CASE OF MALIGNANT DISEASE OF THE HUMERUS.

By W. NEWNHAM, ESQ., FARNHAM.

The subject of the following observations was in his 17th year—was a youth of considerable talent and most amiable manifestations of mind; he had distinguished himself at Harrow, and was equally admired and beloved by those who knew him.

Generally speaking, he was a very healthy person, but when he returned home from Harrow, at the Easter vacation of 1843, he was out of health, and complained of headache, and of inability to fix his attention upon his usual objects of study, without bringing on pain of the forehead, and general distress of the nervous system. His pulse was rather feeble, but not quickened; and the tongue was not loaded, but whitish; in fact, a cerebral tongue; and it seemed evident that the brain had been overworked. The principles of treatment were very clear, viz., to moderate the work of the brain, and to rouse its energies, so as to bear with impunity the ordinary studious habits of an aspiring and talented young man. He did not return to Harrow after the recess, but remained under my care till I could conscientiously give him his "exeat" from home, and allow him to resume his scholastic duties.

Previously to his return, he consulted me one day in May last, for an affection of the upper arm, which he told me he had hurt while playing with his sisters. I confess I thought very little of this injury, believed it to be an undue stretching, or, perhaps, rupture of a few of the fibres of the deltoid, and trusting that quiet and patience would soon make all right again, I prescribed some liniment, and thought I should hear no more of it.

He returned to Harrow, whence I continued to receive good accounts of him from time to time. Again in August he came home, and his anxious friends were immediately struck by his not looking well; by the powerlessness of his arm; by the pain produced when it was attempted to be placed in certain positions, and by the fact elicited from him, as a matter of history, that any shock communicated to the arm gave him great pain.

It so happened that just at this period I had left home on a tour in search of health, and, in consequence of this absence, he was taken to town to an eminent surgeon, well known to the family, and of whom, as they were well aware, I entertained the highest possible opinion. Another surgeon being at his house at the time, my patient had the advantage of their combined opinion, which was, that there existed "fracture of the anatomical neck of the humerus," and accordingly the arm was put up with splints, and appropriate bandages, &c. A fortnight afterwards he was again taken to London, when it was pronounced to be improving—that the process of union had evidently advanced—and that the patient was going on well.

On my return to Farnham, I visited my young friend at the very latter end of August; I considered the whole matter, and at my next visit, which, by the date of the subjoined letter, I see was on the 30th, I ventured to express my doubts as to the reality of the fracture of the anatomical neck of the humerus. I was at once requested to write to my friend ——— and state those doubts to him, which I did immediately as follows:—

Farnham, Aug. 30, 1843.

My dear Sir,—Since my return from a tour in search of health, among the Welsh mountains, I have visited my young patient, ———, whom, I understand, you have seen during my absence, and have judged the accident, which he met with last May, to have been "fracture of the anatomical neck of the

humerus." The kindness and friendship which you have always shown me, induce me to venture the following doubts upon the subject; drawn—

First, from the nature of the accident.

Secondly, from its immediate consequences.

Thirdly, from the effects of treatment.

1. The nature of the accident. It arose from his *throwing a pillow across the room*: could muscular power have so acted upon the "*anatomical neck*" of the sound humerus as to fracture it? There was no external violence of any kind.

2. The immediate consequences of the accident were violent pain, such as would have occurred from the rupture of muscular fibre, &c., and some loss of power to execute certain muscular movements; but there was no entire immobility of the limb, no necessity for its being supported; on the contrary, he carried the arm without support, he dressed and undressed himself, wrote, rode on horseback, even played at cricket: the chief inconvenience which he felt was in raising his hand to his head, as in the act of bowing, &c. Could all these things have been if the shaft of the bone had been separated from its head?

Again, at *this* time, there was no shortening of the arm, no perceptible deformity: but if the bone had been thus divided, would not muscular action have necessarily pulled its shaft inwards, forwards, and upwards?

3. The effect of treatment:—I have not, of course, removed the splints; I have merely readjusted the external bandage, and tightened the straps. But if the *anatomical neck* were broken, would not the effect of *raising the elbow*, as in the present apparatus, necessarily be that of *aiding the muscles to pull the broken shaft forwards and upwards*?

Forgive me, my dear sir, if I have not stated my doubts with sufficient deference to your superior judgment; all who know me, also know how super-eminent I value your opinion; but with you or with me, I trust that a *truth*, which is, or may be of consequence to a patient, is *valued* more highly than coincidence of opinion.

Always believe me to be,

&c. &c.

To this letter an answer was sent by return of post, couched in the kindest and most friendly terms.

It was stated that the designation of *fracture of the anatomical neck of the humerus* was given rather by the other surgeon present, than by himself; that for himself, he should have said simply "*neck of the humerus*", leaving it open as to whether the course of the fracture was *transverse* below the tuberosities, or *oblique*, and separating the great tuberosity with the shaft, from the head and small tubercle, held by the subscapularis muscle. The obliquity of the fissure, and the extent of the surface of crepitation, rather conveyed the impression of a split bone to my mind, than a transverse fracture. Of the *separation of the shaft from the head of the bone by fracture*, not a doubt existed in either of our minds; nor, I venture to say, would it have existed in yours. The round obicular projection of the deltoid, the pulling or drawing forwards and inwards of the shaft, giving a remarkable width to the joint; the perfect facility of communicated motion in all directions, the pain of overhand motions, and the patient's incapacity of performing them but by a jerk from the trunk, and never without pain; the plainness of the fissure to be *felt* by the surgeon's fingers, the pain it gave the patient when pressed there, and the very perceptible crepitus put the fact of fracture beyond doubt.

"The most remarkable circumstance attending the case, is its non-discovery for so long a time; not its occurrence, for this I believe may, and does, though

not often, result from strong muscular action; such as is competent to lacerate the muscle itself, might do it in a young subject. This common, painful, and laming accident was so obvious to conjecture, that perceiving through the clothes that the stump of the shoulder was not flattened, I immediately attributed the mischief to this cause. But in stripping to examine, which had not been done before, the interval that had elapsed only confirmed the deformity, and the father of the patient, Mr. —, observed that it had never been free from pain, if extensively elevated, or touched by a schoolfellow, though the underhand motions he could perform without pain.

"On examining it at the end of three weeks, both — and myself were of opinion that the process of union had fairly commenced (ligamentous though it might be), that the fissure was considerably less perceptible, and no crepitus given on gentle rotation as before; and I believe that the hollow, or cupped splint, has operated most beneficially in keeping the broken parts in contact and at rest. The cervix scapulæ, coracoid process and head of the humerus, were sound and in situ; indeed, the appearance had no resemblance to luxation, independent of fracture; and the elevated and advanced position of the top of the shaft, *i.e.* the inferior fragments, would be explained by the action of the great pectoral, clavicular portion of the deltoid, and coracobrachial. Except for the quieting of muscles by more complete rest, and taking off the weight of the forearm, I agree with you that the trough for the elbow and flexed forearm, was not called for; but I think it contributes greatly to the objects of immobility and security in all upper-arm and shoulder injuries. I think you are premature in your third head—effect of treatment—which, unless you had examined the case when I did, you could hardly appreciate if now subjected to your examination, and not at all without this advantage.

"Believe me, my dear Mr. Newnham,

"&c. &c."

Not at all convinced by this letter, which, though it enfeebled some portion of my argument by shifting the injury from the *anatomical* to the *surgical neck* of the humerus, yet left the main scope of my reasoning untouched. I next suggested that I should accompany the patient to town, for the purpose of consulting with my friend personally; but this was frustrated by its now being the beginning of September, and by his leaving London for his annual period of relaxation. I was then requested by the friends to take charge of the case.

My young patient was now beginning to suffer a *great deal more pain* than he had done—apparently from the pressure of the splints and bandages. Of course this was incompatible with the quiet process of union which was supposed to be going on; and the apparatus was removed for the purpose, *first*, of ascertaining whether this pain was occasioned by any uneven pressure. Upon removing the bandages, almost the entire upper-arm was found to be much swollen, with increased heat and redness. It is needless to say that the splints were not reapplied; an evaporating lotion was employed, and absolute rest enjoined. This accidental, and common inflammatory action, no doubt aggravated by the pressure of the bandages under the *existing circumstances*, subsided in a few days, but left behind it what appeared to his father to be a *great increase of the swelling*, since he had seen it last opened in London. From the examination which I now made, and which was very gentle, for very slight movements gave him exquisite pain, I could perceive no loss of continuity between the shaft of the humerus and its head. I could not discover the sulcus in the body of the bone referred to in the above letter. I could discover that the upper half of the bone was implicated in the disease, and enlarged, the principal swelling, however being at the superior extremity, and immedi-

ately below the acromion scapulæ. At one part of this general swelling, there was a greater degree of prominence, evidently occasioned by some increase of the tumour underneath the insertion of the pectoralis major. My impression at this time was, that whatever might have been the original mischief, we had now to contend with an advanced stage of inflamed bone; and, while the idea of malignant disease was apprehensively present to my mind, I fondly hoped that it might be a case of necrosis, and, as I then stated, I should only be *too happy* to have suppuration set up in the bone; for, frightful as might have been the result, it appeared as nothing when compared with the horrors of malignant disease. The case was now treated accordingly. As soon as the simple inflammatory action had subsided, the iodide of potassium ointment was freely rubbed in upon the shoulder, and that medicine was given internally in combination with sarsaparilla, &c., and an appropriate diet.

As yet, the health did not seem to suffer materially; but the swelling of the arm was never controuled; it went on increasing, and ever increasing. Having thought, for some time, that I could perceive a deep-seated fluctuation in the prominent part of the tumour before mentioned, under the pectoral muscle, I determined on inserting a fine lancet, in the hope of eliminating pus, and relieving my suffering young friend. This was effected on the 30th of September, but to my extreme regret and mortification, no pus followed—while the few drops of intensely black blood, and the widely gaping wound, too surely told the malignant nature of the tumour, and too fully revealed the expectation that we should have fungoid structure sprouting immediately. Fully sensible that nothing but the entire removal of the disease could now save my patient's life, yet doubting the possibility of securing even this result, I immediately requested a consultation with —, who had not yet seen the case, and who most kindly came out from his retirement in order to give us the benefit of his counsel. This consultation occurred on the 4th of October, and the result was, an entire confirmation of my worst fears; no doubt of the malignant nature of the disease, and a decision against any operation, from the situation and extensive connexions of the tumour, and the impossibility of removing it with any hope of eventual success. It was recommended that we should push the present system more actively; and cautiously, but fully, give a fair trial to IODINE, in gradually augmented doses, as far as the system could bear it; not so much from any real hope of success, as from feeling that it was the only remedy within our reach, and that the patient ought to have the benefit of the trial. This was given in union with steel wine; and morphine, which had been already exhibited for some weeks, in order to allay the intense pain and nervous irritability of the system, was to be increased according to the exigencies of the case.

No practical good can arise from following the deepening shades of destructive malady, or recalling scenes of exquisite suffering, and of the most deeply harrowing and agonizing character. Suffice it, therefore, only to say, that the treatment seemed to exert no influence in arresting the malady—the tumour continued daily to increase—the fungoid structure to sprout—two other fungi shot up from two spontaneous ulcerations—a third, and still larger, was daily expected: the digestive function gave way. A few days before his death, a considerable sanious discharge took place from the wounds, and the system, worn out by pain and irritation, and defective nutrition and discharge, finally gave way, and death put a period to this extremity of human suffering on the 20th of November, about six months from the accident alleged as the cause of this mischief.

The following are my notes of the post-mortem examination which took place forty hours after death,

and was conducted by my partner, Mr. Sloman, and myself.

Section Cadaveris.—Æt 17.—40. horis post mortem.

External appearances—great emaciation, but a well-proportioned body, with the usual degree of development; and a particularly well-formed, fine chest.

A large swelling of the right shoulder and upper arm, measuring in its largest circumference 32 inches, from which protruded three fungoid excrescences: rapidly advancing decomposition: the tumour, which had been tense and firm, had become soft, and evidently contained much fluid matter.

This fluid matter was evacuated partly by puncture, partly by being laded out with a teacup: and in these ways about six pints of a thickish dark-coloured grumous matter were removed; as also, three small and undefined portions of bone, not yet thoroughly decomposed. In this fluid matter was also found, in a state of ulceration, the cartilaginous structure which had originally formed the covering of the head of the humerus in the glenoid cavity: no trace of the said head of the humerus was discoverable. There was no trace of muscle; the remaining more solid mass consisted of that kind of encephaloid degeneration, which is usually found in this species of malignant tumour.

As regards the bony structure, two-thirds of the shaft of the humerus, with its articulating head, were gone, and not to be traced at all; of the remaining third, about one half was in a rapidly advancing state of decomposition; and the remainder, close to the elbow, appeared to be sound. Of the SCAPULA, the acromion process had been consumed, the coracoid process partially so—only a small portion of its articulating surface remained—and in that, ulceration had already far advanced. The first, second, and third ribs were deprived of their coverings, and were perfectly rough and scabrous. The clavical was extensively involved in the same destruction for about two-thirds of its length.

About four ounces of serum were found in the right pleura; at the lower and under part of both lungs, but especially of the right, were found many encephaloid tubera of the same description; and in the right lung, one of these equalled in size a walnut: the upper portions of both lungs appeared sound.

The heart was small but healthy.

The liver was enormously enlarged, extensively granulated, of too light a colour, and possessing several similar tubera, similarly situated.

The mesenteric glands were generally enlarged—*many* much so—and all assuming more or less the same kind of tubercloid appearance.

The kidneys were enlarged, and seemed to have undergone a similar degeneration.

There were some scybala in the colon; the coats of the stomach were thickened but not diseased; the spleen, pancreas, and bladder, were healthy.

The head was not examined.

REMARKS.

I. It is important always to attend to apparently slight injuries, for although no other treatment could have been adopted in the present instance, and although the most careful examination would, I am persuaded, have thrown no new light upon the subject, yet it would have afforded a satisfactory answer to the subsequent difference of opinion; and on some other occasions, much ultimate mischief may possibly be prevented by a minute acquaintance with what appears to be a trivial injury.

II. The accident was not the cause of the subsequent disease. It is quite absurd to suppose that the mere tossing a pillow across a room should produce malignant disease, which should destroy life in six months. The only satisfactory conclusion to be drawn from this fact is, that dormant morbid action had

been insensibly going on in the part, in all probability covered in by the fibrous structure of the periosteum, or by some adventitious investiture of its own, which cracked or gave way under this unusual movement of the arm, and afterwards assumed a sure and steady, though at first slow, and eventually rapid progress, to the destruction of the parts, and of life itself.

III. Had the state of health at Easter, 1843, anything in common, with the subsequent malady? I am not prepared to trace this connection; and yet on reflection, I do not now think that the state of apparently overworked brain could be fairly attributable to the duties of school, in a youth whose abilities were far above par—who had therefore not undergone any especial labour—and whose position had long since placed him beyond the annoyances of “fagging.” My belief is, that that state of brain was produced in part by the *impending physical evil*, which operated as an unseen and an unknown irritant to the nervous system; and that perhaps both the one and the other were the product of a constitutional diathesis, the effort to trace which up to its *final cause*, would be just as fruitless, as the search after *final causes in general*.

IV. It is quite impossible in a review of this case, to pass over the question of *fracture or non-fracture*, especially as the examination after death does not aid its determination; and while I avow my own belief that no fracture existed till the continuity of the bone was destroyed by the destructive progress of the disease, I must leave it to the profession to determine whether my reasoning is sufficient or insufficient.

a. If the bone were *broken*, independently of *disease*, such fracture must have resulted either from *muscular action*, or *external violence*.

We are well aware that some bones, which have fixed or ligamentous attachments on the one hand, and on the other, form themselves the points of insertion for very strong muscles, as the patella, the olecranon, and, less frequently, the calcaneum, are thus fractured by violent muscular action; but it is obvious that in the present case we want both the fixed point and the muscular agency, which shall be capable of separating the *head* of the humerus from its *shaft*.

External violence there was none: therefore according to a priori reasoning there could be no fracture.

b. The fracture of the neck of the humerus must involve immediate deformity, immobility of the limb, pain, swelling, tension,—all of which sensible signs of fracture were absent.

c. But we must examine minutely what was the state of things in August, when my patient went to London.

As it appears to me, the *first impression* on stripping him, was that the case was one of fractured neck of the humerus; and this first impression thus produced, gave its prevailing tinge to all the subsequent examination. Every one who has at all watched the influence of first impression or preconceived opinion, upon the subsequent stages of apparently unbiassed investigation, will have perceived that such impression or opinion does almost always exert a tyrant sway upon the mind, and compel it, in spite of itself, to see every object through its own medium.

That such was the case in the present instance appears, first, from the different shade of opinion, exhibited by the two consultants, the one characterizing the state as “fracture of the anatomical neck of the humerus,” thus marking the course of the fracture as so completely transverse, that it occupied only an imaginary line, possessing neither breadth nor extension; the other conceiving that the fracture was *oblique*, conveying the impression of a split bone, perceiving a fissure or sulcus of considerable length, and confirming these views by the extent of the surface of repitiation.

It is obvious, that with opinions so *varied*, formed by observers at the same moment, the case could not

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,