mutually opposed. In an ulcer, certainly, the two processes are very apparent; there are the disintegration going on, and also the formation seen in the cells and granulations. A very valuable and scientific paper on this subject is to be found in the *Guy's Hospital Reports*, by Mr. Golding-Bird. It wants but a moment's consideration to see how all organic life on the globe is necessarily associated with death, and that the same law is in operation in pathological processes.

Let me for a moment suggest to you my meaning. Let there be a softening or destruction of the brain; we find it surrounded by a formative cyst or inflammatory products; and, in the syringo-myeloid of the cord, the same thing is seen, large cavities associated with new products. It should be remarked, however, that, although reparation is going on, it is only by the formation of the simplest material; the highly complex structures are never renewed (except, perhaps, in children under special conditions); no injury to any of the parenchymatous organs is replenished by the same material, the repair is made by simple fibre. Bone may be replaced, and skin in an imperfect manner. In the case of the lung, the two processes, destructive and reparative, are always seen together; indeed, an ordinary phthisical lung could never have been witnessed without the double process. The ulcerated tissue would have produced a fatal hæmorrhage or pneumothorax long before any extensive change could have occurred in the lung; but the vessel becomes closed, and the lung adherent. Now, the more chronic the case, the more are these reparative conditions marked, so that the pleura becomes greatly thickened, much of the pulmonary tissue cicatricial, and the cavity becomes lined with a firm membrane. Under these circumstances, we say that the signs of phthisis are well marked, the dullness on percussion is extreme, there is a falling in under the clavicle, and the pectoriloquy is pronounced. These are not so much signs of disease as of repair or cure. This is the reason why the young practitioner in such a case gives an unfavourable prognosis, forgetting the meaning of the signs, and, to his astonishment, may find his patient in much the same state a year afterwards. It is worthy of note that fatal hæmoptysis rarely occurs from the destructive process; it is due to the giving way of a dilated vessel in a localised and very chronically affected part of the lung. The same probably occurs oftener than is supposed in the stomach; the chronic ulcer is characterised not so much by the destruction of the tissue, as by the new growth around it; and it is in connection with this that the vessel becomes expanded and thinned, and subsequently ruptures. In cases of scirrhus pylorus, it is quite as much the hypertrophied natural tissue which constitutes the tumour as the adventitious substance.

In the case of the liver, it is often difficult to say what changes are reparative or constructive, and which destructive. As the liver wastes, we observe a large concourse of veins on the surface of the abdomen, and we regard these as one of the signs of cirrhosis; but it is rather to be regarded as a consequence of the disease than one of its constituent parts. For, while the liver is being compressed, new blood-vessels and new bile-ducts are being formed, and the blood from the portal vessels, which would otherwise be arrested, makes its way to the surface and systemic veins, through the opening out of the old umbilical vein. The case of cirrhosis is a marked example of what I have been saying of the ulcer, whose description would demand demonstration of both a destructive and a formative process; but, in the case of the liver, where the change is so complex, it would be difficult to draw the line; many of these changes, however, are clearly no part of the primary pathological process. I have seen it stated, but I do not know with what authority, that, if the pulmonary arteries be injected in phthisical lungs before removal from the body, the injection may extend into the chest-walls through newly formed vessels. I may take another example, in the case of the pseudo-hypertrophic paralysis of children. Now, in all the cases which I have seen, there has been a marked atrophy of the muscles, so that parts are wasted whilst others are enlarged, giving a general deformity to the whole body. In those limbs, too, which are hypertrophied, there is often perfect helplessness, showing that the natural muscular tissue must have undergone a decay. In the case of a boy lately under my care in Guy's Hospital, the difference in the size of the legs was most remarkable, one being three or four times the size of the other, but they were both equally helpless. The one showed merely wasted muscles, the other was firm from the production of new false tissue. The arms were wasted, and yet large masses of new tissue were felt in the deltoid and biceps. In these cases, there is atrophy of muscular tissue, but, besides this, there is what is never observed in the adult, a productive process going on at the same time; the latter, however, has not the power to reach to the development of muscle, but remains in its lower state of fibre only. The example, however, is a striking one of the productive process going on with the atrophic one. In the case of the blood-

vessels, degenerative and productive processes are constantly seen progressing together. In the cancers and other tumours of bones, we perceive a destruction of the original tissue, and yet new bone growing up in the tumour.

You must forgive me for these few desultory remarks. I should have liked, had time and opportunity served, to have offered to your notice some well developed theme; but I had, on the present occasion, no other resource than to look around me, and see what subjects had occupied my thoughts; one or two of them, although crudely brought before you, are, I think, worthy of your consideration.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF SURGERY,

At the Annual Meeting of the British Medical Association, held in Cardiff, July, 1885.

By EDWARD H. BENNETT, M.D.,

Professor of Surgery in Trinity College, and Surgeon to Sir Patrick Dunn's Hospital, Dublin; President of the Section.

INJURIES OF THE SKELETON: VALUE OF ACCUMULATION OF SPECIMENS.

My first duty on taking the chair is to welcome to this Section our distinguished foreign *confrères*, who have come from afar to join in the important discussions which we open to-day.

On the part of the officers of the Section, I thank the goodly array of members of the Association who have given us their zealous aid, both by their presence and by their labour. Personally, I desire to express to the Council of the Association my sense of the honour they have conferred on me by asking me to preside in this Section. I trust that, by my conduct in the chair, I may be able to maintain the fair fame of Irish surgery—a duty to which I am doubly bound, by virtue of the allegiance I owe to the Royal College of Surgeons in Ireland, whose president I have lately been, and again to the University of Dublin, whose professorship of surgery has been in my hands now for many years.

I propose, in the few words with which I shall detain the Section, to deal only with the subject with which I am myself most familiar, and to which I have devoted most of my work. I mean the pathological and clinical study of injuries of the skeleton. I think I can show that real progress here is to be made, not by the study of rare and attractive specimens and cases, but by the accumulation of large numbers of examples of ordinary injuries, and, above all, by laying down the rule, never to reject or throw away such things as appear to possess no interest. I have often been asked, what is the use of taking up space with such numbers of duplicate specimens as the museum under my direction in the University of Dublin contains? To such I have always answered that, only by the examination of a large number of individual specimens, can the details of any typical injury be worked out. I hope to show that, by this means, not only can the details of familiar injuries be rendered more complete, but new types can be discovered.

I may briefly refer, as an example, to the facts relating to Colles's fracture of the radius, which I was able to bring before this Association at the meeting in Cork in 1880. I showed then 54 specimens of this injury, and demonstrated that, among these, 23 gave proof of having been impacted fractures, while 31 were injuries in which positive evidences of impaction were absent. Since the time of that communication, our collection has grown to 100, this number being reached only within the last few days. We now find that the figures stand relatively 48 and 52; and so, perhaps, the truth is more nearly reached. We have ample evidence before us, in these facts, to put aside alike the opinions of Voilemier and of Smith; and we must be prepared, in practice, to meet these two conditions of the fracture in almost equal numbers. I speak on this matter with great deference to the illustrious names I have quoted. The merit of the first step in the investigation Smith rightly attributes to Voilemier, he being the first to demonstrate the constancy of the details disclosed by a section of the united fracture. From Smith's statements, the conclusion has

been drawn by many that no such thing as impaction exists in the fracture. Between these contending views, nothing but the appeal to a large number of facts could decide. I found it all the more difficult to enter into the contest, for I was brought up as a pupil of Smith, learned from him for many years as a colleague, and have succeeded him in his professorship. I hold in these matters, however, that we must follow the Horatian maxim, "Nullius addictus jurare in verba magistri."

In another group of injuries—fractures of the ribs—my investigation of a great series of specimens has led me to endorse, in opposition to the commonly accepted theory, the opinion of Malgaigne, which seems never to have taken hold of the profession. Our standard text-books to this day, in this country and in America, hold fast to the theory of Petit, which assumes that, when the chest-wall is broken by indirect violence, when the ribs break as the result of compression of the chest, as a whole their fragments are thrust outwards. Not a clinical observation, or a specimen that I know of, confirms this view, and yet I say it is taught everywhere. Let any one who seeks to verify its value examine some hundreds of rib-fractures, and his views will soon change.

Similarly, some mystery attaches to fractures of the costal cartilages, chiefly because they had attracted but little notice until the beginning of the present century, when Lobstein directed attention to their peculiar mode of union by bone. This mystery is only the result of want of observation—it is said they are very rare. In 1865 the total record of such numbered thirty. Well, they cannot be very rare, seeing that I have myself observed some 25 examples dissecting and preserving in these 15 specimens, all which go to prove that only in the peculiarity of cartilage uniting by a structure different from itself does this fracture differ from that of bone.

But I may not delay the Section over examples of this kind. I must pass on to show how the mere preservation of specimens apparently uninteresting and unimportant can lead the way to useful results, and direct clinical observation. If I place before the meeting a single specimen of broken fibula, such as the diagram represents, such as any single one of the series on the table is, most men conversant with surgical pathology will pass it by as worth very little indeed. So did I; only I put these by safely as they came. After a time I found that their increase in number demanded attention. Every one knows the great importance that attaches to fractures of the lower end of the fibula, their great surgical interest, the writings of Pott and of Dupuytren. In all these and elsewhere no mention is made of fracture of the upper third of the bone, except as it occurs as an element of fracture of both bones of the leg, or with a complete diastasis of the bones at the ankle.

In looking over these specimens, I was struck with the fact that one feature was constant in them all, a fracture of great obliquity, but without material displacement. What was the cause? Certainly not casual direct injury. Going on with my study, I was not long in finding evidences that the injury was in some associated with lesion of the ankle-joint; in some, a very special fracture of the tibia exists, which, when present, is alike in all. In others, evidence of past traumatic inflammation of the bones at the ankle bear testimony to the origin of the force from the ankle. In one, the fracture is associated with fracture at the usual site of Pott's fracture in the lower third. I could produce several fractures of the fibula in its upper third similar to these, where, unfortunately, the tibia has not been preserved, which I doubt not are of the same type, for the extreme obliquity without displacement is quite distinct from the form of lesion which goes with fracture of both bones. Any way the number is remarkable, ten in all, where both bones are preserved. Seeing these facts, I could not doubt that the injury, although unfamiliar to me as a clinical observation, must be sufficiently common. I was not long on this track before I found what I wanted. This case I published in 1880. I will now give an example observed by my colleagues in Sir P. Dun's Hospital, and so free from the objection that might be advanced against myself on the score of prejudice.

A man slipped in carrying a sack of grain down a sloping plank in unloading a vessel. His foot was suddenly checked in its slide by some irregularity of the plank, and he fell, conscious that something had given way in his leg. He did not strike or hurt his leg otherwise in the fall. Admitted to hospital, as he was unable to bear weight on the limb, he presented the ordinary features of a sprained ankle, without the ordinary signs of fracture of the ankle-bones. Treated for sprain, the case attracted but little attention for several days, when the circumstance of ecchymosis, high up in the limb, attracted notice. On this part being examined, the localised pain and crepitus peculiar to fracture left no room for doubt as to the diagnosis.

Somebody will say, Of what value is the observation? the bone unites well, and without deformity. So it does; but I hold that we surgeons, for our own sakes, if not for the sake of our patients, should avoid every error of diagnosis. Certainly, the surgeon who treats a sprained ankle for a week or a fortnight only to find it necessary to confess the existence of a fracture at that date, discovered by himself or another, does not feel pleased or proud of his skill in diagnosis.

I will only note further that this injury seems, as far as I have seen it in the living, to result, as in this case, from a sudden jar or wrench while the foot is fully extended.

I now will detain the Section only a few moments while I give one other example of the benefit which the preserving of every, of even the least interesting specimen, yields. I may so show that, in mentioning these matters, I do not seek to sound my own trumpet, for the results are only the direct result of the method I have followed; with the clinical and pathological opportunities afforded by my position, and this mode of study, it needed little originality to see the family likeness connecting typical groups. Passing in review each series of pathological specimens, I laid aside the few examples of united fractures of the metacarpal bones in our collection, waiting until their larger and more important neighbours could be arranged. The small size of the bones, and the light value, clinically, which we surgeons have been apt to assign to simple fractures of the metacarpus, delayed my study of them, and so, perhaps, the result has been all the better; for, without some time spent in accumulating, a number sufficient to attract attention could not have been reached.

Taking at last the group into closer study, I was struck by the fact that one particular fracture outnumbered all others.

Malgaigne and Hulke have both asserted that the fracture of the metacarpal bone of the thumb is more common than that of any other of these bones, opposing the views of Boyer, who assigns the premier place to the fifth. The site of the fracture in the individual bones is set down as being "just above the middle," "in the middle or distal third."

Now, if we look to the series before us, a remarkable fact is disclosed. Amongst these fractures, there occur six examples of fracture of the base of the metacarpal of the right thumb, and no others of this bone. In each of these, the injury is the same—an oblique fracture, detaching the palmar half or more of the articular surface which faces the trapezium, with that projection of the base of the bone into the palm which supports the surface. The entire bone, except the little piece so separated, slips backwards, simulating in the living a subluxation of the bone in this direction. The appearance in the living of a subluxation is confirmed by measurement; for, as the fracture does not implicate the dorsal surface of the bone, the length on this aspect is unaltered.

When I published my first note on this injury in 1879, I had but the experience of a single clinical observation, and so I spoke with but little confidence about it. Since then, and as the immediate result of my publication, I have seen a great number of examples both of the recent injury and of the united fracture. Most remarkable is the fact that in every case the accident has been on the right side of the body. Certainly this injury, once we know of its existence, becomes vastly more common than any other of the metacarpus. If left to itself, it unites with such deformity as this cast shows—a trivial deformity, after all. Why, then, deem the matter worthy of the notice of this Surgical Section of the British Medical Association? Simply for this reason, that I have ample proof that a hand so injured remains, under the best of treatment, long disabled, and, without treatment, for a greatly longer time. When we consider the value of the right thumb to anyone who lives by handicraft, or indeed to any, rich or poor, we should not let pass unnoticed and undiagnosed this common injury. One point I have omitted to mention—the cause. In every case but one which I have seen, the cause has been a fall which might well have broken the radius, but some slight deviation has directed its force against the thumb. Once I have seen it as the result of a blow of the fist against the jaw of an adversary in a fight.

I have already mentioned the essential features of the injury. One word as to the reason why probably it has long escaped notice. The pain and swelling of a sprained thumb are familiar to all, and prevent the ready appreciation of crepitus in the recent injury; but this may be found readily if pressure be made on the base of the bone from palm to dorsum, while a slight traction is made, sufficient to reduce the large fragment into place. I am confident that, before long, many of my hearers will test this matter for themselves, and perhaps then excuse my weary discourse to-day. I have said enough to establish the merit of patient and diligent pathological study, combined with clinical observation.