UNUNITED FRACTURE OF THE FOREARM, WITH DEFICIENCY OF THE ULNA, TREATED SUCCESSFULLY BY EXCISION AND THE WIRE SUTURE.

By THOMAS ANNANDALE, F.R.S.E.,

Surgeon to the Edinburgh Royal Infirmary and Lecturer on Clinical Surgery.

R. K., aged 29, was admitted into my wards on June 24th, 1873, suffering from an ununited fracture of the bones of the forearm. About six months before his admission, his forearm had been severely injured by machinery. Both bones were fractured, and a large lacerated wound was caused by the accident. He was taken to a provincial hospital, and carefully treated for several months. About three months after the accident, a large piece of bone (a portion of the ulna) gradually loosened, and was removed. Three weeks after this, the wound was healed, but the bones had not united properly.

When the arm was examined, a large cicatrix was noticed over the middle third of the bones of the forearm; it was adherent to the ulna for a short distance, but was otherwise free. Both bones were movable at the junction of their middle and lower thirds, but the radius less so than the ulna. The ulna was not only quite ununited, but was deficient for about one inch at the seat of fracture, the result, no doubt, of the necrosis which had followed the injury. The fractured ends of the ulna were displaced towards, and adherent to, the radius. Pronation and supination could not be performed, and the arm was also weak, and, in consequence, useless.

On June 27th, I performed the following operation, with the hope of making the arm more useful. An incision, about three inches long, was made over the dorsal aspect of the ulna, so as to expose the fractured portion of this bone. It was then found that the fractured ends were rounded off and atrophied, and united to one another and to the radius by some strong fibrous texture. These ends were also displaced inwards, and there was fully an interval of an inch between them owing to the deficiency of the bone.

The condition of the bones is illustrated diagrammatically in Fig. 1, the dotted lines at A A and B B showing the amount of bone removed from the radius and ulna at the operation.

Fig. 1.

Fig. 2.

About a quarter of an inch was now sawn off the ends of the ulna; and, as it was quite evident that these ends could not be brought together, a second incision was made over the dorsal aspect of the radius, and

a portion of this bone, including the partially united part, was also sawn off. By thus shortening the radius to a sufficient extent, the ends of the ulna were allowed to meet, the adhesions connecting them to the former bone having been divided. The ends of both bones were then drilled, and secured with strong silver wire, as shown in Fig. 2. The edges of the wounds being brought together with a few carbolised silk sutures, antiseptic muslin was applied in the usual way, and the arm adjusted on a splint. On the 3rd of July, it is noted that the patient has progressed favourably since the operation, and the wound is healing well. On the 8th of July, the wire through the ends of the ulna being a little loose, was twisted more firmly. The patient's progress continues good.

On the 3rd of August, the wounds were quite superficial, and the wire through the ends of the radius being quite loose, was removed. On the 13th of August, the wire was removed from the ulna; and on the 29th, the patient left the hospital, the wounds being almost healed.

Six weeks after this, he returned to show himself, when it was found that the bones were firmly united. The forearm, to a limited extent, could be pronated and supinated; but these and the other movements of the arm were steadily improving, and the limb could already be used in many ways, its strength being greatly improved since the operation.

Remarks.—For the successful treatment of this case, it was necessary to overcome two principal obstacles. These were: (1) the deficiency of the ulna; (2) the displacement inwards of the ends of the ulna, and their adhesion to the radius. In addition, the large cicatrix forming the chief covering of soft parts over the injured bones made operative interference more difficult than if these coverings had been sound. The first of these obstacles was successfully overcome by removing a portion of the radius, so as to allow the ends of both bones to be brought together. The removal of this portion of bone by diminishing the amount of the osseous element of the forearm also permitted the contraction of the wounds in the soft parts to take place satisfactorily. The second obstacle was successfully combated by dividing the adhesions, drilling the ends of both bones, and securing them with strong wire, as shown in Fig. 2.

This method of securing the fractured ends would, I believe, prove very valuable in many cases of recent compound fracture of the bones of the forearm. It is a most efficient means of preventing their inward displacement, and therefore assists much in preventing also the union of the radius and ulna to one another, a condition not easy to overcome in this class of injury. The wire which I employ in this and other operations of the kind is silver, of the thickness of that usually employed to secure the corks of soda-water bottles. The instrument used for drilling the bone is a joiner's common small pricker. Having tried more complicated instruments for this purpose, I have now a decided preference for the more simple tool, which I always find to be most efficient.

MALARIA: REPLY TO DR. INMAN.

By W. C. MACLEAN, C.B., M.D., Surgeon-General,
Professor of Military Medicine in the Army Medical School at Netley.

ALTHOUGH I have no intention of entering into a controversy with every member of the profession who happens to believe there is no such entity as malaria, I must ask to be allowed space for a few words on the not very courteous article of Dr. Inman.

1. The candid confession with which his letter opens, that he has no personal experience of the subject on which he writes so confidently, was hardly necessary; the fact is sufficiently apparent in his letter.

2. I beg to say that I did not "attack" Dr. Oldham. This gentleman published a book, in which he put forth certain opinions, the accuracy and scientific value of which I have called in question: a very different thing from "attacking" their author.

3. When Dr. Inman, in effect, asserts that I have not read his friend's book, he is in error. Dr. Oldham was kind enough to send me a copy of his work when it issued from the press, when I not only read it with care, but, without loss of time, communicated to its author, as well as I could within the limits of a letter, the grounds on which I differed from his conclusions.

4. Dr. Inman has evidently no knowledge of what I have elsewhere written on malarious fevers, if he supposes that I am ignorant of the notorious fact that paroxysmal fevers are seen in some localities where there are no marshes, and in ships at sea. In the first instance, water is often found at no great distance from the surface, and the apparently barren soil has been found to abound in organic matter; to the above conditions add high temperature, and we have the factors needful for the genesis of the poison of malaria. In other cases, where such fevers have been found to prevail on rocky places with scanty or no

vegetation, it has often been found that the rock consists of granite in process of disintegration, in common language, "rotten granite", holding an immense amount of moisture, and largely charged, as Friedell has shown in the case of Hong-Kong, with fungi, which, with the addition of high temperature, give, when exposed to the air, the conditions required for the generation of malaria. In yet a third class of cases, it has again and again been found that the cause of the fever has been traced to the condition of the drinking water in wells left uncleansed for ages, and charged with malaria, the result of the decomposition of organic matter accumulated in them. In every known case of malarial fever occurring in ships at sea, the cause of which has been investigated by competent persons, it has been traced to one of three causes: (a) either recent charging of the systems of the victims with malaria, by residence in a malarious locality, as is seen every day in our invalids returning from India; or (b), the genesis of malaria in the holds of ships by the decomposition of vegetable matter, e.g., chips left by ship-builders, acted on by bilge-water and high temperature; or (c), a still more frequent cause, the issuing to the crews of drinking-water taken from places notoriously malarious, as in the famous example carefully recorded by Boudin. If chill after exertion could generate a genuine paroxysmal fever with its sequels, this class of fevers ought to prevail as a common disease among the maritime population of all countries, which is not the case.

5. Dr. Inman dismisses with a sneer the examples I gave in my critical notice of Dr. Munro's paper, of malarial fevers following immediately on disturbance of the soil in Hong-Kong and Paris. But the facts cannot be sneered away. It was not the workmen engaged in the above operations, heated by day and chilled by night, who alone suffered, but the population exposed to the emanations from the soil, as recorded by many observers quite as competent to note natural phenomena as either Drs. Oldham or Inman.

At Hong-Kong, at the time indicated by me, four officers of Artillery, of whom my friend Major-General Barrow, C.B., is now the sole survivor, occupied a bungalow at a considerable height above the level of the hill-side on which excavations for building were being carried on. They enjoyed perfect immunity from the fever which prevailed lower down. At last, a young and inexperienced officer insisted on scarping away part of the hill-side, to give more room in rear of his own bedroom, thus exposing a large slice of soil to the air. The result was an attack of remittent fever that cost him his life, with the dangerous illness of two of his brother officers from the same cause; and the immediate abandonment of the house was rendered doubly necessary from the prostration of most of the servants, who, up to this time, had escaped. Will a reference to the stale joke about "Tenterden steeple and the Goodwin Sands", explain such a fact as the above? There is not a coffee-planter in India who could not give Dr. Inman many such examples from his own experience.

6. Dr. Inman justly enough points to the fact that chills cause bronchitis and pneumonia, and, as in the case of the servant-girl mentioned by him, congestion of the kidneys, and he might have added, rheumatism in all its degrees. Who disputes so familiar a fact, known to every adult person in this realm? What Dr. Inman fails to see is that, when men die from any of the above causes, we do not see in their organs after death the changes observed with an uniformity as remarkable as the phenomena of the disease itself, in those who perish from malarial fevers—the diseased blood, the pigmented organs, the diseased spleens and livers, the tinted skins, in a word, the whole train of morbid appearances characteristic of the malarial cachexy. If Dr. Inman, instead of taking his notions on malarial diseases at second hand, will pay us a visit at Netley in the coming summer, it will give me pleasure (as much pleasure, at least, as can be derived from the contemplation of so sad a sight) to show him, by the hundred, men with the stamp of malaria on them so plainly marked that those who run may read; and on the shelves of the museum the results of the action of an agent, that, call it by what name we please, works on the organs of man, in a way not explicable by the "chill" theory of Dr. Oldham or his supporters.

THE TREATMENT OF ACUTE RHEUMATISM.*

By THOMAS S. DOWSE, M.D., F.R.C.P.Ed.; Medical Superintendent of the Central London Sick Asylum, Highgate.

As a mere humble clinical observer, I should not have ventured to trespass upon the valuable time of this Association, if I did not feel that, in the relief of suffering humanity, as far as our honest endeavours extend, we are each and all of us upon one common level. The

* Read before the Medical Section at the Annual Meeting of the British Medical Association at Norwich, August 1874.

end and aim of medical science is not to record an age of special originality, unless it have a direct bearing upon an improved treatment in the art of healing, and the simplest paths of learning in this respect must not be ignored. The rapid advances which have been made of late years to throw a flood of light upon a hitherto darkened spectrum, must claim at all times our warmest approbation and most careful scrutiny. Yet there are some, and by no means unessential, points in the treatment of diseased states which no aids at our command can reveal, save the practised eye and careful observation of the painstaking practitioner. In the few remarks which I am about to make to you, I shall not allude to any obscure theories, but I shall confine myself strictly to the practical part of my subject. The essential nature of my treatment of acute rheumatism is both eliminative and pyretic. I believe it is usually admitted that the *materies morbi* circulating throughout the entire body in this disease is of an acid character—lactic, if you will; and, although there might be some doubt as to whether this be correct or not, still it is quite certain that we have a condition to deal with in which acidity is the most prominent feature. I am not aware that its causation has been explained in any more definite way than that it is in all probability the product of malassimilation producing faulty tissue-metamorphosis. The vagueness of such a definition as this must at once be admitted. It is quite certain that especial individuals are prone under fixed laws to the generation of this rheumatic factor; that the like individuals are especially subject to a nervous condition known as chorea, and that these alternate in the same person is a matter of daily observation; but the relation which this has to the etiology of acute rheumatism, on the one hand, and chorea, on the other, has never to my mind been explained.

It is the rule (not, however, without its exceptions) that the poison of rheumatic fever is generated in an especial type of constitution, and characterised by a definite train of symptoms and signs which are well known, and need not be here enumerated. In addition to these, however, we have fibro-serous inflammations and muco-congestive complications.

It would be useless for me, in the short space of time allotted to the reading of this paper, to attempt to describe fully the various opinions which have from time to time been put forward in reference to the treatment of this disease; but, under all circumstances, the chief point of clinical interest to the physician must be to prevent inflammation of the heart and its membranes. For some time, the alkaline treatment so much advocated by the late Dr. Fuller held unbounded sway, and the admirable tables drawn up by Dr. Dickinson in 1869 prove most incontrovertibly that alkalies have the power to prevent endocardial and pericardial inflammation. In this year, Sir William Gull and Dr. Sutton somewhat startled the profession by bringing forward a number of cases successfully treated by mint-water; in other words, by adopting careful regimen, mechanical and physiological rest, and allaying pain by opiates. Under this treatment, the average duration of the acute symptoms was seventeen days. Some years ago, when I was registrar at Charing Cross Hospital, the treatment of this disease occupied my serious attention. Dr. Salter gave three grains of quinine every three hours, whilst Dr. Headland gave alkalies. I then found that the course of the disease was cut short by the quinine, but that the tendency to heart-complication was greater than when it was treated by alkalies. From a careful survey of the various modes adopted in the treatment of acute rheumatism, including vapour-baths, lemon-juice, tincture of iron, aconite, hydrochlorate of trimethylamin, blistering, and others, I am inclined to think that its progress can be cut short by the means which I shall immediately explain to you, and that cardiac inflammations can be avoided. I am right in stating it to be an admitted rule that, if valvular disease of the heart or pericarditis supervene upon an attack of rheumatic fever, it does so within the first week or ten days. This is to a certain extent undoubtedly correct. I am one of the few who believe that inflammation of the endocardium is a constant attendant of the acute rheumatic state, not giving rise in many cases to valvular lesion at the time, but predisposing to subsequent change, which reveals itself under circumstances apparently the least provocative. It is quite true that in acute rheumatism no absolute *bruit* referred to the sounds of the heart can, in the majority of instances, be detected, and at the base the sounds will be clear and natural in rhythm, but at the apex they will be attended by a characteristic turbulence and commingling, which, with the hurried action of the heart and feeling of oppression in the epigastrium, will be indicative of hyperemia of the mitral valves.

Thus it seems to me that, in the treatment of rheumatic fever, we have, first, to consider the best way to eliminate the acid products of the diseased state; secondly, to relieve pain. Beyond this, the case can be treated as an ordinary one of functional glandular derangement, or febricula; for, when once the sour secretion from the skin is