CLINICAL NOTES.

BY

ALEXANDER JAMES, M.D.

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CLINICAL NOTES.

In the performance of my duties as clinical medicine tutor for the Ordinary Physicians of the Royal Infirmary, I have opportunities of studying disease in almost all its forms, and through the kindness of those gentlemen I have been enabled from time to time to make a special study of cases of unusual interest. As some of the results of this, I beg to lay before you the following paper:—[1]

I. Tendon Reflex and Clonus Phenomena.

The first case is that of a patient, Kellie, æt. 30, in Dr Balfour's wards. He has the well-marked symptoms of lateral sclerosis, *i.e.*, loss of voluntary power over the muscles of the legs, which are, however, very well nourished, and when he attempts to perform any movement with them he fails almost entirely, owing to the spasms which occur. There is, in addition, some loss of the cutaneous sensibility about the feet and ankles, and this, along with the lightning pains which he complains of, leads us to believe that other parts of the cord are involved. As a rule the cutaneous reflexes are diminished and the deep increased. It is the latter and the clonus which I now wish to discuss.

The ankle clonus is well-marked in both legs (especially the left), either on sudden passive tension or on tapping the muscle or tendon after gradual passive tension. In the left leg the "front tap" contraction can usually be easily induced. By means of tambours, one placed in connexion with the foot and the other with a revolving cylinder, I have obtained tracings of the clonus, and by the chronograph I find the contractions to occur at the rate of about 6·8 per second. The movement is, as usual, perfectly uniform. By grasping the foot and suddenly pressing it inwards or outwards a lateral clonus, due to clonic spasm of the peronei or tibialis posticus can be induced. The "toe clonus" I have not been able to excite.

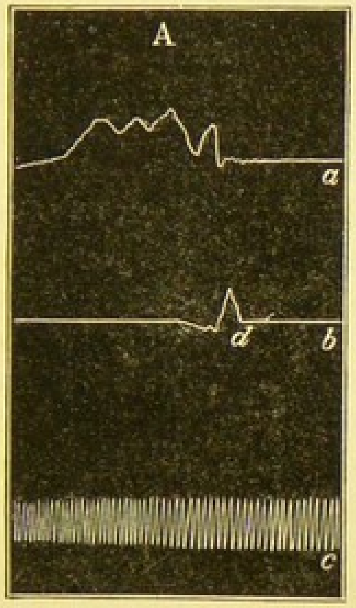
On getting the patient to stand erect, it is found that in a certain position of the leg as regards the trunk (the significance

[1] It was intended that these notes should be read before the Medico-Chirurgical Society of Edinburgh previous to publication. Want of time prevented this being done.

of which will be denoted by-and-by) a clonus of the gluteal muscles can be induced on stimulating them by sudden pressure. Its tracing shows it to be quite uniform, the time being about nine contractions per second.

I have also been able to obtain tracings of the "knee clonus," but this I propose to discuss in a more detailed manner. In his article on "Tendon Reflex Phenomena," in the *Medico-Chirurgical Transactions* for 1879, Dr Gowers notes that he has met with this knee clonus in two cases in which the patellar tendon reflex was very well marked. It differed from the ankle clonus in both cases, being slower in time, only about two and a half contractions occurring per second. Dr Gowers explains this difference between the intervals of contraction in the knee and ankle clonus by regarding the former as a truly reflex, and the latter as a direct contraction, and in support of this theory he notes that the intervals between the application of the stimulus and the contraction of the muscles are different. As regards the knee reflex, he has found that the interval between the tap on the patella and the contraction of the quadriceps is on the average ·10 or ·11 second, whilst in the ankle the interval between the tap on the muscle, or front tap, and the contraction of the gastrocnemius he found to be only about ·035 or ·04 seconds. Thus he says, "The proportion between the frequency of the ankle clonus and the frequency of the knee clonus is nearly as four to ten, and the proportion between the interval which intervenes between a tap on the Achilles tendon and the contraction of the gastrocnemius is to the interval between the tap on the patellar tendon and the contraction of the thigh muscles also as four to ten." He therefore concludes that in the case of the knee clonus the several contractions are of true reflex origin, that "the sudden tension of the quadriceps by the weight of the extended leg on the fibres after the cessation of one contraction probably constitutes the afferent stimulus for the next," whilst in the case of the ankle clonus he concludes that the clonic contractions are the result of direct stimulation, the muscle being in a state of extreme irritability developed in a reflex manner as the result of the passive flexion of the ankle-joint.

In the cases which I have been studying I have never been able to induce knee clonus by a stimulus applied to the muscle or tendon, the limb being in the position in which the patellar tendon reflex is usually induced, *i.e.*, the patient sitting, and the legs hanging at right angles to the thigh; but what suggested itself to me was to place the quadriceps muscle under the same conditions as is the gastrocnemius when clonus can be induced in it, *i.e.*, in a state of passive tension. On strongly flexing the leg on the thigh, and so stretching the quadriceps, a tap on the tendon or muscle, however, produces neither reflex nor clonus, but I found that with the leg slightly flexed (about 25° to the line of thigh) a tap on the tendon usually produced a well-marked clonic spasm. This, however, although quite distinct, never lasted long. The greatest number of contractions I estimated to have been about twelve; usually they did not exceed four or five. I was able to obtain several tracings of this clonus, of which A is a specimen.



a, Tracing of knee clonus; *b* at *d* marks where the tendon was tapped; *c*, Chronograph tracing 50 per second.

Tracing of patellar tendon reflex, where leg hanging over edge of bed, to show initial slight fall. The writing needle was flexed to the leg about three inches above the ankle.

The upper line shows the clonus, the middle the instant at which the tendon was tapped, and the lower is the chronograph tracing. The clonus tracing was obtained by placing the button of the receiving tambour in contact with the quadriceps muscle or tendon, close to the knee-joint, and is the only one of the three for the comprehension of which more detailed description is necessary. It will be noticed that in all there is at first a slight fall, and that this is followed by a rise showing usually four distinct undulations. The primary slight fall I believe to be due to the fact that at the moment of contraction of the quadriceps there is not only a raising of the leg on the knee-joint, but also a slight downward movement of the distal end of the thigh. The extent of this downward movement will for physical reasons be greater when the leg is slightly flexed as regards the thigh, than when it is at right angles, as obtains when the patient is sitting with his legs hanging over the edge of a bed or table; but even under these latter circumstances a tracing of the knee reflex shows an initial slight fall (*see* B). The rise and its undulations are, of course, caused by the clonic contractions, the muscle not undergoing complete relaxations during the intervals. On studying this tracing with reference to the periods of stimulation and the chronograph vibrations, we find that the contractions occur at the rate of about seven per second. This is slightly more frequent than the ankle clonus, and is quite different from the knee clonus as described by Gowers. We further find that the interval between the tap on the patellar tendon and the contraction of the quadriceps is about ·025 second. This is about the same as Gowers has found in the ankle reflex, but is different from what he has found in the case of the knee. The conclusion is that (in this patient at anyrate) the knee clonus is just the same in mode of production as the ankle, and probably also the hip, although in the latter I have not been able to estimate the interval which elapses between the application of the stimulus and the occurrence of the contraction.

But several other important considerations at once present themselves. Why is it that in lateral sclerosis this clonus can be so readily elicited in connexion with the different muscles? Gowers explains the action of a stimulus in producing it by considering that the "passive extension leads to a reflex irritability, a hair-trigger susceptibility to local influences, an incipient contraction excited to developed contraction with extreme readiness, and that this irritability is at its maximum after the cessation of a previous contraction."[1]

But have we grounds for considering that the passive extension is the cause of this reflex irritability? In the case of the ankle it might seem to apply. Here it is found that clonus, readily induced when the muscle is tense, can be stopped at once on relaxing it. But if tension of muscle were the sole cause, clonus should be most readily induced when the tension is greatest, as, for example, on pushing up the foot with the leg perfectly straight. In this case, however, as in others which I have observed, and as noted by Gowers (*Spinal Diseases*, p. 23), it occurs most readily when the leg is slightly flexed.

In the case of the knee it will certainly not apply, for here the clonus can be induced only when the leg is but slightly flexed, and when, consequently, the quadriceps is rather relaxed than tense.

What next suggests itself is, that this so-called "irritability" of the muscles may be in some way connected with the co-ordinated movements which the limbs have to perform. For example, in walking, in making the step from one foot to the other, we first cause contraction of the gastrocnemius of the rearmost leg, by which the heel is raised. Then, the leg being slightly flexed on the thigh, we contract the quadriceps, and so bring forward the foot; then, having placed the foot in front, we find, as we begin to bear on it, that the gluteal muscles contract. Now, it will be found in the case under consideration that it is precisely when the limb is placed in the positions in which it is when any of these muscles are contracting that clonus of them can be induced.

Further, that this "irritability" is specially manifested at certain

[1] *Medico-Chirurgical Transactions*, 1879, p. 300.

definite positions of the limb, rather than due to tension of the muscles concerned, can be shown by experiment on ourselves. On testing this we find that clonus of the gastrocnemius can be most readily (if not only) induced when sitting on the edge of a chair, with the foot resting on the ball; and we find also, that as regards the knee, we can most readily induce it with the leg slightly flexed on the thigh, and the toes drawn up. Clonus of the muscles of the upper arm can be best induced voluntarily when the arm is semiflexed. I have obtained tracings of these from five healthy adults, and in two of these I was also able to obtain tracings of a "head clonus." The numbers of contractions per second of these I give in the following table:—

	Ankle.	Knee.	Arm.	Head.
A.	7·4 per sec.	8·4 per sec.	9·5 per sec.	
B.	8·0 „	7·3 „	8·8 „	
C.	6·2 „	7·0 „	7·5 „	12·4 per sec.
D.	6·0 „	7·6 „	9·5 „	13·0 „
E.	6·7 „	6·5 „	8·4 „	

I have also endeavoured to discover if the number of contractions can be varied at will, but have concluded that if this occurs at all it can only be to a very slight extent. Thus, in the arm clonus of A. (my own case), I found that by the greatest effort I could produce 10 contractions per second, whilst in ordinary circumstances 9·5 is the number. In the ankle clonus of B. I found that on his being asked to increase the number of contractions there was really a diminution from about 8·6 to 8, whilst in C. there was in similar circumstances an increase from 6·2 to 6·5 per second.

And now, having so far considered the phenomena of clonus, can we say any more as regards its production in health and disease? The facts (1) that it occurs most markedly in certain definite positions of the limbs; (2) that in these positions it can be set agoing voluntarily, and that when so it seems in most individuals to continue for a time, to a certain extent, independently of the will; (3) that the rate of contraction cannot be materially altered by the will; and (4) that in lateral sclerosis it can be set agoing by peripheral stimulation when the limb is placed in one or other of certain definite positions,—suggests to us, I think, some ideas as regards the nature of its production.

We know that the nervous axis is formed of cells and of fibres, afferent, efferent, and those connecting cells, and we know that on peripheral irritation afferent impulses are carried along certain tracts and cause stimulation of certain cells, and that by these, impulses are generated which pass along definite efferent tracts. We further know that for the proper performance of any movement the muscles employed must be co-ordinated, *i.e.*, when some, singly or in groups, are contracting, others must be relaxing, and that these must alternate as regards contraction and relaxation. All this implies that in the nerve centres at one time one set of cells must be stimulated, at another time another set, that the resulting motor impulses must travel now along one tract of fibres, now along another. When, therefore, we consider the complicated movements which we daily unconsciously perform, and the even more complex processes in the nerve centres, etc., which these necessitate, we can, I think, more easily understand how clonus occurs, and why most markedly or solely in certain definite positions. Thus, in ankle clonus, we may suppose that, with the gastrocnemius somewhat contracted so as to raise the heel, and the leg somewhat flexed on the thigh, the effect of a stimulus applied to the muscle will, like a wave, be carried to cells in the cord, and that the impulse there generated will be carried from these back again to the muscle as a stimulus to contraction, *without loss by escape along other tracts to other cells or muscles.* And we are the more inclined to believe this when we remember that, in the act of walking, impulses must pass along certain definite tracts without escape thousands of times every day. Similarly with the knee clonus, arm clonus, etc.

Conversely, we may suppose that the fact of its being difficult or impossible to excite clonus in muscles when the limbs are not in such positions, even although the muscle concerned may be in a state of greater passive tension than before, is due either to increased resistance in the appropriate nerve channels, or to the escape or overflow of the impulse to others.

I therefore would suggest that the position of the limb as affecting through the muscle the nerve tracts and centres in the cord is, rather than tension of muscle, the important item in the production of clonus.

But how is it that if, in a case of lateral sclerosis, we place the limb in a certain position and apply a certain stimulus peripherally, we excite a clonus which will last a longer or shorter time, and which is beyond the control of the will; whilst in health, in like circumstances, this either does not occur at all, or, if it does, only to a very limited extent?

Before we attempt to discuss what may be said in answer to this query, let us see if there is nothing else of interest to be derived from a study of these tracings of healthy clonus.

On the justifiable assumption that for each contraction there must be the passage of an impulse from the muscle to the centre in the cord and back to the muscle, we should expect to find that in the clonus of muscles distant from the cord the intervals between the contractions would be greater than in those in closer proximity to it, as in the former a greater length of nerve has to be traversed. A reference to the notes of the tracings does, I think, bear this out. In the head clonus, in the cases in which it could be obtained, the number of contractions per second was the greatest, and the arm clonus was next in number to this. In three out of the five cases the knee clonus was more rapid than the ankle. In the two cases in which the knee clonus was the slower, very great difficulty was experienced in producing it as compared with the ankle clonus, and this I believe to be the explanation of their being exceptional.

I further endeavoured to obtain data on which to form an estimate of the distances which the nerve impulses must travel in the case of the different muscles, and I found that in a man 5 feet 11 inches in height the distance from the 12th D. V. to the middle of the leg was 42 inches, and to the middle of the thigh 27 inches. From the 5th C. V. to the middle of the upper arm the distance was about 20 inches. Seeing, then, that the distances to be travelled are so unequal, and that the differences between the intervals of contraction are often comparatively slight, we may conclude that the great proportion of the time is taken up at the peripheral and central terminations of the nerves, and not in their course, and this further leads us to expect such exceptions as the two cases of knee clonus before mentioned.

Note further that in the case of lateral sclerosis which formed the subject of these notes the ankle clonus was 6·8, the knee 7, and the hip 9 contractions per second.

Can any relation be traced between the clonus of health or disease and the normal muscle contraction? The latter is, as evidenced by the muscle tone, caused by successive stimuli, and according to Helmholtz and others these number 19·5 per second. As to this being the same for all muscles, there is, I think, some room for doubt. I have examined and compared the "muscle tone" in the corresponding muscles (biceps) of boys and men, and in different muscles (masseter, biceps, gastrocnemius) in individual cases, and have concluded that the farther distant they are from their nerve-centres in the cord, the lower is the pitch of the tone resulting from their contraction.[1] Should I be correct in supposing that this, like the clonus, is explained by the differences in the distance along which the nerve impulses have to travel, the connexion between clonus and normal muscle contraction is evident. Thus in A. the arm clonus was 9·5, whilst the normal stimuli to the upper arm follow one another at (as nearly as necessary for our argument) double that rate. We have only to suppose, then, that the difference between normal bracing of the muscles of the upper arm and clonus is that in the former the stimuli pass simultaneously to biceps and triceps muscles, whilst in the latter they pass alternately. Similarly with the thigh and gastrocnemius, the fewer contractions per second of the clonus corresponding with the lower-pitched muscle tone.

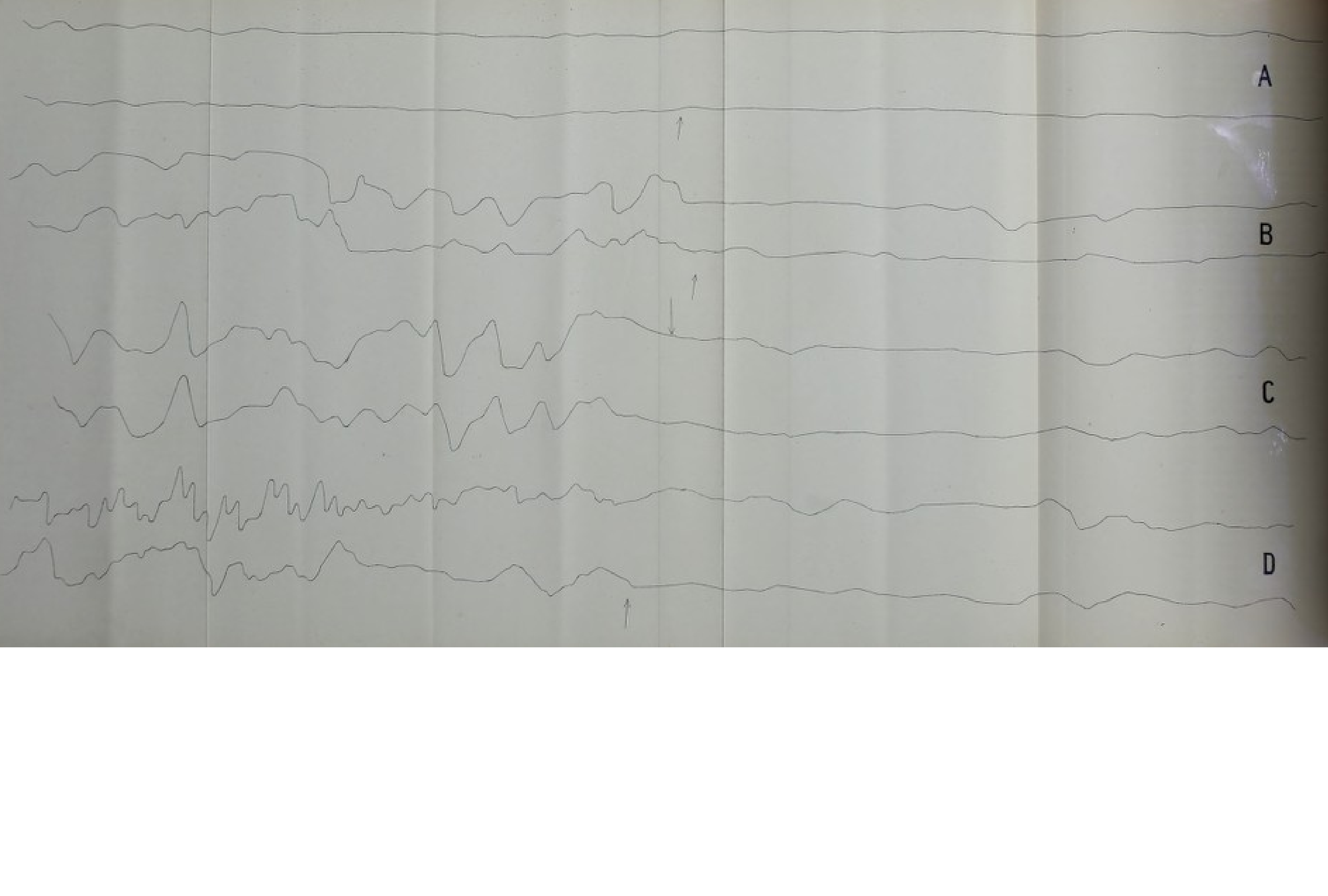
And now let us return to the query which we left unanswered

[1] It might be said that the size of the muscle had to do with the different pitch of the muscle tones. It may be an argument against this to mention that the toe clonus described by Dr Gowers (*Med.-Chir. Trans.*, vol. lxii. p. 288) was about the same in rate as that of the gastrocnemius. Compare Herbert Spencer's paper on the *Physiology of Laughter*, and also his *First Principles*, 3d edition, p. 238.

at page 8. Why is it that in lateral sclerosis clonus can, under certain conditions, be readily induced by peripheral stimulation, and will go on independently of, and to a great extent in direct opposition to, the will, whilst in health this does not occur? The theory that it is due to an irritability of the reflex mechanisms in the cord, though it does not give us much information, is conceivable enough, for we may suppose that just as sclerosis of the posterior columns manifests itself to the cells in the brain by lightning pains, etc., so in lateral sclerosis the condition of the cells in the anterior cornua cannot be one of continued quiescence. But there are other considerations. It is well known that when the brain of a frog is removed, reflex action is developed to a much greater extent than in the entire animal; and many interesting experiments might be mentioned to show that stimulation of the optic lobe will restrain or inhibit this function. It has, therefore, been concluded that in the optic lobes in the frog, and there or thereabout in other animals, there is an inhibitory centre. This theory, however, although useful to us in many ways in the present state of our knowledge, can never be regarded as an exact explanation of the phenomena. Apart from the experimental evidence against it, *e.g.*, that stimulation of any afferent nerve will, in the absence of cerebrum, optic lobes, and medulla, cause inhibition of reflex action, we must remember that, for general reasons, an increase of reflex action on removal of the brain is to be expected. In the entire frog the effects of a peripheral stimulus are carried in part to the motor centres in the cord, and in part to the brain; in the former resulting in motion, in the latter in consciousness. In the decapitated frog, on the other hand, the nerve channels leading to the brain are cut, and hence the effects of the peripheral stimulus are manifested as motion alone. This suggests a very simple method by which what may be called the "mechanical equivalent" of consciousness may be obtained. To do so we have only to connect a muscle—say the gastrocnemius of a frog—with a given weight, and to apply a stimulus of a given strength, so as to produce reflex contraction of that muscle. The difference in the height to which the weight is raised before and after decapitation will, *cæteris paribus*, yield this equivalent. I am, I trust, sufficiently acquainted with physiological experiments to know the value of the term *cæteris paribus*, and I shall not at present discuss this subject; but in support of these ideas as regards inhibition or inhibitory centres, I take the liberty of quoting the following from George Henry Lewes's *Physiology of Common Life*, vol. ii. p. 201:—

"What is the process of control? Every action is a response to a sensitive stimulus. Muscles are moved by motor nerves which issue from nerve centres; these nerve centres are excited by impressions carried there either by sensory nerves going from a sensitive surface, or by impressions communicated from some other centre. A stimulus applied to the skin excites a sensation,

The tracing reads from right to left. ↓ marks where eyes were shut.



which, being reflected on a muscle, excites a contraction. But, instead of the sensation exciting a muscle, it may be reflected on some nerve centre and excite a reflex feeling. This secondary or reflex sensation may either play upon a muscle or upon some other centre, and *this* will excite an action. Thus it is that the same external stimulus may issue in very different actions. We decapitate a frog, and half an hour after prick or pinch its leg; the frog hops or suddenly draws up its leg. We now prick or pinch an uninjured frog in the same way, and we mostly (not always) observe that its leg is motionless; it does not hop away, it only lowers its head and perhaps closes its eyes; a second pinch makes it hop away. In the decapitated frog the action was reflex; the stimulus transmitted from the skin to the spinal cord was directly answered by a contraction of the leg. In the uninjured frog the stimulus was also transmitted to the spinal cord, but from thence it ran upwards to the brain, exciting a reflex feeling of alarm, but, though alarmed, the animal was not forced into any definite course of action to secure escape; and while thus hesitating, a second prick came, and the urgency of the sensation then caused it to hop away. This hopping was reflex, but it was indirectly so; it was prompted by a reflex feeling, which, in turn, had been excited by the original sensation."

But will this increase in the reflex function of the cord which occurs when the brain is removed, on the above theory as to the nature of its production, account for the increased tendon reflex and clonus of lateral sclerosis? On the theory that they depend on the results of peripheral stimulation being kept within certain channels, it will, I think, go far towards doing so, for in this disease their passage towards the brain will be more or less blocked. The argument that the fibres of the lateral columns have a centrifugal and not centripetal function, and hence that their being sclerosed will not prevent the access to the brain of the effects of any peripheral stimulation, is to my mind hardly valid, inasmuch as a consideration of the evidence will, I think, show that the conduction of centrifugal impulses is not the only function of motor nerve fibres. The consideration of the muscular sense leads us to believe that they will in addition conduct centripetally; at any rate it will, I presume, be admitted that this theory is not more hypothetical than one which supposes them to conduct inhibitory impulses from a centre in the brain above to nerve cells in the cord below.

On testing the power of maintaining the equilibrium possessed by this patient, it is found to vary very much from time to time. When, as he says, "the jerkings have come on," it is almost impossible for him to stand without support; at other times he is able to do so with the feet a few inches apart, and even with the eyes shut. By means of the *taxograph* I obtained tracing *B*, and this may be looked upon as giving a fairly good idea of his equilibrium power in the absence of any clonic spasm. For the purpose of comparison I have added other three tracings—*A*, from a healthy individual; *C*, from a case of locomotor ataxy; and *D*, from a case of lateral sclerosis, showing the effect of the tremors on the tracing. It will be seen that Kellie's tracing, *B*, resembles that of the case of locomotor ataxy rather than of lateral sclerosis. But this, it must be noted, is in the absence of the spasms, and hence we may suppose that, apart from the disorders of equilibrium induced by these, this patient is ataxic. When the spasms come on Kellie's tracing at once resembles *D*. As in these circumstances, however, it is impossible for him to stand on the instrument for more than a few seconds without support, no tracing is given.

In concluding this section of my notes, I would draw attention to a probable bearing which it has on the subject of electro-therapeutics. In a paper on "Automatic and other Medical Electricity,"[1] Dr Imlach of Liverpool has described different forms of apparatus which he has devised, the purposes of which are so to act in stimulating the muscles of the limbs as to make them perform co-ordinated movements. Dr Imlach says (p. 259), "Take a hemiplegic patient, and, by automatic electric arrangement, make him raise the dragging limb as he walks, and stand as firmly upon it as upon the other; or make a paraplegic patient, in whom, for the present, walking is out of the question, rhythmically flex and extend his limbs by alternating electrization of his flexors and extensors. You do more than merely electrize the paralyzed muscles; his expectant attention is directed in turn to each moving limb, and an effort of the will is aroused as the oppressive sense of habitual inability is removed." If there is any truth in the suggestion brought forward in this paper (p. 8), that the position of the limb has, through the condition of the nerve channels, etc., in the cord, an important influence on the muscles as regards their capability of responding to stimuli, another important advantage of electro-therapeutics as carried out in this way is apparent.

II. Increased Vocal Resonance.

Into a discussion of the physical causes of brónchophony, and of the various conditions in which it is met with, I do not propose to enter. What I desire to do is to suggest a probable, and, as far as I know, as yet undescribed element in its production in cases of pleuritic effusion.

As the books tell us, and as we all have many opportunities of testing, an increase of the *vocal resonance*, and I may to this add *fremitus*, occurs in pleurisy only at certain parts of the chest, and at certain stages of the disease. The parts of the chest are those above the level of the fluid, and the stage of the disease is when the effusion has become partially absorbed. It is necessary, however, to note that, as regards both locality and stage, the books

[1] *Practitioner*, vol. xxiii., 1879.

do not altogether agree. Thus, as regards the former, they show a tendency to limit this to the posterior aspect of the chest.

Skoda.[1]—"The bronchial breathing and the weak bronchophony are in most cases heard between the lower angle of the scapula and the vertebræ, and somewhat above and below this line."

Walshe.[2]—"When the effusion is moderate, bronchophony may commonly be detected in the immediate vicinity of the larger bronchi between the scapulæ;" but, again, "Bronchophony may often be heard in the upper front regions when the effusion is sufficient to condense a considerable portion of the lung inferiorly."

Clapp.[3]—"Sometimes bronchophony above the level (of the fluid) or pectoriloquy (especially in pleuro-pneumonia or pleurisy with phthisis), heard best over the scapular and interscapular regions, on account of the usual situation of the compressed lung."

Guttmann.[4]—"Bronchophony is also heard when the lung is compressed by pleuritic exudation, but only at those parts of the chest at which the dense lung is in immediate contact with the chest wall, that is, posteriorly, between the vertebral column and the scapula, when the effusion is non-encysted."

As regards the stage of the disease, the cases in which I have observed it have been where the effusion is partially absorbed; but some of the books describe it as occurring in cases of "moderate effusion."

In a typical case the physical signs may in a general way be described as follows:—Below the "curved line" (which at this stage is beginning to show itself), absent fremitus, dull percussion, diminished breath sounds, and vocal resonance; above this, increased fremitus, somewhat high-pitched and often tympanitic percussion, weak indeterminate breath sounds, and increased vocal resonance.

What is the cause of this increase of fremitus and resonance? The usual explanation is, that as that portion of lung yielding those physical signs has lost a greater or less amount of air as the result of compression, it is more homogeneous than healthy lung tissue, and so is a better conductor of sound. While granting that relaxed lung tissue may act in this way, I wish now to show that there probably is another very important element in the production of these phenomena. To demonstrate it, let us first consider why the vocal resonance in health is so much less over the surface of the chest than over the trachea. On this matter Gee[5] says:—The lungs "are kept in a state of permanent openness or distention, which favours the conduction of sound along the air columns

[1] Markham's translation, p. 317. [2] *Diseases of the Lungs*, 4th edit., p. 129.
[3] *Auscultation and Percussion*, p. 55.
[4] Sydenham Society translation, p. 171.
[5] *Auscultation and Percussion*, 2d edit., p. 113.

within the tubes. But, on the other hand, the progressively increasing number of air tubes renders the sound they conduct to any given spot progressively weaker; the voice is no longer confined within a single cylindrical tube, but is spread out and diffused by an enormous number of minute diverging tubes having a total sectional area very much greater than that of the single tube whence they spring." We must also remember that the sound vibrations in the larger tubes will be carried in part directly through the lung substance, and however much they may lose in intensity through healthy lung tissue being a bad conductor of sound, they will be propagated in varying directions towards the surface of the lung. In this way, then, we may conclude that the vocal resonance as heard over any part of the chest wall is very much weaker than that heard over the trachea or larger tubes, because in the former the sound is very much less concentrated. There are, of course, other considerations; for example, we must remember the weakening due to the diminished reflective power of the smaller tubes, and also that the vicinity of the trachea or larger tubes will increase the resonance at certain parts of the chest. These, however, do not affect the main point as regards the cause of the weak chest resonance. To obtain an idea of what this loss in intensity may amount to, we have only to compare the sectional area of the tubes with the area of chest wall corresponding to lung tissue. The sectional area of the trachea is about $\frac{1}{2}$ sq. in., and of right and left bronchi about $\frac{3}{10}$ sq. in.; while that of the right and left chests each is about 140 sq. in. In this way, then, supposing the sound vibrations to radiate out from the large tubes to the chest wall as from the apex to the base of a cone, and remembering, also, the great extent of the inferior and internal surface of the lungs, we can imagine how great this diminution must be.

Let us now apply these considerations to our typical case of pleuritic effusion, and let us suppose that the left is the affected side. Here, owing to the partial absorption of the fluid, the bronchial tubes at the root of the lung are pervious, so sound vibrations occur in them. Those vibrations tend (either through the tubes or lung tissue) to pass to the surface of the chest, but they cannot do so in the same way as in health. Below, there is a surface of fluid upon which all those vibrations which tend to pass to the lower part of the chest will impinge, and from which they will be reflected. The direction in which this reflection will occur will be outwards and upwards, and hence the vibrations will be directed to the upper portion of the chest, and will reinforce those which pass out here directly. Thus will we have produced an increase of the resonance and fremitus, and from the following data a rough idea may be formed of the amount of that increase.

In our typical case let us suppose that the fluid with its "curved line" above is found by percussion to be behind at the level of the sixth rib, laterally about the third or fourth, and anteriorly about the fifth rib. This represents an area of about 60 sq. in., so here the vibrations, instead of passing out in varying directions so as to reach a surface having an area of about 140 sq. in., are concentrated and reach a surface having an area of (140 — 60) say 80 sq. in. There will thus be an increase of resonance and fremitus as compared with the normal, and we may suppose that, with patent bronchial tubes, the greater the amount of effusion the greater will be this increase. Practically we see this exemplified in the gradual diminution of fremitus and resonance as the fluid becomes more and more absorbed.

I therefore would suggest that, in this restriction of the passage outwards of sound vibrations to the upper portion of the chest, we have an important item in the production of increased fremitus and resonance in cases of pleuritic effusion.

III. Obliteration of the Intercostal Spaces.

In the wards I have had many opportunities of noting cases where, with a considerable amount of fluid in a pleural cavity, there existed neither any marked increase in the perimeter of the affected side, nor obliteration of the intercostal spaces; and in explaining such cases I am in the habit of quoting from Dr Gee[1] and Dr Douglas Powell.[2] I have also had opportunities of noting the closer association of increased circumference of the side and bulging intercostal spaces with cases of empyema than of serofibrinous effusion; and in empyema, and also in cases of lung tumour, I have noted a frequent occurrence of dropsy of the corresponding side of the trunk.

That in empyema the pressure signs should be so marked we may suppose is due to the more acute pleural inflammation producing at the same time a greater amount of paralysis of the underlying intercostal muscles and a greater intrapleural tension.[3] The œdema of the side of the trunk may of course be caused by direct pressure on veins, etc., but, apart from this, it may, I think, be due to the fact that the interference with the proper performance of the respiratory movements caused by empyema or lung tumour will impair to a greater or less extent the absorption of lymph. The influence of the respiratory movements on lymph absorption is well known, and Dybkowsky[4] has shown that fluid

[1] *Auscultation and Percussion*, p. 28.

[2] "On Some Effects of Lung Elasticity in Health and Disease."—*Med. Chir. Transactions*, vol. lix. p. 165.

[3] See "Transudations and Exudations."—*Med. Times and Gazette*, January 1880.

[4] *Ludwig's Arbeiten*, 1866: "Ueber Aufsangung und Absonderung der Pleurawand."

artificially introduced into the intercostal tissues finds its way into the pleural cavity as the result of the respiratory acts. This latter experiment is specially interesting in connexion with empyema, for here we have, in addition to interference with the respiratory acts, high intrathoracic tension and an altered condition of the pleural walls.

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