it did not appear to be gangrenous; but, a few hours after the operation, a copious discharge of thin faecal fluid escaped from the wound. The patient sank rapidly. At the after-death examination, the mucous membrane of the bowel was found completely ulcerated in a circle corresponding to the seat of stricture, and in two points the other coats had given way.

In the second case, the hernia was a recent one, and the period of strangulation two days; and in this case, too, two spots of ulceration perforating the bowel were found at the point which had been strangulated. The patient was a woman, aged 76.

In the third case, the hernia was also a recent one, and the period of strangulation four days. The patient, a young woman, was in a perfect state of collapse, with stercoraceous vomiting, and great tenderness over the belly, which was tympanitic. Forcible taxis and purgatives had been repeatedly tried before her admission into the hospital. The sac contained a quantity of bloody fluid, and the bowel was of a very dark colour, and mottled. The patient never rallied, and sank in twelve hours after the operation. Extensive peritonitis of a low form was found at the examination after death.

Excluding, then, the case in which death was evidently caused by internal hæmorrhage, I find that I have, in this hospital, operated upon twenty-one cases of strangulated femoral hernia, in all of which the sac was opened; and that out of these twenty-one cases, three have died—that is, at the rate of one case in seven.

Such results will more than bear comparison with the most favourable results published by the surgeons who so strongly advocate that the sac should not be opened in strangulated femoral hernia. True, the number of cases is small; and in a greater number of cases, I might perchance not be so fortunate as I have been; but that the sac should have been opened in these eighteen cases, and that they should all, notwithstanding this, have done well, proves, at any rate, that the real danger does not rest with this mode of operating.

In the after-treatment, shortly after the operation, I usually give some opium, the dose of which is regulated by the state of the patient; but, as a rule, I do not give more than one grain, and this is repeated, or not, according to circumstances. The bowels I make a point of not interfering with. You will have seen in the wards of this hospital many of the cases of hernia on which I have operated, in which there has been no action of the bowels for several days—a week I find to be a very common period among my cases, and yet the cases, as you know, have done well. This is a very different state of things from what it used to be some few years ago at this hospital, when a drachm of salts and some infusion of roses immediately after the operation, and every two hours, until the bowels had acted freely, was the rule.

But should any symptom attributable to the non-action of the bowels perchance arise after a few days, it is then advisable to give either an enema of warm water, or some castor oil, according to circumstances.

As to diet, that ought, as a rule, to be of a generous kind: fluid, until after the bowels have been freely open, and then a gradual return to the ordinary mode of living.

But I cannot conclude these few remarks, gentlemen, without once more impressing upon your minds the imperative necessity of operating without delay in strangulated femoral hernia, as often as you find that the taxis, fairly tried, has failed. Recollect that the great source of danger is in the strangulation, and that the less this is allowed to go on, the greater your chances of succeeding when obliged to operate. And this is a fact which you ought to be careful to stamp upon the memory of every person afflicted with femoral hernia.

Clinical Lectures

DELIVERED AT

CHARING CROSS HOSPITAL.

BY

HYDE SALTER, M.D., F.R.S.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS; LECTURER ON PHYSIOLOGY AND PATHOLOGY AT CHARING CROSS HOSPITAL MEDICAL SCHOOL; AND ASSISTANT-PHYSICIAN TO THE HOSPITAL.

LECTURE V.—ON THE STETHOSCOPE.

Principal Uses of the Stethoscope.—Rules for its Construction: 1, as to Material; 2, Length, Thickness, etc.; 3, Shape, etc., of Extremities—Practical Hints for its Manipulation—Method of Applying—Choice of Whereabouts to apply—Determining Limits of Sound—Auscultating Females and Children.

[Concluded from page 108.]

HAVING thus described to you what I consider the essentials of a good stethoscope, let me give you some practical hints for its manipulation.

1. *Method of applying and holding the instrument.* Choose your part—apex or base of heart, apex of lung, axilla, or what not—and if you are not particular to a quarter of an inch, select it with the view of avoiding inequalities, and securing the most complete and uninterrupted apposition. Then apply the stethoscope thus (fig. 5), holding it between your

Fig. 5.

first and middle finger of either hand, according to the ear you are going to listen with; then press it firmly, applying your pressure to the spreading part of the chest-end. In this way, all chance of obliquity and partial contact is avoided, and the direction of the parietes alone influences the direction of the instrument. Then, keeping the chest-end gently but firmly pressed against the chest, bring the ear carefully and exactly over the centre of the ear-piece; then, applying sufficient pressure to secure the firm contact of the pinna, so as to stop all

chinks and support the stethoscope, remove the fingers that hitherto had held it against the chest. That is the right way. What is the wrong? The right way is one; but, as in other things, the wrong ways are many. One wrong way is to hold the stethoscope near the ear-piece; another, to place it obliquely against the chest; another, to approach the stethoscope to the ear, when stooping to listen, as well as the ear to the stethoscope; another, to keep the fingers in contact with it after the ear is applied, and support unnecessary. If the stethoscope is held near the ear-piece, it is not at liberty to adjust its direction to the chest, its direction is regulated by the hand and not by the thoracic parietes; for in proportion to the distance from the chest-end that the stethoscope is held does its perfect adjustment to the chest's surface become difficult and a matter of chance. If it is held obliquely so that only one edge of the chest-end is in contact with the chest, and between the other and the skin there is a chink, the column of air in the stethoscope is not isolated, but is in communication with the external air, and that murmurous noise is generated, like that which one hears on applying the ear to a shell, and to which I have already referred. I have often seen this method of handling the instrument fatal to efficient auscultation, and have watched the auscultator listening with the greatest solemnity when he could not possibly have heard anything except the droning hum of an open stethoscope. Then again, when the auscultator is stooping to bring his ear to the stethoscope, he very often unconsciously brings the stethoscope to his ear, especially if he is holding it near the ear-piece, so that by the time the ear is adjusted the chest-end is tilting up on one edge, although it has just been applied quite flat. The only antidote to this is to press the stethoscope firmly against the chest, in the way I have mentioned, till the ear is fairly over the centre of the ear-piece, then press the ear firmly against it and keep the head fixed. Perhaps the commonest fault of all is holding the stethoscope in the hand while listening: this is perfectly unnecessary, for the pressure of the head against the instrument is quite sufficient to support it, and the continued application of the hand often produces disturbing adventitious friction-sounds.

2. In choosing the point at which you will listen, if the object of your search is such as to give you liberty of choice, choose those parts that are the least covered with muscle or fat. Sometimes half an inch will make all the difference. I have not unfrequently seen both medical practitioners and students listen on the thick edge of the *pectoralis* or *latissimus dorsi* when the movement of the stethoscope one inch or less would have thrown them into the axilla, with nothing but the thin digitations of the *serratus* between the stethoscope and the ribs. If, for example, you wish to ascertain the general character of the respiratory murmur, any part of the chest will, with certain qualifications, give you the information required; but no parts are so good as the axilla and the neighbourhood of the apices in front, because in none is the muscular and fatty integument thinner, and therefore in none is the sound clearer. This, however, involves a corollary; namely, that in estimating the standard of sound in different situations regard must be had to the amount of intervening fatty and muscular tissue in those particular situa-

tions. For an amount of indistinctness and obscurity of breath-sounds that would be abnormal if heard in the axilla, would be quite natural if heard through the female breast.

3. In endeavouring to ascertain the *limits* of a sound, work gradually towards it and then gradually from it; the former will give you the limits of its first appearance, the latter the point of its first diminution. But in estimating the extent of the seat of a sound by the limits of its audibility, the character of the sound must be regarded as well as its distribution; for a loud sound transgresses the limits of its generation more than a feeble one, a dry sound is heard further than a moist one, and a deep sound than one of high and slender pitch.

4. In auscultating a female's chest, if you should wish to apply your bare ear to the bare skin, (and I do not hesitate to say, that for getting the most perfect information it is sometimes extremely desirable that you should,) or if you want with your bare hand to examine a friction or souffle fremitus, always pave the way by first applying the stethoscope. A physician in search of a diagnosis is a being of no sex, he is a mere machine; a woman's bare breast is to him only so much integument intervening between him and the object of his interest; he sees twenty in a day, perhaps, and thinks no more of seeing a female chest than he does a female tongue. But it is not so with our patients; and a woman in undergoing a thorough and free auscultation does so at the sacrifice of a certain amount of instinctive sensitiveness and modesty. No doubt, no sensible or reasonable woman would for a moment hesitate, or balance feeling against thorough results; but still there *is* the feeling and it has to be overcome, and we cannot be too careful not to do violence to it. It may seem a mere trifle, a mere sentiment; but it is not, it is a most important element in our constitution, and plays a most important part in the social and moral system of our race. There is a very good French proverb to the effect that "Modesty is the rampart that nature has raised around virtue"; and a Latin one more generally and less happily expressed—"Pudore amisso omnis virtus ruit." And he would be a bad philosopher who would regard as a trifle a feeling normally universal, and in its nature respectable and beneficent.

5. There is nothing more difficult than using the stethoscope with children, (very young children I mean,) and successful auscultation with them often requires a good deal of strategy and *finesse*. Once set a child crying, and it is all over; you get nothing but the held breath and the roar. The difficulty is enhanced from the fact that different ages and different children require different management. Some children are afraid, and roar as soon as you produce a stethoscope, although they do not mind your applying your bare ear to their chests. Others enjoy it as a sort of game, and are immediately quiet and good when you apply the "trumpet" to them. Sometimes, therefore, it is better to use the stethoscope, and sometimes the bare ear; sometimes to shew the instrument, and sometimes to hide it. As a rule, older children are quieted and interested by the mystery of the stethoscope; the younger ones are afraid of it and resent it. My practice with infants is this: never commence by auscultating them; always occupy them while doing it; always listen to the back first; always listen with the bare ear before

applying the stethoscope; always warm the stethoscope before using it. I have known it of inestimable value as a toy, occupying the little patient in front, while I have been stealing a march on him in the rear and employing immediate auscultation without his knowledge.

Remarks

ON

THE MOLECULAR THEORY OF ORGANISATION:

IN REPLY TO DR. BEALE.

BY

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I AGREE with Professor Beale that there are few questions of greater interest to those who are investigating the principles upon which medicine is based, than a consideration of the laws governing the ultimate molecules of which every living organism is composed. I have great pleasure, therefore, in replying to his remarks; because, probably from want of clearness on my part, he has misapprehended the nature of the theory which, after long and mature consideration, I have ventured to place before the profession in the pages of the *Lancet*.

Dr. Beale thinks that, because I have described histolytic molecules as being *often* larger in size than histogenetic molecules, and as being *sometimes* associated with the *débris* of broken down textures, there is a discrepancy in my figuring the latter in disintegrated tubercle unconnected with such *débris*. But I have never stated that the one class of molecules may not exactly resemble the other; and a glance at the figures representing the development of *ascaris mystax* must prove the contrary. Moreover, as it is an essential part of my theory that the histolytic molecules of one period may become the histogenetic molecules of another, there is nothing contradictory in making one figure represent both. Dr. Beale says that I "surely will not maintain that lifeless particles become aggregated together and form a living mass." But tubercle is not lifeless, and is subject to the general law of organisation. As to when, how, and where life is communicated to matter, these are questions I need not now discuss.

Dr. Beale says I ignore the use of high powers and reproduce figures ten years old, drawn under a magnifying powers of 250 diameters linear, an enlargement far less than he and others employ in the present day. I have frequently used powers varying from 700 to 1200 diameters linear; but in rare cases have gained any advantage thereby. If a thing can be seen distinctly under a low power, we seldom see anything new by making it four times as big. Inexperienced persons, also, by the use of such high powers, are peculiarly liable to be led into error. For the purposes of illustration, therefore, I consider that in most cases (not all), a power of 250 times

linear is amply sufficient, and it has the great merit of saving space and expense in wood cutting.

Dr. Beale says that the term molecule can scarcely be properly applied to a body nearly as large as a blood-corpuscle. But I can see, say in milk, no difference in structure between the larger and smaller molecules. I have always maintained that, whatever powers are employed, you cannot reach the ultimate molecule; I am far, therefore, from disputing that many of them may be less than the twenty-thousandth of an inch, and still possess all their vital and physical properties.

In the formation of vibriones, I have certainly never seen two particles in the very act of uniting, any more than I presume Dr. Beale has seen them divide under his eye. Either view is a matter of inference. But if Dr. Beale has been fortunate enough to see them actually divide, this is no objection to my theory, as there is no reason why they may not form in one way and multiply in another. In the same way, having been produced by precipitation, they may increase by absorption or imbibition of nutritive fluids.

Dr. Beale thinks it an objection to my statement, that the formation of bone is connected with the molecular law of aggregation, because "the laminae of the Haversian system are formed layer *within* layer, while the layers of calculeous matter are deposited layer *upon* or *outside* layer". But molecular aggregation may take place equally from within or from without; and, as in bone the nourishing fluid is derived from vessels lining the Haversian canals, it is only from within that one could expect such aggregation to be produced.

Dr. Beale thinks there is some confusion in the definition of molecules, because they may be living and dead, simple and compound; may originate from others, and be precipitated from fluids; may be so large as the 1-4000th of an inch, and so small as not to have been reached by the highest magnifying power; and because they have different properties. I can see no inconsistency in all this. Other elementary parts—for example, fibres—possess equal differences. Cells also vary in size, origin, chemical composition, complexity, and properties; yet they are still cells. As to whether molecules of the same kind or molecules of different kinds become aggregated together, there can be no question that coalescence may occur in compound organisms, both between the same and between different molecules, although the former is the most common. When a minute particle of oil is precipitated in an albuminous fluid, there is aggregated round the former a layer of albumen; and so a simple is at once converted into a compound molecule.

I have thus endeavoured to answer all the topics contained in Professor Beale's communication, in which I have been unable to see either facts or arguments in any way opposed to the molecular theory of organisation. On the other hand, were it necessary, I could easily prove from the valuable facts Dr. Beale has placed on record, and from the laborious investigations which he has made into the structure and growth of the tissues, that he has himself furnished most powerful arguments in its support.

I would deprecate further discussion on this matter until the whole of my lectures are published, when I shall be most happy to argue the subject, in all courtesy, with any opponents who, like Dr. Beale, possess an earnest desire to elucidate the truth.