CASE OF PROGRESSIVE PARALYSIS OF THE ULNAR NERVE, CONSEQUENT UPON INJURY: OPERATION: SUCCESSFUL RESULT.¹

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WALTER P., aged 15, a miller's assistant, was admitted into the Northern Hospital, under my care, on the 8th October, 1883. His right arm had been caught between a broad leather "travelling-band," and the wheel over which it passed; so that he was dragged off his feet, and suspended by his arm, the result being a compound fracture of the bones of the forearm. There was a small transverse wound on the outer side of the forearm, through which about an inch of the upper fragment of the radius protruded, being tightly embraced by the skin; the fracture of the ulna was not absolutely compound, but the sharp end of the obliquely fractured upper fragment was sticking in the skin so as to dimple and displace it. Forcible extension was required to reduce the fragments into their proper position. The case was treated on Listerian principles, and, so far as the fractures were concerned, no further trouble was experienced; except that there was considerable delay about the union of the ulna, and for many weeks grating could be felt at the site of this fracture, which gave rise to the suspicion that a spiculum of detached bone must be lying between the fragments; especially as there was an abundance of so-called provisional callus surrounding the fracture, showing that non-union was not due to want of action in the part. However, no fragment ever came away, and I am now disposed to think that a piece of fascia must have been dragged down between the fragments, and so, for a time, prevented union. Be the cause whatever it might, union ultimately became complete.

But the interesting feature of the case appears later on. The lad had ceased to be an inmate of the hospital after a month's treatment, and was attending as an out-patient, on account of the non-union of the ulna, when it was noticed that there was a tendency to obstinate flexion of the fingers, more especially of the little and ring fingers. A hinged splint, by which elastic extension can be used for the purpose of straightening contracted fingers or hands, was therefore applied; and in a day or two the patient presented himself, with the ring and little fingers blistered and discoloured by the pressure of the splint and bandage. This, of course, at once led to inquiries as to the state of the ulnar nerve; the rest of the hand appearing quite healthy. Then it was ascertained that the lad (who had always been most patient and uncomplaining) had experienced curious sensations, partly of pain, partly of numbness, ever since the accident; and now it was found that the affected fingers were deficient in sensibility. The natural conclusion was that the nerve had been pinched or bruised by the sharp edge of the bone at the time of the accident. There was reason to believe that the nerve had not been divided, and we hoped that the effects of the injury would pass off.

But they did not; and gradually the numbness increased, and the fingers wasted at the tips; the circulation was very sluggish, the fingers purple and cold, the skin moist and clammy. Then the interossei and the lumbricales wasted, and the adductor pollicis and the muscles of the little finger followed suit. Combined with the numbness, there was evidently a considerable amount of pain felt on any attempt being made to straighten the flexed fingers; and a dragging sensation was complained of at the seat of the fracture, just above which, pain was felt on pressure. The symptoms, therefore, pointed to gradually increasing compression, or constriction, with adhesions of the nerve at this point.

For the sake of brevity, I have hurried through the account of the case up to the middle of April (six months after the accident), at which time the hand was rapidly assuming the "bird's-claw" appearance characteristic of the later results of paralysis of the ulnar nerve, and thus tersely described in Dr. Vivian Poore's Bradshawe Lecture, delivered on August 18th, 1881: "When the ulnar nerve is paralysed, sewing, writing, and all delicate manipulations become impossible. The patient cannot move the fingers to and fro in the same plane. The mid and far phalanges cannot be extended, nor the near phalanges flexed. After a time, the hand, by the unopposed action of the flexors and extensors of the fingers, becomes 'clawed'; that is, the near phalanges are extended, while the mid and far phalanges are flexed." As before

mentioned, the interosseous spaces, both on the dorsal and on the palmar aspects, were hollowed out; the muscles of the ball of the thumb, and those of the little finger, were much wasted. With regard to the cutaneous sensibility, there was complete anaesthesia of all the surface usually supplied by the ulnar nerve, and also incomplete anaesthesia of the adjacent sides of the middle and ring fingers (due, no doubt, to this being one of that minority of instances where the ulnar nerve supplies two and a half, instead of one and a half, fingers).

On April 28th, I proceeded to explore the site of the fracture of the ulna, which was now firmly united, but was marked by a slight thickening of the bone. An incision was made as for exposing the ulnar artery, the centre of the incision being over this thickened portion. Neither artery nor nerve was to be seen, though carefully searched for; the bone was reached; the fracture was found satisfactorily united, the upper fragment projecting very slightly above the lower; there was some thickening from callus, and, moreover, what seemed to be a band of thickened periosteum, tightly stretched over the callus. The question arose, Was this the sought-for nerve? The wound was enlarged upwards, and the nerve was then found unmistakably, and traced into this band; then the wound was extended downwards towards the wrist, and what appeared to be nerve was found, and proved to be continuous with the band; but, whereas the nerve above the adhesion was pink and fleshy-looking, the part below was colourless and wasted. Partly by the knife, and partly by gentle elevation with a raspator, the nerve was freed from the bone. We then had between two and three inches of ulnar nerve completely free, so that it could be lifted above the level of the wound, and thoroughly examined. The upper part was, as before mentioned, pink and fleshy-looking, evidently swollen; the part which had been fixed down was thinned and flattened, ragged from small tags of fibrous tissue attached to it; and the lower end of the nerve was round, but pale, and apparently wasted. To prevent the lately adherent portion from dropping down into its old situation, and probably again becoming attached to the bone, I connected a few fibres of the flexor carpi ulnaris with the adjacent flexor profundus digitorum by one catgut suture, thus interposing a fleshy bed between nerve and bone. The wound was then closed, and dressed in the usual Listerian method.

On May 1st (three days after), Mr. Horrocks, the senior house-surgeon, notes that "the patient was able to tell when the skin on the proximal phalanges of the little and ring fingers was pinched, and appeared to have perfect sensation along the adjacent sides of the middle and ring fingers."

On May 7th (ten days after the operation), he notes "sensibility of fingers increasing, and extending downwards towards the tips, which are, however, at present insensible. The patient made the remark that, for the last two or three days, he has had a sensation as of blood rushing along towards the finger-ends."

On May 20th, the wound was quite healed, and tactile sensibility was rapidly increasing, being more acute in the ring-finger than in the little, in the proximal than in the second phalanges, almost absent in the last phalanges. The improvement since that date has been slow, but steady and persistent, until now (eight months after the operation), there is hardly any appreciable difference between the two hands. Tactile sensibility appears to be perfectly restored; the hand is now natural in shape, the interosseal spaces having become perfectly filled by their proper muscles; and the muscles of the thumb and little finger are restored to their normal state. The colour of the skin and of the nails is natural. The lad has returned to his work in the mill.

It should have been mentioned that until quite recently he has had some pain in, and tendency to flexion of, the index-finger, with disordered nervous sensations therein; and forcible extension of the finger has always, until recently, given rise to a dragging sensation at the site of the old fracture. Probably, a small portion of the median nerve, or rather of its anterior interosseous branch, sustained injury through the compound fracture of the radius, and some adhesions took place in that locality; and these, as he has begun to use his arm, have become stretched or broken down.

It does not appear quite certain how this peculiar injury of the ulnar nerve occurred, but to me it seems most probable that the sharp lower end of the upper fragment of the ulna caught and became fixed in a portion of the neurilemma, and, on reduction of the fracture, dragged it down, so that it lay in contact with, or was nipped between, the fragments. It was previously observed that union of this fracture was long delayed; and this could be readily explained by supposing that not only neurilemma, but also a piece of muscular fascia, had become entangled between the fragments. The paralysis certainly did not show itself until weeks after the accident, and then was for a time but slowly progressive; later on, when the nerve was not only caught

¹ This paper was read, and the patient shown, at the Liverpool Medical Institution, December 18th, 1884.

and bent, but also suffered from pressure and constriction, the progress of the disorder and of its symptoms was much more marked. The rapid return of sensation in the fingers was remarkable, and far beyond my expectations; the adherent portion of nerve being so thinned and flattened, that I doubted if it could ever resume its functions, and thought it most likely that I should be obliged to operate again, remove the damaged portion, and stitch together the divided ends.

I alluded to the fact of our not being able to find the ulnar artery in the wound. The probability is that it was so crushed by the sharp bone, that it was either divided, or so damaged that it became impervious immediately after the injury.

It may occur to some that an exploratory operation should have been performed earlier. Possessed of the wisdom which comes after the event, it certainly does appear that an earlier operation would have been at all events justifiable; and, with the knowledge gained in this instance, I should feel disposed, should I meet with a similar case, to operate before such a condition of paralysis was reached.

ON THE PRECISE RELATIONS OF MICRO-ORGANISMS TO DISEASE AND THE SCIENCE OF DISINFECTION.

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WHILE the connection of micro-organisms with the chief contagious fevers is, as yet, a matter of pure inference, it is impossible, in the face of the results of modern investigations, to deny the intimate relations of micro-organisms to certain other diseases, including puerperal fever, pyæmia, septicæmia, and anthrax of cattle; but of the nature of the precise relations, next to nothing is known. The physiological effects produced by inoculation-experiments can be readily observed, but the physiologist cannot lay his hands upon the active principles which cause them. In this connection, I am convinced he can ascertain nothing of a final character without the aid of the chemist; and, because this fact seems to me to have been entirely neglected in all recent investigations, I now beg to direct special attention to this matter, and to indicate how far reliance is to be placed upon the germ-theory of disease, and the use of disinfectants as controlled by that theory. In the first place, then, I shall endeavour to show that the effects which are witnessed in certain diseases are not caused by micro-organisms, but by chemical substances which are elaborated in or by them by way of secretion, excretion, or otherwise; and, secondly, I shall show that the methods now commonly employed by many microscopists and physiologists for testing the action of disinfectants are entirely and radically erroneous.

To demonstrate my views upon the first of these subjects, I will take three well known facts, and consider each very briefly.

In the Report of the Medical Officer of the Privy Council for 1876, there is a description of some carefully conducted and important experiments made by Dr. Burdon Sanderson, in confirmation of the earlier investigations of Panum. In these experiments, a septic solution was prepared by precipitating putrilage with alcohol, redissolving the precipitate in water, evaporating the extract to dryness, and redissolving the dried residue in water. From a series of physiological experiments made with this solution, Panum arrived at the conclusion that "there exists in putrid fluid a specific chemical body which is soluble in water, and is endowed with the property, when introduced into the circulating blood, of calling into existence that peculiar group of symptoms which are recognised as those of septic infection;" and, to use the words of Mr. Simon in reviewing these results, "Dr. Sanderson, though apparently still supposing that the septic ferment is particulate, seems to regard, as proven by Professor Panum's experiments, as well as approximately by his own, that it 'does not consist of living organisms.'" It has been proved then that micro-organisms, in course of putrefactive fermentation, initiated by them, produce one or more chemical products, which act as specific poisons and as infectants in pyæmia, septicæmia, etc.

The attenuation of the virus of chicken-cholera, which has been described by Pasteur, admits only of a chemical explanation. How can it be explained that micro-organisms, freshly cultivated in clear *bouillon de poule*, exhibit a murderous fatality when inoculated under the skin of previously healthy fowl, while the same micro-organisms, in equal number and activity, taken from a cultivation-mixture which has been freely exposed to the air for a long time,

are devoid of this property? To suppose that a morphological change can account for the observed difference in effects is, if not actually absurd, at least entirely unwarranted; and the only explanation is that the micro-organisms in question have nothing to do directly with the poisoning effects we are considering, but that they are due to a specific chemical product which is present in the freshly cultivated mixture, but which is absent in the stale mixture. Possibly it is destroyed by oxidation carried on by atmospheric oxygen.

Again, in the thirteenth annual report of the Local Government Board, Dr. Klein has described experiments which seem at first blush to indicate a variability in the degree of virulence of the *bacillus anthracis*; but further experiments proved that the observed facts were "irreconcilable with the assumptions (1) that the bacillus is in reality capable of undergoing a diminution of its physiological activity, that is, suffering a real attenuation, and (2) that there exist anthrax-bacilli having an intrinsic virulence of various degrees." He then adds, "The facts seem, however, capable of another explanation. Owing to different conditions of growth, for example, high temperature, or other artificial conditions, or owing to different soil on which they grow, for example, the body of a mouse or of a guinea-pig, the bacilli, although themselves the same, embody or appropriate, chemically or otherwise, some new or different substance, which produces the alteration for a particular species of animals. Whether this substance is comparable to a ferment or not, I am not in a position to say; possibly, it is some ferment produced by the new conditions."

It will be at once seen that Dr. Klein almost embraces the chemical theory of disease, which for some years I have persistently advocated; at least, he has himself furnished evidence which gives immense support to my views.

Professor Virchow also, while hesitating to allege the inadmissibility of a mechanical hypothesis of disease caused by micro-organisms, yet clearly thinks the assumption of chemical action remains as the only real explanation, as will be evident to all who are conversant with his writings, and notably with his article on "Infectious Diseases in the Army," which has been translated from the German by Dr. John James.

The chemical theory places us at once upon ground with which we are familiar, and gives us an assurance of security. Just as the yeast-cell decomposes a solution of sugar by the agency of a soluble zymase which it is supposed to produce, and as the *mycoderma aceti* oxidises (by the assistance of the air) alcohol into acetic acid, and as the *bacterium lactis* sours milk and produces lactic acid, so also do the micro-organisms which initiate putrefaction produce definite chemical products, which act as blood-poisons; and the micro-organisms which are known to be intimately associated with certain specific diseases act as the excitants, not in a primary or mere mechanical sense, but in a secondary sense, viz., by the agency of chemical substances elaborated by them under suitable conditions.

These reflections necessarily take us a step further. If there be no sugar present in its soil, the yeast-plant cannot produce alcohol; but it is not to be assumed (at least, in the absence of sufficient evidence) that this micro-organism can live only upon sugar. It is highly probable, indeed, that any one micro-organism can live and thrive under a variety of conditions, and upon a great number of soils; and so the products of fermentative change must necessarily vary accordingly. Many of them may be poisonous in character, while many others may be innocuous. So also it must be with the micro-organisms which are associated with disease; in consequence of which it follows that so-called zymogenic organisms may become pathogenic in character, simultaneously with a change of soil or other conditions, and *vice versa*.

Reviewing all these possibilities and facts, is the science of disinfection to throw overboard all accumulated knowledge, experience, and faith in the action of all disinfectant substances which are not under all conditions germicidal in property? Is the object of mankind henceforth to be the destruction of micro-organisms? If such an object could possibly be regarded as well founded, even then it were idle to attempt its consummation; for micro-organisms are ubiquitous, and constitute a necessary order in creation. By acts of hydrolysis, and oxidation carried on upon all dead organic matter by their agency, such substances are resolved into final innocuous products of change, which are essential to the well-being of the higher orders of creation (plants and animals).

The fact is, that men always outstep the natural limits of a discovery, and jump at conclusions which are not warranted by the results of further investigation. The somewhat sudden discovery of the intimate association of micro-organisms with disease led many to think that, in order to prevent the spread of disease, the micro-organisms must be killed wherever met with; but, now that scientific