In estimating the probable amount of benefit or improvement likely to be derived from the use of the Bath mineral waters in any given case of rheumatism, at least two conditions must be considered, independently of age or sex: first, of *complications*; secondly, *conditions of parts affected locally*.

As to *complications*, those of the head and chest are the most likely to arise. I only give my own private opinion on these conditions. I have an unfavourable opinion of this form of mineral water treatment in cerebral complications suggestive of organic disease, whether it be in rheumatism or any of the diseases usually submitted to such treatment. As regards functional disturbances, such as are frequently produced by rheumatic poison—*e. g.*, headache, vertigo, visual affections, etc.—I believe that they form no bar to the use of the waters, carefully applied, internally or externally. I have in my own private practice, and also in hospital cases, met instances of rheumatism affecting the membranes of the brain, frequently preceded by rheumatism of the scalp, which were permanently relieved by the mineral water treatment. In such cases, the diagnosis is occasionally obscure.

In chest-complications, as we would expect, those of the heart are by far the most frequent. Here, again, the existence of organic or functional disease makes all the difference. Rheumatic poison, as we all know, produces functional cardiac affections generally of the anæmic type; but I have generally found the internal use of the waters, occasionally supplemented by tonic treatment, produce the desired effect. In organic cardiac affections complicating rheumatism, I consider serious questions may arise as to the advisability of such treatment. In a case of decided fatty degeneration, I should certainly not advise baths. In cases of valvular disease, much would depend on the age of the patient. Were he or she over sixty, I might advise the internal use of the waters, but not the baths. I am speaking, of course, of the treatment of the rheumatic, not of the cardiac, affection. Under forty, and certainly under twenty, I do not think the existence of valvular disease need absolutely prevent the careful and judicious administration of the waters sufficiently far to alleviate pain and other symptoms in chronic rheumatism; but I must be understood to say that much depends on degree as to the valvular affection.

As to pulmonary complications, those most frequently met with in rheumatism are bronchitis, asthma, and phthisis. Bronchitis does not appear to me to be so frequently a complication of rheumatism as of gout; nor do I think that the existence of chronic bronchitis predicates the probable inutility of the mineral waters in a given case; neither does the existence of spasmodic asthma. As a rule, the complication with cardiac asthma is, I think, very generally highly unfavourable; but this is scarcely a pulmonary complication. Pulmonary tubercular complications of every kind, and at every stage, I have, in my own experience, found, as a rule, fatal to beneficial results from the use of the waters. There may be exceptions to the rule, but I have not met with them. I can recall to my memory at least three cases in private practice, all females, who came a long distance to Bath for the benefit of the waters, each crippled with rheumatism, having softened tubercle in the lung and constitutional symptoms of advanced disease. Even the occasional use of the baths increased the local lung-irritation, and also as surely aggravated the constitutional symptoms. They had much better have remained at home, and been spared the trial and consequent disappointment of the effect of the mineral waters.*

I now come to prognosis as depending upon local conditions. I think I may assert that muscular rheumatism, independent of articular affections, is the form most favourable for the exhibition of the Bath mineral waters, both internally and externally. I have already pointed out from statistics that cases under twenty are in large proportion, and probably most cases of simple muscular rheumatism are to be met with under that age or under thirty; but, even at much more advanced age, muscular rheumatism, if not entirely curable, is certainly affected by treatment so far as to be made much better. As to articular rheumatism, one must speak much less decidedly. Much depends on the conditions inside and outside the joint affected. True ankylosis (osseous), I believe, scarcely ever exists as a result of rheumatism; but fibrous ankylosis of long standing, particularly if accompanied by thickening of the capsule and loss of all movement of the tendons, connected with flexion, extension, or other movements of the joint, produces results almost quite as unfavourable for treatment by baths as complete ankylosis. The best guide in prognosis in such cases is to take into account, first, the age of the patient; secondly, the duration of the comparative immobility of the joint; thirdly, the amount of mobility which may be effected by the hands of the examiner. The position in which the joint has been allowed to become ankylosed forms also an

important ingredient in the prospects of improvement. I may here mention that a perusal of Mr. Brodhurst's work has firmly impressed me with the idea that there are numerous cases in which surgical interference, supplemented by the use of mineral baths, would be successful in restoring to joints a favourable position and sufficiently useful motion to make an useful joint.

I think my readers will fully understand from the foregoing remarks the conditions which will probably render a cure or even improvement likely to take place in any given case of arthritic rheumatism. Chronic rheumatic arthritis, once developed as a fact, is, as we all well know, a form of disease in which cure is hopeless. I mean when it has arrived at that stage in which there are "eburnation of the cartilages" and locking of the joints by osseous or fibrous deposits around the head of the bone, or rather around the circumference of the joint, if it be an enothrardial joint, as the hip. Nothing can restore the destroyed cartilages or the synovial membrane. Nevertheless, pain may be relieved, and even some slightly increased motion imparted; or, if the disease be recognised and submitted to treatment in its early stages, much may be done to prevent pathological results; and I believe that no therapeutical agents will be found more effectual in promoting this most desirable object than the timely use of thermal mineral waters, such as are to be found in Bath. I have delayed, I fear, too long on the subject of rheumatism; but many remarks which I have made will be equally applicable to other diseases usually treated by the Bath mineral waters, my remarks on which I hope I may be permitted to lay before the readers of the BRITISH MEDICAL JOURNAL on a future occasion.

[To be concluded.]

CLINICAL OBSERVATIONS ON MACULÆ ATROPHICÆ.

By ROBERT LIVEING, M.D., F.R.C.P.

Lecturer on Dermatology in the Middlesex Hospital Medical School.

STRIÆ atrophicæ or linear atrophy is a fairly common affection, and usually takes the form of white stripes or bands from one to several inches in length and about half an inch or more in width, tapering towards each end. They occur in groups, especially about the hips and thighs, and are arranged in more or less parallel curves. Their colour is peculiar, and of a glistening bluish white, resembling mother-of-pearl. The atrophied condition of the skin is more easily felt than seen; to the touch, these streaks appear like furrows depressed below the surface, while the tissues over them seem tense, dry and thin. Microscopical examination shows that this is really the case, for there is a complete atrophy of the papillary layer of the skin, and a great diminution in the vascular and fatty tissues of the part affected.

The *macular* variety of this disease is far more rare than the striated form, but it is of exactly the same nature, and presents greater facilities for studying the different stages of the disease. I have lately had under my observation a patient suffering from maculæ atrophicæ, in whom the period of its duration had extended over six or seven years, and who exhibited a large number of spots in all stages of progress and of ages varying from a few weeks to several years, so that I was well able to observe the disease in its different phases of development. In the case to which I refer, the eruption affected the skin about the upper edge of the sternum, and extended up the neck nearly as high as the cricoid cartilage. It had been developing slowly for several years, fresh spots appearing from time to time, and going through a regular series of changes, which could all be seen and studied at the same time in the same individual.

The *first* stage, which I do not find described by authors, is one which is characterised by slight redness and by well marked hypertrophy rather than atrophy, for the spots are raised above the skin and are hard and fibrous; this enlargement is soon followed by an atrophic change, and we have produced the white appearance which is so characteristic of the *second* stage of the disease. The atrophy at this period is easily seen and more easily felt; on pressing a spot with the finger, the sensation is produced of touching a pit-like scar covered by a thin membrane. The size of the largest spots in the case mentioned did not exceed a threepenny-piece, and most of them were much smaller; they were all discrete and more or less round or oval. The general effect produced by maculæ atrophicæ is very striking and quite unlike that of any other affection. This is due chiefly to the abrupt contrast in colour between the opaque bluish white spots and the surrounding healthy skin. Another point of interest that I was able to observe, in addition to that already mentioned, was a *third* or *final* stage, consisting in the obliteration of some of the oldest

* I have not yet had sufficient experience of the results in Bright's disease to enable me to form a decided opinion.

spots; I cannot say that this obliteration was quite complete, because it was possible, by a close inspection, to see where they had been. They had much shrunk in size, and this shrinking appeared to me to be produced by a kind of lateral compression, as if the surrounding healthy tissues had encroached upon the spots so that they ceased to be a disfigurement. Thus we really have three distinct stages in this curious and rare disease; first, a hypertrophic stage, which is well marked; secondly, an atrophic stage, which is the only one usually described; and thirdly, a stage of contraction or obliteration.

A very interesting question arises as to the nature of these maculae atrophicæ. Kaposi places them among the partial idiopathic atrophies of the cutis, and seems to overlook altogether the early stage of the disease that I have here described. We have no more right to regard them as simple atrophies of the cutis than we have so to regard many forms of scleroderma, unless it be contended that as atrophy is only occasional in scleroderma, and invariable (if it be so) in striæ and maculae atrophicæ, therefore they may with more propriety be regarded as local idiopathic atrophies. Be that as it may, there is a hypertrophic stage, which I have described above, and which precedes any atrophic change whatever, and leads one to look upon this disease as closely allied to scleroderma. This view is confirmed by the fact that striæ atrophicæ have been observed in several instances coincidently with morphea and scleriosis.

THE POSITION OF REST IN FATIGUE AND IN PAIN.*

By THOMAS S. ELLIS, M.R.C.S. Eng.,
Surgeon to the Eye Institution, Gloucester.

THE importance will readily be acknowledged of everything which helps to define the principles on which *rest*—physiological or mechanical—should be applied to the treatment of disease.

Passing by other points (for instance, the discrimination between inactivity too prolonged or too complete, and necessary or desirable rest), it has seemed to me that, as this latter can in many cases be obtained only by completely fixing limbs for the time in one position, it is worthy of inquiry—What is the position in which the various parts of the body are most at rest? and, What are the laws which determine it?

It may, I think, be laid down as a rule that the position which limbs assume, when from any cause in pain, is essentially the same as that which is sooner or later assumed when in a state of rest from fatigue.

A man may "stretch his wearied limbs" for a short time, but he never retains them for long in a stretched position; by-and-bye they are drawn into one similar to that assumed when in pain, as a rule immediately on the pain accruing; for although, on a blow being struck over a muscle, the first movement may be to put that muscle in a state of relaxation, this is temporary only. Sooner or later, a limb in pain, as in fatigue, seeks what I call the *position of rest*—more marked in joint-disease, and especially in disease of the hip, but essentially the same. Briefly, it may be said that the arm falls to the side, the forearm is flexed and pronated, the hand in a line with it, and the fingers bent, but the index and thumb less so than the others; the thigh is flexed and rotated inwards, the knee bent, the foot tending towards the line of the leg, the great toe a little extended, the others rather contracted; and these rules hold good for parts of limbs left after amputation, at whatever point divided.

It has appeared to me probable that these constant positions, set up on the operation of different exciting causes, would have an explanation common to all. None that I know of has been given, as far as relates to pain generally, or to fatigue; but Mr. Hilton thus explains why an inflamed joint is flexed: "That the irritated or inflamed condition of the interior of the joint, involving the whole of the *articular nerves*, excites a corresponding condition of irritation in the same nervous trunks which supply both sets of muscles, extensors and flexors; but that the flexors, by virtue of their superior strength, compel the limb to obey them, and so force the joint into its flexed condition." (*Rest and Pain*, page 150.) Further on (page 157), he explains an exception in the case of the wrist, which when inflamed is straight, "in consequence of the flexing and extending forces being pretty equally balanced". So also the relatively less flexion of the thumb and forefinger, when the joints are inflamed, as compared to the other fingers. But this, even if otherwise satisfactory, involves no explanation of the position, when assumed as a consequence of pain from causes other than joint disease, or as a relief to fatigue; and I presume to offer an opinion that the real explanation of *all* is simply this:

That the limbs seek a position in which the parts of them are in a state of least tension and of least pressure; as skin and muscle have a property of ready adjustment, while ligaments have not, so, to obtain this, which I call the position of rest, it is necessary that the joints should be in such position that none of the ligaments are on the stretch, that is, somewhere about a mean between the ranges of easy motion.

This, then, I believe to be the principal element in determining the position of rest, though no doubt it is essential, too, that the articular surfaces should be in the position best adapted for each other. To me it is a readier explanation of the difference between the wrist and the elbow or knee; the wrist allows flexion and extension beyond the straight line; the position of rest, therefore, is neither, or straight: while the elbow and knee range in motion between extreme flexion and the straight line; the position of rest, therefore, is moderate flexion. It accords, too, with my statement that the position, essentially the same as in pain from other causes and in fatigue, is more marked in joint-disease.

To object, as I have heard done, that the ligaments are not sensitive, seems to me like arguing that to pull the hair of the scalp cannot cause irritation, because hair has no sensibility. Tension of any structure is never long tolerated.

Mr. Hilton (*op. cit.*, page 150) lays great stress upon the anatomical fact that "The same trunks of nerves, whose branches supply the groups of muscles moving a joint, furnish also a distribution of nerves to the skin over the insertions of the same muscles; and, what at this moment more especially merits our attention, the interior of the joint receives its nerves from the same source." On this he remarks: "This implies an accurate and consentaneous physiological harmony in these various co-operating structures." Exactly so, but this is just what I insist on; the muscles, as the motor agency—some contracting, some relaxing—put the limb in a state of rest, a state of the least tension, which I would suggest as more like an "accurate consentaneous physiological harmony between co-operating structures" than would be the excitement of an unequal contest between flexor and extensor muscles, leading to the inevitable victory of the one, and inducing a position which, Mr. Hilton says, ought at once to be changed. I am referring to the first passage I quoted (*ante*) and also to page 155, where he lays it down, as seeming to him, "that it is a great mistake on the part of professional men, when they see an inflamed knee or other joint beginning to be contracted or flexed, not to correct it at once".

On page 153, the "mischievous assiduity" of the flexors is spoken of; and again (page 152): "The muscles, indeed, appear to be told, through the medium of the nerves of the interior of the joint, that its articular surfaces are overtaxed; and, the antagonistic muscular forces of the joint being involuntarily excited, the joint is at once rendered rigid and stiff, for the purpose of keeping it at rest."

Let us now inquire what is the effect of irritation of a portion of skin upon muscles which are supplied by nerves from the same trunk as those supplying the skin. The musculo-spiral, which is an example given by Mr. Hilton, is a good test. Let the outside of the forearm be bitten by a dog, or torn, or burnt: does the injury excite contraction of the supinator-extensor group of muscles which are alone supplied from the same nerve? Do we see patients, under such circumstances, with the arm in any other position than that which I call the position of rest? I think not. I can understand this on the principle of "harmony between co-operating structures", but not on the ground of the pronator-flexor group being the stronger. In the first place, there is nothing in the injury to excite these muscles to action; secondly, I cannot see how this group can be the stronger, unless we are to believe that a carpenter would drive a screw with greater force if the thread were reversed.

Again, if there have been an amputation below the elbow, the stump is drawn up, flexed. Is this because the biceps and brachialis anticus are stronger than the triceps? I do not think they are stronger; but further, the biceps is a powerful supinator, uninjured by the operation, yet the stump is always prone rather than supine.

It does so happen that there is one flexor muscle which, beyond all question, is stronger than its corresponding extensor: the flexor longus pollicis pedis; yet the great toe, at rest, or when a joint is inflamed, is always extended, never flexed. Moreover, to say that flexors are always stronger than extensors, is directly opposed to the teaching of Carpenter's *Physiology*, that "the extensors of the leg upon the thigh are much more powerful than the flexors, a character which is peculiar to man". We know the enormous strength of the extensor cruris in voluntary and involuntary action; but, upon what facts can it be asserted that the flexors are even stronger? I know of none.

I have felt it to be no light matter to differ from one of the great masters of surgery on a subject which he has made his own; but the opinions, which, to me, have at present the force of convictions, I am bound to express with no uncertain sound, or it would be vain to hope

* Read before the Gloucestershire Branch.