully watched. Of the nauseants, ipecacuanha has been highly vaunted. Trousseau used it largely, and praised it highly, quoting Baglivi, who, 150 years before, wrote of it as a specific, "*Radix ipecacuanhe est specifi- cum et quasi infallibile remedium in fluxibus dysentericis aliisque hæmorrhagiis.*" Trousseau was in the habit of giving it even until it produced emesis, and, he stated, with great benefit—although it might be thought, *a priori*, that vomiting was an effect it would be desirable to avoid.

One other method of treatment remains for consideration, to which I venture to think sufficient importance is hardly attached, in writings, at any rate, upon hæmoptysis. I refer to the question of dieting. It is the rule of thumb practice with many physicians in hæmoptysis, to place their patients upon a restricted diet. Knowing the close relation between the lungs and stomach which exists through the nerve-supply, it is not irrational to expect a detrimental effect upon the lungs by an overloaded or irritated stomach, and the harm which experience proves to often revert from these conditions is usually referred to reflex action. But careful dieting is not only requisite because of the risks which improper diet may introduce, but because, when rationally employed, it becomes a real means of active treatment of the diseased condition. The principles of restricted diet advocated by the late Mr. Tufnell for thoracic and abdominal aneurysms, and which often have such signal success, are, with the necessary modifications, equally applicable to the conditions of the lungs upon which profuse hæmoptysis has been shown to depend. The object held in view is to reduce the quantity of blood, while not affecting its quality. This is effected by rigidly restricting the diet to the smallest quantity requisite for the healthy nutrition of the body while at absolute rest, and reducing to the lowest possible limit especially the amount of fluid taken daily. I do not propose to go farther into the details of treatment by dieting. I will only add that, by itself or in combination with drugs, it yields very valuable results.

One of the secondary advantages of this line of treatment seems to be that, while reducing the amount of the blood, it also increases, it appears, when combined with absolute rest, the clotting power of the blood, and, therefore, increases the possibilities of permanent cure.

The principles of treatment which have been considered may be briefly summarised as follows.

1. *Rest* (a) of body generally, (b) of part diseased. Many of the indications under this heading will be met by the use of opium.

2. *Hæmostatics*.—(1) The topical astringents; (2) the vascular con-

stringents. Topical astringents cannot be applied to the bleeding part of the lung, and if they act at all, it must be only as vascular constringents.

The belief as to the use of the vascular constringents in pulmonary hæmorrhage is probably based upon an incorrect theory of the pathology, and reasons have been given why they can be expected to do but little, if any, good. Ergot was probably introduced on account of its efficiency in controlling uterine hæmorrhage; but it is shown that the necessary conditions for its action do not exist in the lungs unless their action be by vascular constriction, and this is probably ineffectual.

As the risk of death in profuse hæmoptysis is not so much from loss of blood as from suffocation, and as profuse hæmorrhage tends to bring about of itself the conditions most favourable to its cessation, an attempt may be made to imitate these conditions in treatment. When a vessel is divided hæmorrhage ceases (1) from contraction of the vessel; (2) from clotting of the blood, aided by the great fall in blood-pressure which severe hæmorrhage induces.

1. *Contraction of the Bleeding Vessel*.—The vessel is so diseased in hæmoptysis, that its muscular coat can no longer act as the diseased spot, and therefore this condition cannot be fulfilled.

2. *Clotting of the Blood*.—There is, so far as I am aware, no drug which, by internal administration, can increase the clotting power of the blood.

The effect upon the blood-pressure can be imitated in various ways.

1. By free blood-letting from artery or vein. This old-fashioned method of treatment is as rational in cases of suffocative hæmoptysis as in apoplexy, for in both cases the risk to life is not from the loss of blood, but from the mechanical effects of the hæmoptysis upon the organ involved.

2. If blood-letting be inapplicable, the same end may be aimed at by detaining the blood in some part of the body other than the part diseased; (a) for example, mechanically, as by the use of Junod's boot, or free dry cupping; (b) or by dilating some of the great vascular systems of the body, and making them act as temporary reservoirs for the blood; (a) the abdominal system (purgation); (b) the cutaneous system; counterirritation; possibly pilocarpin, or even nitrite of amyl. These two drugs dilate the vessels throughout the whole body, and might possibly be of great service. Some objections in theory to their action have been considered.

3. The blood-pressure may be further influenced through the heart: (a) by means of the cardiac depressants, of which antimony is the most reliable; (b) by the nauseating emetics, though their action upon the heart is probably only a part of a more general action. Of the emetics, ipecacuanha was much used by Trousseau.

4. Lastly, dieting is of great importance. The principle of absolute rest and restricted diet, which is the essence of Tufnell's treatment for aneurysm of the thorax and abdomen, is equally applicable and useful in pulmonary hæmoptysis.

Instead of giving a long list of drugs, or discussing in detail the various methods of treatment of hæmoptysis, I have endeavoured in this paper to ascertain the conditions which have to be fulfilled, and to indicate the essential principles which should guide our choice of remedies. I have purposely avoided speaking of my own experience, desiring rather to elicit that of others; and, although some of the lines of treatment are, it is true, open to the objection that they are so far speculations, still it cannot be denied that our present treatment of hæmoptysis is far from satisfactory, and perhaps, from the nature of things, may be doomed to continue so. Speculation may, perhaps, suggest lines of treatment which experiment may follow out, and of the value of which observation alone is the true test.

A CASE OF LONGEVITY.—According to the *Sibirsky Vestnik*, there lives in Irkutsk a man, Grigory Korolkoff, who has reached the age of 120. In 1858, he became a Tomsk burgess, the pertaining official document showing that he had been aged 93 at the time. Mr. Korolkoff is still quite hale and hearty, his sight being in best order.

A CURIOUS BOY.—A correspondent living in Dorpat writes to the *Olevik* (a Livonian periodical) that on October 12th a destitute lad, clad in rags, and looking ill, had been brought to a local almshouse. The lad had lived several years at the town, and had been known to many inhabitants. Two hours later, he was safely delivered of a healthy male infant—"to the greatest surprise of all who knew him," the correspondent gravely adds.

CENTRAL LONDON THROAT AND EAR HOSPITAL.—The winter course of lectures instituted by the medical staff last November, will be resumed on Thursday, January 21st, by Dr. Dundas Grant, assistant-surgeon to the hospital.