succumbs to poisoning caused by the absorption of the putrid products engendered by this gangrenous phlegmon". The experiment is of great interest, and, as Chauveau says, well worthy of the attention of the surgeon, but it is too widely different from ours to admit of comparison with it. To make this difference evident, all that is necessary is to refer to the professed purpose for which it was made. M. Chauveau's object was, to use his own expression, to investigate the action of "pus putride"—i. e., of stinking pus. Our object, on the contrary, was not to inquire into the properties of putrid liquids, but of the products of inflammation; and for this reason, the most fastidious care was used to avoid the introduction of the element of putridity at any stage of our experimental procedure, by preserving our liquids from the possibility of septic contamination. In the first inoculation, the "pyæmic liquid" (as M. Chauveau calls it) used was simply the product of an acute inflammation, produced by the subcutaneous injection of a chemical irritant. This liquid was introduced into the peritoneum of a guinea-pig in extremely small quantity, and produced there an intense peritonitis. It was the product of this peritonitis which exhibited that extraordinary virulence, which I sought to illustrate to the Society by the experiment shown. In describing the results, I applied to them the term *intensification*. To this term M. Chauveau objects, and reasonably enough, for he supposes that in using it I meant to imply that in the case of the guinea-pig which received the first product and yielded the second, the former was *intensified* by mere residence in the uninfamed peritoneum of a healthy animal. I need scarcely say that this was not what I meant. In using the word *intensification*, I meant to apply it, not to the liquids, but to the *process* which produced it; for obviously the infective liquid finally obtained is not the same with that which was first injected, but simply the product of the inflammation excited by it—i. e., of the peritonitis. Its virulence, its infective activity, depends not on the mode in which the inflammation originated, but on the actual intensity it had acquired during the forty-eight hours of its duration.

There is one other point in M. Chauveau's paper to which I would ask space to refer. He objects strongly to my statement "that the ordinary bacteria of putrefaction do not in themselves possess a toxic action, and that liquids containing them can be injected into the circulation of a living animal without producing any effect". The statement, as it stands, is perfectly correct. Bacteria of putrefactive origin, if cultivated in liquids which are themselves innocuous, can be injected into the veins without producing any disturbance. It was suggested that in all probability the same thing would hold good of the bacteria of infective origin; but it was not stated either that the organisms of an infective liquid would, if they could be separated from it, be innocuous or inactive, or that the liquid without its organisms would retain its activity. As a result of these considerations, the opinion was expressed that, although these living particles are probably the carriers of infection, there is as yet no sufficient ground for supposing that their existence is the essential condition of infectiveness of certain products of inflammation.

A CASE OF RESECTION OF THE KNEE FOR BONY ANKYLOSIS, WITH THE LIMB IN FAULTY POSITION.

By JAMES E. ADAMS, F.R.C.S.,
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CASES of bony ankylosis of the knee-joint with the bones at right angles to one another, are fortunately of somewhat rare occurrence; but it is evident, from the examination of specimens in the museums, that even among the few cases one is likely to meet with, there will be considerable variety in form and degree of union between the bones. It is therefore not sufficient to determine that any case is one of true bony ankylosis, but we must ascertain, as far as possible, what extent of the surfaces of the articular ends of the bones are in contact, how much new bone has been thrown out, etc., before determining how to proceed.

Dr. Little, in his article in Holmes' *System of Surgery*, does not, apparently, allude to any cases except such as can be relieved by subcutaneous chiselling, for he says, "that it (the subcutaneous section) cannot fail to become a standard operation, and is, in every respect, preferable to the operation of resection, when performed for mere ankylosis."

Now, the point I wish to bring forward is this; that there are certain cases of knee ankylosis which can easily be cured by the operation described by Dr. Little, and others in which anything short of an excision of a wedge-shaped piece is wholly useless.

Drawing No. 1* is intended to show a case of slight ankylosis with the anterior portions of the articular surfaces entire and separated by a wide

Fig. 1.

interval, and No. 2 represents such a case as mine, where all trace of the articular layers is destroyed, and the cancellous texture of one bone is continuous with that of the other, over the entire extent of the articular surface.

N. S., aged 14, a pale but fairly nourished boy, was admitted into the London Hospital in April last, with his right leg fixed at a right angle, the skin at the point of flexure marked by numerous cicatrices on all sides, some, especially those in the popliteal space, puckered in and adherent to the bones. There was absolutely no pain or tenderness; there was the most complete rigidity (under chloroform); the adjacent portions of the bones were expanded and rounded, so as to leave no interval or depression between them; the position of the patella could not with certainty be made out. The disorganisation of the joint occurred when he was one year and a half old, consequently the limb had been wholly useless for twelve years and a half; notwithstanding this, there was not much wasting and no appreciable shortening. On May 8th the operation was undertaken; a semicircular flap was raised from the front as in ordinary excision of the knee, and the original point of union carefully sought for, but no indication of it was found. A cut was then made with the saw in the direction of line 1 (Fig. 2), extending not quite through

Fig. 2.

the bone; this was joined by a second cut (line 2) meeting it about half an inch in front of the posterior surface; the piece between them was then removed, and the remaining portion broken by flexing the limb. The object of this was to avoid any possibility of wounding the popliteal artery, which, it was surmised, might be lying closer to the bones than natural (supposing the posterior ligament to be destroyed); moreover, it was expected that there might be some new bone thrown out at

* Both diagrams are intended to represent vertical sections through the bones.

the posterior surface, which could not be safely cleared away at one cut. After this, the ends of the bone were smoothed down with the chisel until the limb could be easily straightened; it was then put up on a back-splint, specially made for it, and the wound covered with wet lint. In the course of the repair there was a fair amount of suppuration, and a few abscesses required opening; at the end of eight weeks there was firm union. One noteworthy circumstance was, that, for several days after the operation, the boy was literally tortured by pain in the back, which I attributed to his being obliged to remain in a position which, for obvious reasons, he had not been in the habit of assuming. The limb is now firm and straight, with less than two inches of shortening; a few sinuses remain open, and the muscles require time to gain strength, but, as far as the mechanical part of the cure goes, it is complete.

[Since the above was written, the boy has made uninterrupted progress, the limb being strong and straight, and able to bear the weight of the body.]

ON THE ACTION OF THE INTERCOSTAL MUSCLES.

By ARTHUR RANSOME, M.D., M.A. Cantab.

IN the lectures on Human Myology recently delivered by Dr. Humphry before the Royal College of Surgeons, he has entered upon the much debated question of the action of the intercostal muscles, and has argued that both the external and internal sets of fibres act as levators of the ribs, and that they are both inspiratory in their function. Had the subject been treated mainly as one of observation or experiment, it is probable that no scope would have been found for doubting the truth of the conclusions arrived at by so able an observer; but the arguments used are, on the contrary, merely deductive, and are chiefly based upon geometrical reasoning.

In a vital problem of so complex a nature as this has proved to be, it seems needful to examine somewhat carefully any attempt to employ mathematical demonstration in its elucidation. It is seldom that physiological questions are so simple as to allow all their conditions to be included within the terms of a geometrical proposition, and it is rarely possible to limit the argument to one aspect only of the case; but this particular question is one of peculiar difficulty when thus approached. Dr. Humphry very fairly objects to the results obtained by Mr. Hutchinson, on the ground that the real conditions of the respiratory act are very imperfectly represented in the experiments to which he refers, in his work upon the Thorax in the *Cyclopedia of Anatomy and Physiology*. It would, however, be very difficult to avoid this accusation in any attempt at a simple explanation of the action; and I fear that it will be found that the data upon which Dr. Humphry's calculations are based are equally faulty in this respect. But, before giving instances of this defect, it may be well to notice for a moment one consideration, which has probably been allowed to have more weight in the controversy than it deserves.

An argument which has been much used in proof of the inspiratory action of both sets of intercostals, is to be found in Haller's remark that the upper ribs are the most fixed, and hence that any intercostal muscular fibres, whatever their direction, will necessarily draw the lower ribs upwards, the more movable being dragged towards the less movable parts. But it does not seem to have occurred to those who have been influenced by this argument that, although Haller's remark is true when the abdominal muscles are relaxed, and when the ribs are at rest, or after they have been drawn downwards, as in expiration, yet it is not true when these levers are in the position of extreme inspiration, and when the abdominal muscles begin to contract after inspiration. In this case, all the ribs below the first are just as free to move downwards as they were to move upwards when inspiration commenced. The argument, from the greater fixity of the upper ribs, thus goes for nothing, and if any depressor influence can be shown to be exerted by the internal intercostals, it can hardly be denied that they have free scope for their action. But I think that the geometrical reasoning employed by Dr. Humphry to prove the levator action of both the intercostal muscles will hardly counterbalance the direct observations of Hutchinson, Sibson, and Traube; and I venture to call his attention to the following defects in his proof.

1. Both in the text of the lectures (BRITISH MEDICAL JOURNAL, June 29th, 1872, p. 687) and in the foot-note (p. 688) it is assumed, probably for the sake of simplicity, that the ribs are semicircular; that they have only one chief motion, one of rotation about a sterno-vertebral axis; that they retain their parallelism during inspiration; and that the sternal and vertebral joints are fixed joints. Now, none of these assumptions are correct, and by this mode of stating the problem, the

backward arching of the spine in deep inspiration, and the very considerable movements of the sternum are neglected; the motion of each rib upon a horizontal axis passing through the costo-vertebral joints is unnoticed, although it permits much greater extent of movement than that about the sterno-vertebral axis;* and the proposition also ignores the third or vertical axis of movement, which permits the lateral or outward motion of the ribs, and causes a considerable separation of their ends. The independent action of each rib, which has been fully proved by both Mr. Hutchinson and Dr. Sibson, and which is corroborated by my own measurements, is not admitted into the problem; and no note is taken of the fact which I have recently ventured to point out, that the ribs are not rigid bars, but that they are bent inwards forcibly during extreme expiration. It is doubtless permitted to a mathematician to neglect conditions which, from their minute influence on the result, might safely be left out of the proposition, and he may at times take for granted formulae which may be more conveniently dealt with in a separate calculation; but I submit that none of the above mentioned facts come within these categories. I much doubt whether they are not enough to prevent any mathematical treatment of the subject whatever, and they certainly interfere directly with the reasoning employed by Dr. Humphry and his friend.

2. With regard to the "double action" which Mr. Hutchinson has attributed to the intercostales interni, I do not see in what way this conclusion is contradicted by the demonstration relating to fig. 5 (BRITISH MEDICAL JOURNAL, p. 687); since, if the line A B be drawn in the direction of an internal intercostal fibre, it will be shortened by depressing the ribs in the posterior part of the space, and lengthened by the same movement in the anterior half of the area; or, to use Dr. Humphry's method of illustration, the angle A D B will be increased by

the depression of the lines A E and C B, but the angle A' D' B' will be lessened by the depression of the lines A' E, C' B'.†

3. Again, it must be remarked that Mr. Hutchinson's straight-line figures, although they come short of strict truth, yet they give a less incorrect view of the area of operation of the internal intercostal fibres than the geometrical diagrams used in the lectures. Mr. Hutchinson's figures show the costal cartilages rising to meet the sternum at a point in the rib-circuit which is practically true to nature; but the semicircular lines representing ribs in the illustrations to the lectures, bring the points, at which the costal cartilages may be supposed to commence their rise, exactly midway between the sternum and the vertebral column, a circumstance which is never found in human beings. It is acknowledged that the internal fibres are levator in their action within the intercartilaginous area, and that they are absent behind the angles of the ribs—i. e., for about one-fourth of the true intercostal area, but there remain three-fourths of the space, in which the slope of the ribs is such that even Dr. Humphry would acknowledge their depressor or expiratory action, were it not that, in his opinion, this tendency is counterbalanced by the obliquity of the plane in which they act, owing to the widening diameter of the lower ribs. Unless this latter circumstance can be shown to be overwhelming in its effect, therefore, it must be allowed that the internal fibres in the whole of the true intercostal area are depressor muscles, and that they are expiratory in their function.

4. Now, the tendency of the increasing diameter of the lower ribs is indeed to diminish the depressing force of the internal muscle; but Dr. Humphry is hardly correct in stating that it increases the levator effect of the external intercostals, since it diminishes the obliquity of their fibres to the ribs in the same ratio as it diminishes that of the internal muscle. Its influence, however, upon either set of fibres is so small, even where the obliquity of the plane of the intercostal space is considerable, that it can interfere but very slightly with their special action; and in the lower regions of the chest, where the ordinary depressor action of the internal muscles would have most play, its effect

* In the case of the fifth rib, this joint sometimes allows its anterior extremity to rise vertically upwards to the extent of two inches.

† I would refer those who wish for direct proof of this "double action", to the conclusive experiments of Traube (*Gesammelte Beiträge*, pp. 160-164), from which he deduces (1) that the true intercostales interni are not rib-raisers, and (2) that the true intercartilaginei act during inspiration in difficult breathing.