have been *well defined*; and the probability exists that neither judgment could have been correct.

Both, however, concur in the *existence of fracture*, but as the *altered form* of the shoulder is to be accounted for on a different hypothesis, and as the two are not agreed as to the *nature and extent* of the injury which the bone has received, the only *SENSIBLE sign of fracture on which they ARE agreed, is the presence of crepitation*. But this of itself is insufficient to establish the existence of fracture, because fracture may exist *without it*, if the fragments are not in contact with each other, and if there are no inequalities in the new surfaces; and therefore crepitation might not have existed in the present case, where there is supposed to have been a *fissure* in the split bone, and the *shaft* of the humerus to have been pulled much inwards and upwards by muscular action. And again, crepitation may exist *without fracture*,—some inflammatory or undefined alteration in joints, and their neighbouring bursæ and sheaths of tendons, may occasion a similar phenomenon, so that this is not to be relied upon.

This is still farther strengthened by the supposed condition of the parts, a fortnight afterwards, when it appears that the fissure was lessened, crepitation was gone, and union, though it might be ligamentous had commenced:—a memorable instance of the influence of preconceived views, even upon enlightened and scientific minds, and strong corroborative testimony to the non-existence of fracture. For if fracture had existed, it must have been occasioned by *violence or disorganization*. We have already shown that it could not have been the result of the former; but admitting that it *was so*—that *union had commenced*, and *advanced considerably* towards the third week in August, how account for the development and unwearied, unchecked progress of malignant action, from the 30th of that month? Was this the result of the application of splints, bandages, &c.? **ASSUREDLY NOT**: and if not, we conclude that malignant action *existed beforehand*, though in a less marked degree; but *if this were the case*, the fracture was not from violence, the advancing union existing only in the previously biased opinions of the surgeons.

The *production* of this *previously biased opinion* is most easily explained—the *first impression* on seeing the shoulder, was fracture of the neck; the mischievous changes in the neighbourhood from malignant inflammation *gave the crepitation*. A healthy humerus in a thin muscular subject, gives, on examination with the fingers, quite the impression of a longitudinal sulcus or groove running along its interior edge, and these things, added to the apparent change of form, were confirmation strong of fracture.

But we must take the other position, that fracture existed as a consequence of cancerous or malignant disorganization. That it afterwards existed has been *demonstrated*; but if this disease had proceeded to such an extent in the beginning of August, how came it to pass that the work of *reparation* had so rapidly *advanced* on the third week? It is easy to understand that crepitation might have been lessened by advancing ulceration and absorption; but *then* there would have been a corresponding increase of mobility, and greater separation of the head from the shaft, instead of, as is reported, greater firmness and advancing union. On the whole, therefore, we must conclude that the bone was not fractured.

V. Lastly, the only other practical point, which remains to be examined, is—if the disease and its nature had been detected earlier, might it have been removed? and could anything have been further done to arrest and subdue this morbid action?

To the first question I am disposed to reply, that it has ever been my opinion, that if malignant disease in a part, could be removed before the constitution had participated in the malignant action, it might be

effectually isolated. Numerous cases have occurred as proofs of this position during a practice of more than thirty years. But it is a mistake to suppose that the absorbent vessels and glands, are the only agents in conveying this poison to different parts of the system. The law of symmetry, or that which connects the sympathies of organs by means of nervous influence, as for instance, the eyes, mammae, testes, hands, feet, &c., operates as a powerful agent in developing morbid sympathy. But a still more frequent and often overlooked agent is the blood. The rudiments of malignant action are conveyed into the general circulating system by the poisoned blood of the diseased part, and as they traverse every part of the animal economy, are deposited wherever they find an agreeable nidus, and form the nuclei of fresh morbid action. I need scarcely say how beautifully this law was exhibited in the case now recorded, by the *universality* of a similar morbid action.

So much for the general principle; but with regard to its application to the present case, it is very manifest that no operation could have been of any avail, unless at an early stage of the disease, because isolation of the morbid structures would have been impossible. But do we possess any means of distinguishing in its *early stage*, malignant action from simple periosteal inflammation—from death of the bone, or other diseases which admit of ultimate relief either by the agency of nature, or by the skill of the surgeon, without having recourse to the loss of an entire limb? I think *not*: and *if not*, who is there among us, who will peril the life of a patient, and subject him to a fearful mutilation, upon the *possibility* of malignant action? Hence it follows that an operation at an early period would have been unjustifiable,—that at a *late* period it must have been useless, and therefore wickedly cruel.

But could anything have been done that was not done, with a fair prospect of arresting the morbid action? I confess that I am not aware of any such means, but I leave this question to be answered by the profession.

W. NEWNHAM.

Farnham, Feb. 29, 1844.

ON THE EMPLOYMENT OF DIGITALIS DURING THE PREMONITORY SYMPTOMS OF STHENIC EPILEPSY.

BY R. WAKEFIELD SCOTT, M.D., LIVERPOOL.

It is now some time since I was led accidentally to the employment of digitalis in epilepsy, in a mode which I think has not been usually adopted, and from the success which attended it, I was induced to publish an account of it in the *Edinburgh Medical and Surgical Journal*. (No. 90., p. 19.)

I have subsequently employed it with equal advantage in several other cases; and it was also adopted at my suggestion, on rather an extensive scale, and with very beneficial results, on maniacal epileptics, by the late Mr. Davidson, Surgeon to the Lancaster Lunatic Asylum.

The case to which I have alluded, was that of a boy aged 13, almost an idiot, of a sanguine and irritable habit, who for several years had been subject to epilepsy, and at the period when he came under my care the attacks amounted to from 12 to 20 daily, of more or less intensity, and were invariably preceded by convulsive muscular motions, suffusion of the face, and other signs of increased vascular action.

In administering the tincture of digitalis, his mother observed that when she gave it, after the twitchings,

as she termed them, had come on, they were very much diminished, and by repeating the dose, sometimes ceased, without any paroxysm following.

She was therefore directed to administer it only at such times, and in such quantities as might be sufficient to allay the twitchings. By following this plan she was enabled to arrest the paroxysms for a considerable time, and they only occurred when the medicine was omitted, or when she was afraid of giving it in a sufficient quantity; and when I lost sight of him, notwithstanding the discontinuance of the medicine, he had been perfectly free from an attack for four months, whilst no other remedies of any kind had been employed.

From 20 to 30 minims were generally sufficient, but on one occasion 60 were given before the twitchings subsided. No bad effects ever followed the administration of these large doses, and it would seem that the constitutional excitement and sedative influence of the remedy were neutralized by their conflicting agency. It has been stated by Georget, that premonitory symptoms do not occur in more than five cases in a hundred,* but according to the statistical observations of M. Beau, they are observed in about half the attacks which take place;† and in my own experience, the latter is by far the most correct statement, though in many instances they are very transient, and do not admit of the employment of any counteracting measure, though they are often sufficiently prolonged to allow their application.

In the sthenic species of epilepsy, the premonitory symptoms which have come under my observation, have usually been those of nervous and vascular excitement, gradually increasing until the cerebro-spinal congestion has been sufficient to produce the paroxysm; and it seemed reasonable to suppose, that if the excitement could be allayed, the paroxysm might be arrested, and by continued prevention, the disease might be eventually removed, provided it was not dependent upon organic causes. This has been effected in so many instances, by the instrumentality of digitalis, without detriment to the powers of the constitution, that I cannot but think, that it presents a valuable resource, and is deserving of a more extended trial in similar cases.

In consequence of having described this mode of treatment in my lectures, I have been recently favoured with the following case, in confirmation of its utility, by an intelligent pupil, Mr. Eccleston, who is connected with the Eastern Dispensary, which may perhaps be thought not unworthy of insertion.

"Owen Jones, a lad, aged 12, of rather an idiotic expression of countenance, and residing in Birkett Street, came under my care on the 29th of January last. He had been for two years the subject of epileptic attacks, but these did not occur so frequently as to excite much alarm in the minds of his parents. I learnt that from last October up to the time I first saw him, the fits had increased in frequency, and then occurred every 15 or 20 minutes. These attacks were always preceded by increased action of the heart, which acted as a warning to his friends of the coming attack.

"When first seen he was ordered five grains of calomel and five grains of compound scammony powder, in order to remove any source of irritation that might exist in the intestines. After that we tried digitalis on Dr. Scott's plan, regulating the dose by the effect on the heart's action. He took five minims at the commencement, with good effect; his largest dose was half a teaspoonful, or about 30 minims. He rapidly improved, and after taking it a fortnight, the fits entirely left him, and he has had no return of them up to the present time."

Liverpool, March 13, 1844.

* Dict. de Med.: Art. "Epilepsie."

† Arch. Gen. de Med. Tome 2,

ON MALIGNANT PUERPERAL FEVER.

TO THE EDITOR OF THE PROVINCIAL MEDICAL AND SURGICAL JOURNAL.

SIR,

In compliance with the request contained in your circular, I venture to send you a few hastily drawn up remarks on Puerperal Fever, of which I have seen more cases during the last year and a half than I had done in forty years previous practice. The treatment recommended is the only one which I have seen at all successful. So much has been written on this subject, that I do not know whether or not you will think the accompanying observations at all worth inserting in the *Provincial Medical and Surgical Journal*. If you do, they are much at your service.

I have the honour to be, Sir,

Your obedient servant,

W. HENCHMAN CROWFOOT,

Fellow of the Royal College of Surgeons.

Beccles, March 20, 1844.

The interesting cases of Malignant Puerperal Fever, which have lately been published by Mr. Storr and Mr. Fisher, in the *Provincial Medical Journal*, cannot fail to have painfully called the attention of the profession to the uncertainty which prevails, amongst writers and practitioners, as to the nature and treatment of this pre-eminently fatal disease.

Some authors consider Puerperal Fever as purely inflammatory, non-contagious, and almost certainly controllable by the free use of the lancet; whilst others believe it to be highly contagious, class it with typhus, and place all their hopes of cure on supporting the depressed powers of the system.

As it is important that the medical attendant should have some principle on which to found his treatment, under the appalling circumstances which accompany the prevalence of this most formidable disorder, it may not be unprofitable to take a hasty survey of such facts as have been recorded, and of such opinions as have been formed by the most cautious and experienced observers of the disease, and to examine the bearing they have on the above questions.

The contagious character of malignant Puerperal Fever is, perhaps, better established by such cases as those of Mr. Storr and Mr. Fisher, which were scattered over a considerable extent of country, than by such as have been concentrated in hospitals, or large towns, where the patients may have been exposed to some local and common source of disease. The evidence afforded by the above cases would be sufficient to induce every conscientious practitioner, to be very cautious how he hazarded the extension of so fatal a malady, by allowing himself to become the medium of its communication, even if it were unsupported by the testimony of the great majority of those who have written on the subject. Mr. Fisher proved that the most careful ablation, and the most entire change of raiment, were not sufficient to prevent his conveying the poison from one patient to another, and that nothing but a temporary retirement from the practice of midwifery was a security against his continuing to extend the disorder.

Puerperal Fever appears to have been known and described by the Greek and Arabian physicians; but, according to Dr. Hulme, Dr. Strother was the first to designate the disease by that name, in his *Criticon Febrium*, published in 1716. Dr. Strother, however, says but little about it, and evidently considered it to depend on uterine inflammation.

Dr. Hulme, in his treatise on Puerperal Fever, published in 1772, has given the histories of seven fatal cases, with their post-mortem examinations; in all he found the results of extensive inflammation, such as