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.

may be said to be imperceptible. The extent of the influence may be calculated with a fair approximation to accuracy by measuring the slope of an intercostal space, the distance between the two ribs, and the length of an intercostal fibre. The influence of the widening diameter of the ribs may then be regarded as measured either by the lessening of the angle at which the fibre is set in relation to the rib, or by the supposed shortening of the fibre itself.

It may be stated that when the distance between the ribs is 0.50 inch, and the length of an intercostal fibre is 0.62 inch (a supposition based upon the drawings of Cloquet and Quain), and when the slope of the intercostal space is even as much as 20 deg., the effect of this slant of the walls of the thorax upon the length of the fibre would be as 62 to 59, and the diminution in the obliquity of the intercostal fibre would only be about 4 deg. (53 deg. 45 min. to 57 deg. 55 min.).

I should, from my own observations upon the movements of points on the chest-wall, be inclined to assign a different effect to this increasing diameter of the lower ribs. I have found an amount of backward and forward thrust of the ribs in forced breathing, which is altogether unaccountable if the ribs be supposed to be rigid levers; and careful direct measurements with callipers show that during extreme expiration the ribs are distinctly drawn in, their antero-posterior diameters in some cases being lessened by as much as 0.5 inch. Now the most probable agents in effecting this indrawing of the ribs are the intercostal muscles, especially the internal fibres; and it may readily be seen that when they are all working together, and when both the abdominal muscles are contracted and the clavicles and upper ribs are fixed, the resultant of the combination of these forces will tend to draw the ribs inwards, especially in the upper regions of the chest, where the greatest amount of horizontal movement is actually found.

I dare not speak dogmatically upon a question respecting which the most able anatomists are at variance; but, from a review of the works of others, and from careful experiments of my own, it has seemed to me most probable that the intercostals have a very mixed function; that, when the upper ribs are fixed, both sets of fibres may act together as levator muscles; and again, when the upper ribs are released and the lower ones drawn down and fixed by the abdominal muscles, both sets of fibres may draw downwards, and in this latter case they both tend to draw the ribs directly inwards, and so constrict the chest. But the two groups of muscles may also act independently; and in that case, if the external intercostals act alone, they raise the ribs and separate their ends, being assisted in doing this by the true intercartilaginous muscles; but when the internal intercostals alone come into play, they draw the ribs downwards, bend them inwards, and press their anterior extremities towards one another.

A QUESTION CONCERNING HYDROPHOBIA.

By GEORGE F. BURDER, M.D., Physician to the Bristol General Hospital.

Is there Evidence sufficient to warrant us in connecting the Group of Symptoms which we term Hydrophobia with the Bite of a Rabid Animal?

Many will, no doubt, consider this question superfluous, and some will think it absurd. I venture to submit the following considerations as at least justifying its discussion.

1. The period of incubation which the received theory assumes—a period ranging from a few weeks up to twelve months or more—being entirely without parallel in the history of disease, demands very strong evidence for its acceptance.

2. In the infinite variety presented by spasmodic nervous diseases of idiopathic origin, cases occur which cannot easily be distinguished from hydrophobia. For proof of this I refer to the second of the two cases recorded in Watson's *Practice of Physic*, of which it is said that "several medical men of much sagacity and experience" believed the patient to be suffering from hysteria.

3. Authorities on dogs are not agreed as to what disease it is which, when engrafted upon man, develops into hydrophobia. Witness the recent controversy in the *Times*. Generally we are told that rabies in the dog produces hydrophobia in man. Not at all, says Mr. Grantley Berkeley; it is hydrophobia in dogs which produces hydrophobia in man. Canine rabies and canine hydrophobia are, according to Mr. Berkeley, two distinct diseases. The former is common, not attended with dread of water, capable of easy cure, not communicable to man. The latter is rare, marked by horror of water, communicable to man, and absolutely mortal to both man and dog. Mr. Berkeley, no doubt, writes from his own observation when he insists on the innocence and

curability of rabies. In his views respecting canine hydrophobia, one may be allowed to suspect an unconscious mental compromise with the popular theory.

4. There must always be in the community a very large number of persons, who within the previous twelve months have been either bitten or licked by a dog or cat; and these persons, no less than others, will be liable to idiopathic nervous disorders.

5. The idea of the connection between hydrophobia and the bite of an animal having once taken hold of the mind, evidence of a bite, if it cannot be discovered, will generally be invented. Every one knows how constantly facts are distorted to fit preconceived notions.

6. Notwithstanding the operation of the two last named causes, cases are every now and then reported in which it is admitted that no evidence of a bite could be supplied by either the patient or his friends. The first of the two cases which Sir Thomas Watson relates may fairly be said to come within this definition. The back of the patient's hand had been "struck" by the teeth of a terrier, but the testimony is positive that no wound had been inflicted.

7. In a large proportion of cases of hydrophobia, there is no proof whatever of the animal which gave the bite having been mad. In many cases it was a strange dog, of which nothing further was known. In others, the dog was undoubtedly ill, but was destroyed before the nature of the illness could be determined by a competent authority. It is worthy of note that in neither of Sir Thomas Watson's two cases is it shown that the dog was mad. In the first case the point is not referred to. In the second, the only evidence is that of the "mob of boys" who had been pelting and chasing the luckless hound.

8. The late Mr. Youatt was many times bitten by mad dogs, yet never contracted hydrophobia. He may, it is true, have been insusceptible of the disease, or the precautions which he took may have protected him. These considerations, however, while they weaken, do not destroy the value of the argument derived from his exemption.

9. It is in accordance with observed facts to hold that the force of imagination might be sufficient to develop the symptoms of hydrophobia in persons of a nervous temperament, who, having been bitten by an animal supposed to be mad, had been thereafter haunted by the apprehension of the terrible consequences believed to be impending. The occurrence of several cases in the same district about the same time might be so explained, and would be no more wonderful than many of the epidemics of imitative nervous disease with which the records of medicine abound.

10. In a discussion on the etiology of disease, it cannot be out of place to insist on the tendency of the human mind, in all times and places, to assign to every malady some outward and palpable cause. Epileptics were formerly "possessed of a devil". Witchcraft, in later times, has been made responsible for most of the ills that flesh is heir to. The untrained mind abhors a vacuum, and cannot endure not to know. Hence it happens that progress in pathology is marked not less by the exposure of old fallacies than by the development of new truths. In our own day some reputed causes of disease have been, or are being, abandoned, and perhaps others are destined to share the same fate.

IS HERNIA CURABLE?

By CARSTEN HOLTHOUSE, F.R.C.S.,
Surgeon to, and Lecturer on Surgery at, the Westminster Hospital.

IN the JOURNAL for October 12th, a "Metropolitan Surgeon" asks:

"1. Can any reader state, from his own personal knowledge, the circumstances of a case or cases of inguinal or femoral hernia (congenital or from other causes) cured permanently by the use of trusses? If cured, then in what manner? by closure of the 'rings', or by obliteration of the canals in consequence of the pressure of the truss-pads?"

"2. Has any demonstrator of anatomy in the dissecting-room had the opportunity of dissecting the subject of a radical cure of hernia—radically cured by truss-pad pressure or by operation?"

In reference to the first question, I shall be happy to refer the writer to several cases which have come under my notice of the radical cure of hernia by truss-pressure; and I imagine there can be few men in much practice, especially midwifery practice, who could not, from their own personal knowledge, testify to the frequency of cures of congenital inguinal hernia by this means. The familiar facts of the great frequency of hernia in infancy and its rareness in boyhood, surely point to the same conclusion. Thus, on referring to a Report of the City of London Truss Society, a copy of which, for the year 1869, I happen to have by me, I find that, of 7,628 patients who were supplied with trusses in that