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.,
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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