convulsions. A section of the two anterior pyramids is followed by the same results, while a section of the olivary columns, which are chiefly the continuation of the anterior columns of the spinal cord, does not seem to produce a notable paralysis; so that the greatest difference exists as to the place of passage of the voluntary motor conductors between the spinal cord and the medulla oblongata.

From the facts we have related, concerning voluntary movements, we think it may be concluded that the idea that there are two columns of the spinal cord (the anterior) alone employed in the production of these movements, must be abandoned, and that it is extremely probable that the voluntary motor conductors pass in the anterior pyramids, and, after having made their decussation, pass chiefly in the lateral columns of the spinal cord and in the grey matter near these columns, and, at last, that, after a short distance, a number of these conductors leave the lateral columns to pass into the grey matter and into the anterior columns.

As regards the practical deductions from the various facts we have related, concerning the channels of sensitive impressions, and of the orders of the will to muscles, they will be discussed in the future lectures.

REPORTS OF CASES

OF

RESECTION OF THE HEADS OF THE PHALANGES OF THE FINGERS,

AND OF

EXCISION OF THE ENTIRE UNGUAL PHALANX.

By E. J. CHANCE, F.R.C.S.,

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UNDER the head of "Clinical Records," page 64 of THE LANCET of Jan. 16th, 1858, there is a case of "Resection of the Heads of the Phalanges of the Right Forefinger," in which the treatment adopted is spoken of as "a proceeding which it would appear is somewhat novel in its application to diseases of the joints of the finger—resection of the articulation;" and the writer goes on to state, "in the treatment a false joint will be sought for. We shall watch the case with some degree of interest."

Judging from these remarks that "resection of the heads of the phalanges" is in reality "a somewhat novel" practice, and therefore of more present interest than I had deemed it, I am induced to lay before the readers of THE LANCET an account of three cases in which I adopted such a course of treatment with perfect success in the years 1853 and 1856. The first case in which I adopted this plan of treatment, and which, from its success, led me to extend the practice to diseased joints of the fingers, was an unreduced dislocation of the extreme phalanx of the thumb, which had existed two years.

In this case, not only had the bone been dislocated, but the extremity of the first phalanx forced through the skin in the front of the thumb.

The condition of the part when I first saw it was as follows: The point of the thumb projected backwards nearly at a right angle, and was firmly retained in that position. The articulating surface of the unguis phalanx rested on the back of the digital extremity of the first phalanx, and this latter part formed a protuberance on the front of the thumb, which could be distinguished by its pulley-like shape, through the tense skin. As the extremity of the thumb pointed directly backwards, the protuberance formed by the digital extremity of the first phalanx in front constituted the *only* part that could be opposed to the fingers in grasping, &c., and as the skin and cicatrix over this protuberance were so exquisitely tender that the slightest pressure on them caused intense pain, from its being brought to bear upon the misplaced nerves, the entire hand was comparatively useless, and the patient, who was a housemaid, was compelled, on that account alone, to give up servitude and become dependent on her friends. Under these circumstances, she applied at the City Orthopædic Hospital in 1853. As I found reduction entirely out of the question, I determined, at the suggestion of my friend Mr. Pollock, and with the approval and assistance of my colleague, Mr. N. H. Stevens, to remove the head of the first phalanx.

The operation was performed by raising a flap on the front of the thumb, and sawing off that portion of the head of the bone which prevented the unguis phalanx from taking its proper position. But little bleeding ensued. The part was afterwards put up, in a slightly flexed position, in a splint made of a piece of card. The wound healed readily, and we had the satisfaction a fortnight afterwards of witnessing a perfect cure. The splint was worn for some weeks afterwards for safety. About three months after the operation, as the joint was firm, slightly movable, and the thumb *could be used with perfect freedom and without pain*, my patient accepted another situation as housemaid. I have seen her every five or six months since, and I can report that a few days back she was still in a situation, and her thumb quite well.

The foregoing case having terminated so successfully, I determined to try the same mode of procedure in a case of *caries* of the heads of the phalanges; and in January, 1856, I had an opportunity of so doing, as Mr. Pollock placed under my care a case that appeared to him appropriate for such treatment. In this case the patient was a labourer at a brew-house, and had had his hand severely injured by a barrel falling on it. The extreme joints of the third and fourth (middle and ring) fingers were both carious, the bones grating perceptibly against each other whenever they were moved; and there was an opening into the joint of the fourth (ring) finger, but not in that of the third finger. In this case I made the flap on the back of the finger, and removed, with a fine saw, a *thin* slice of the head of the second phalanx of each diseased finger, and a like portion from the articular extremity of the unguis phalanx of the fourth finger. The articular extremity of the unguis extremity of the third finger was not carious, and therefore not removed. The parts were then put up, as in the former case, on splints made of a piece of card; but in this case the fingers were kept *firmly and fully extended*. This case was also well in a fortnight; but, as in the former instance, the use of the splints was continued for some months, to prevent the contraction of the flexor tendons. The operation was performed in March, 1856, and in the following September the man resumed his laborious manual occupation at the brew-house. I saw him a week ago, when his hand was still quite well, and he then informed me that he had been at work constantly from the time he returned.

In the foregoing cases it will, without doubt, have been noticed that in each the hand was so materially damaged as to deprive the individual of the power of following his or her ordinary avocation of manual labour, and that in each case the result was so completely successful as to restore that power, and to enable the one to obtain her own livelihood; and the other to support himself and family. "Resection of the heads of the phalanges of the fingers," is, therefore, *in appropriate cases*, a procedure in every way *worthy of attention and adoption*.

As my object, upon the present occasion, is chiefly to draw attention to the success attending this mode of treating injuries, &c., of the fingers, and not to discuss any particular mode of removing the diseased parts, I shall not enter further upon this latter subject than to observe it must be evident the method adopted to expose the bone will of necessity vary in each case, as exemplified in those I have narrated; but, I cannot refrain from advising that the removal of the bone itself, *the essential part of the operation*, should be *always* made by a very fine (watchespring) saw, and not with the bone-nippers, inasmuch as however sharp the bone-nippers may be, they of necessity inflict an amount of *compression* upon the healthy bone, which, considering the small size of the shaft of the phalanges, would be sufficient to prevent the ready healing of the parts, and thereby tend to frustrate the end proposed by the operation.

In the three operations I have described, two consisted of a removal of a portion of one bone only—viz., of the head of the second phalanx; and one of a portion of the articular extremity of each of the bones composing the joint. In this latter instance the bones have become nearly ankylosed; in the former, they permit of a slight degree of motion. This difference in the termination in the two fingers of the same case brings us back to the consideration of the point mooted in the words "in the treatment a false joint will be sought for" as formerly quoted, and it likewise gives rise to the following practical questions—viz., 1st, whether a false or an ankylosed joint in these cases is to be preferred; and, 2ndly, whether it is within our own power to secure the one or the other.

The reply to the first of these questions will, doubtless, be different in different cases and under different circumstances in life; for my own part, and as regards the man whose case I

have narrated, I think it would have been better for him if both the joints had been fixed. In the case of the girl, there is positive motion, but I do not consider it of sufficient *extent* and *freedom* to weigh much in the consideration of the matter. There is, however, another consideration of far greater importance—viz., the angle at which the joint is allowed to become fixed.

With respect to the second question, I am of opinion that it is in our power to secure either the one or the other.

It is manifestly obvious that this operation—"resection of the heads of the phalanges"—is only adapted to those case in which it is the *articular extremity alone* of the phalanges which requires to be removed, and in such cases the less removed, so that it answers the end proposed, the better. There is, however, another class of cases of much more frequent occurrence, which borders so closely upon the former, that I am led to append a few observations upon it, and the more so as I am not aware that the practice I would advise is noticed in surgical works. I allude to the carious condition of the *extremity and shaft* of the ungual phalanx, which is so frequently seen as the result of neglected or wrongly treated onychia; and, occasionally, of mechanical injury to the finger, which I have been in the habit for years of treating by "excision of the entire ungual phalanx."

Such cases as these are of frequent occurrence amongst the out-patients of hospitals generally. I have had many such at the Metropolitan Free Hospital. When such a case is first seen, the end of the finger is considerably enlarged, there is an opening on the front or side, leading down to the carious bone, which can be immediately detected by a probe; and from the orifice of the wound, in all probability, a rugged portion of the diseased flexor tendon will be seen to protrude. If such a case as this is left to itself, and if the caries is confined to the ungual phalanx alone, after a considerable period of time (weeks upon weeks), the lateral ligaments become destroyed, and the bone comes away of itself; or if, on the other hand, the caries passes to the head and shaft of the second phalanx, it then causes the loss of the entire finger.

The treatment that I would recommend as the result of my own personal experience is the early removal of the carious ungual phalanx. This proceeding is easily accomplished by enlarging the existing wound, and dividing the lateral ligaments. After the removal of the bone, the sides of the tip of the finger should be slightly compressed against each other, and the part retained in an extended position. The wound will heal readily, and in this process it will be found that the finger-nail will be drawn down more or less across the top of the finger, according to the amount of destruction the parts in the front of the finger have undergone by the previous sloughing. This drawing down of the finger-nail is, without any doubt, unsightly; but however unsightly it may be, or to whatever extent the nail may be drawn down, and I have seen it in cases that have been left to themselves nearly flat on the end of the finger, the extremity of the finger possesses a degree of sensibility, and the entire finger a degree of mobility that is not possessed by the extremity of the stump of a finger mutilated by amputation, and it is, therefore, of infinitely more service to a patient in any circumstances of life, but especially so to the poor and labouring man.

Old Broad-street, Feb. 23rd, 1858.

ON THE TREATMENT OF DYSENTERY BY THE ADMINISTRATION OF LARGE DOSES OF IPECACUANHA.

By E. S. DOCKER, Esq.,

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IN no part of the world, probably, does dysentery prevail more extensively, or with greater severity, than in the island of Mauritius, and nowhere is it more fatal. As surgeon of the 5th Fusiliers I was stationed there nearly six years, and had therefore ample opportunity of becoming acquainted with this hitherto intractable and fatal disease. I say "hitherto," as it is my firm belief that, henceforward, dysentery may be as much under control and as expeditiously cured as simple diarrhoea.

For the greater part of the above period—viz., from 1851 to

1857—I had availed myself of the remedies in general use. At last, disheartened with my ill success in several bad cases, wherein I had perseveringly but ineffectually employed the *secundum artem* treatment, and remembering to have somewhere seen it mentioned that the powdered root of ipecacuan, in large doses, had been given with great effect in this complaint, I became anxious to make trial of an agent declared to be of such extraordinary efficacy.

I have tested this medicine in cases of every kind and degree. Out of upwards of fifty cases of dysentery I lost but one (in former years the mortality ranged from ten to eighteen per cent.); and in the instance in question death was caused by abscess in the liver: the primary disease had been not only cured, but very thoroughly cured, as I shall hereafter show. I must observe that I had at one time been in the habit of prescribing ipecacuan in the small doses recommended by Mr. Twining; but so ineffective was it when thus administered,—excepting in cases of no great severity, wherein other medicines answered as well, *without* the inconvenience of nauseating,—that I had long ceased to employ it. On resuming the use of ipecacuan, I gave it in doses ranging from ten to ninety grains; rarely less than twenty grains. The larger quantity was given in urgent cases only, the ordinary dose being a scruple or half a drachm. The action of these large doses is certain, speedy, and complete; and truly surprising are sometimes their effects. In no single instance has failure attended this medicine, thus employed. I am not, of course, sufficiently sanguine to expect that it will invariably succeed; but of this I am convinced, that it will effect a complete cure in an immense majority of instances.

In all constitutions, robust as well as delicate, under all circumstances, the result is the same. In the very worst cases, when the strength of the patient is almost exhausted, after the whole range of remedies has been tried in vain, the disease running its course swiftly and surely to a fatal issue, ninety grains of ipecacuan have been given, and forthwith the character of the disease, or, I should rather say, the character of the *symptoms* has been entirely changed; for the disease itself is literally cured, put a summary stop to, driven out. The evacuations, from being of the worst kind seen in dysentery, have, not gradually, not by any degrees, however rapid, changed for the better; they have ceased at once, completely. There has been no inclination even to stool for twenty-four or thirty-six hours, the patient all the time in a state of delightful ease and freedom from pain; then at last, without aid of any kind, a perfectly natural, healthy evacuation, all irritation, pain, and tenesmus having at the same time entirely ceased.

Nor is there the disposition to relapse so common in acute dysentery. I have not observed what may be termed a true relapse in any instance. If the patient contracts dysentery again, he does so *de novo*. All that remains—the medicine having cut short the disease—is for the patient to recover strength; and this quickly follows, without any extraordinary care as regards diet and regimen, so indispensable and requiring such nicety of management in convalescence from dysentery generally. The usual necessity, moreover, for after treatment, in the shape of a long course of astringents, &c., is in most cases entirely obviated, a few doses of some vegetable tonic being all that is needed.

It may be asked by what means the stomach is enabled to retain such large doses of an emetic substance. The course I have generally adopted is as follows:—In the first place, a sinapism is applied over the region of the stomach, and simultaneously a draught given containing a drachm of laudanum. Half an hour after, when the sensibility of the stomach has been, by the action of the opium and counter-irritant, as much as possible diminished, and the patient's attention is occupied with the sinapism or by conversation, the ipecacuan is administered—generally in a draught, sometimes in the form of pill or bolus—and the semi-recumbent posture steadily maintained. In a considerable proportion of cases, the medicine is not rejected, or it is at least retained long enough to enable it to do its work. If necessary, I repeat it till the stomach does retain it. I never yet have been obliged to give it in the form of enema. Where so considerable a dose as sixty or ninety grains has been administered, I in general wait ten or twelve hours before giving another. Should the bowels, however, not meanwhile have acted, a repetition is not generally required. I ought here to mention that I begin the treatment of dysentery, in most cases, with an emetic—always with a thorough clearance of the bowels.

To those acquainted with tropical dysentery, the facts I have stated relative to the action of large doses of ipecacuan may appear almost incredible; the following cases, however, all of