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

year, 885 were under 5 years of age; but only 229 between 5 and 10, and 215 between 10 and 15. Now, whence this falling off? Either the little patients must have died or been cured; that they have not died, at least in anything like the same proportion, may be inferred by comparing the number of children alive in London at these two quinquennial periods with the total population of London. I have no data at hand as to the population of London in 1869; but I cannot be far wrong in assuming that the proportion of the infant population under ten years of age to the total population, remained the same as it was at the census of 1851—this was 1 in 8.004 under 5 years of age, and 1 in 9.69 between 5 and 10; there are, therefore, almost as many children living between 5 and 10 years of age as there were under 5, yet those affected with hernia have diminished by nearly three-fourths. I hold, then, that there is incontestable evidence of the curability of congenital inguinal hernia in the child; and the evidence is almost as conclusive that it has been effected by the use of the truss.

As to the *modus operandi* of the cure of infantile congenital hernia, whether by closure of the rings or by obliteration of the canals in consequence of the pressure of the truss-pads, I am of opinion that the truss cures, not so much directly by its pressure, as indirectly, by preventing the escape of the viscus from the abdominal cavity. In these cases, Nature herself comes to our aid—a bargain is struck. If, says she to the surgeon, you will undertake to keep that little bit of bowel out of my way, I will undertake to close that small passage which I was engaged in doing till interrupted by the intruder. If this view be correct, the cure would result neither from the closure of the rings nor from the obliteration of the canals, but from a simple completion of that process which birth had interrupted.

The great frequency of inguinal hernia in infants is, in fact, due to a retardation of that natural process which we call the descent of the testis: the process has not been completed quite in time; birth has taken place a little too soon, and the vaginal canal which should have been closed and obliterated remains open. If birth could be delayed for a month, congenital hernia would probably be a rare occurrence. As regards the curability of non-congenital inguinal and of femoral hernia by truss-pressure, the evidence is also conclusive, though the occurrence is comparatively rare. This is what might be expected from the different conditions one has to deal with; for whereas in the congenital form of hernia we have seen there is merely retarded development, here we have to do with actual disease. There has been a giving way—a yielding or rupture, as was formally supposed—of a texture already fully formed; a stretching or elongation of this into a pouch (the hernial sac), and a gradual yet forcible separation of tissues which are normally united. As, then, the conditions are different, so is the mode of cure. In these cases, the truss-pad acts both directly and indirectly: directly, by its pressing together the walls of the sac and of the inguinal canal; and, indirectly, by preventing the entrance of the hernia, and so contributing to the natural tendency which the orifice of the sac has to contract whenever the herniated organs cease to act upon it. There is another mode of cure which I have witnessed more than once in cases of femoral hernia: the mouth of the sac becomes plugged by a piece of omentum, which the pressure of the truss-pad has caused to adhere, and thus the further descent of a hernia is prevented, while the original protrusion is effectually cut off from the peritoneal cavity.

The second question of "A Metropolitan Surgeon" I cannot answer; but I can give him the dissection of a case radically cured by operation. It is taken from the *New York Journal of Medicine* of March 1868.

"A German, forty-seven years of age, affected with a scrotal hernia of the right side, was operated on by Professor Carnochan after Dr. Rigg's method. On the 2nd of May, 1857, the operation was completely successful. Towards the latter end of July, pulmonic symptoms made their appearance; and, on the 9th of September, he died of tuberculosis of the lungs.

"*Post Mortem*, September 10th.—Upon opening the cavity of the peritoneum, the orifice to the hernial sac could not be traced, the internal ring being firmly closed around the cord. On the outer side of the peritoneum, and just below the situation of the internal ring, was found a small rounded body of a yellowish colour, supposed to have been the remains of the hernial sac. The upper portion of the inguinal canal, for nearly an inch, was closed by plastic exudation, which had become organised and somewhat fibrous in its appearance; while the canal at its lower part, and the external ring, were to appearance in their normal condition, though the cord throughout the entire length of the canal seemed to be imbedded in plastic formation."

It must be explained that Dr. Rigg's method of operating for the radical cure of hernia consists in the introduction of a skein of silk into the inguinal canal; and the skein of silk used in this case being too large for the puncture made by the instrument, it was not introduced more than one inch, which will explain the fact of the lower portion of

the canal and external ring being in their normal condition, while at its upper portion both the canal and internal ring were firmly closed.

IS HERNIA CURABLE?

By JOSEPH LAWRENCE, M.R.C.S.Eng., Bath.

IN answer to "Surgical Queries" of "A Metropolitan Surgeon" in the *JOURNAL* of the 12th instant, several cases of congenital hernia in children I have known to be radically cured by truss. To be refreshed in memory, I have to-day seen a little boy, five years old, in whom congenital inguinal hernia of right side was detected when he was a month old. Two well-fitting ordinary spring trusses were at once ordered, so that when one was wetted a dry one might be applied; and these were alternately worn night and day, with strict injunctions to the nurse to place her finger on the ring when removal for washing or changing was necessary, so that the bowel was never allowed to descend. New trusses were, of course, made from time to time as the child grew. This treatment was strictly adhered to until the child was two years old; the trusses were then omitted by night, but worn by day for another half-year; they were then discontinued; and, up to this date, two years and a half, there has been no return of the hernia.

On examining this little fellow to-day, I found a distinct depression and absence of the fatty deposit where the truss-pad had been applied, the skin being tied down to the tendinous structures beneath; and the latter appeared firmer and more resisting on pressure than on the opposite side. In the treatment of hernia in infants, there are two circumstances favouring radical cure which do not obtain in the adult, viz.: 1. If the bowel be never allowed to descend, the opening does not increase, but the bowel does; 2. The fibrous tissue surrounding the canal and rings become firmer each month.

ETHER AS AN ANÆSTHETIC.

By HENRY TRENTHAM BUTLIN, M.R.C.S.,

Assistant-Surgeon to the West London Hospital; late Registrar and Chloroformist to the Hospital for Sick Children.

AT a time when many deaths from the administration of chloroform are being reported, I am glad to find Mr. Morgan advocating the claims of ether as a safer anæsthetic. Although this has been done occasionally during the last few years, it has never been insisted upon sufficiently, and has not been backed up by those who are in the habit of largely administering anæsthetics. Now, however, I think everyone who has had much experience in the employment of these two reagents must allow that it is quite worth while to give ether a fairer trial than it has yet had in this country.

There appears to be a particular class of cases in which chloroform is especially dangerous; and this class of cases we have at present no means of diagnosing. That it includes a large number of cases of greater or less fatty degeneration of the heart, is evident from the most carefully conducted necropsies. The histories of most cases of death from chloroform would further point to cardiac weakness, analogous in some way to that produced by fatty degeneration.

It is well known that chloroform, given in small doses to a *healthy* animal during a considerable time, at first stimulates the heart's action, but later depresses it; given in large doses, however, it acts almost immediately as a depressant to the heart's action: but even the smallest dose, where the heart is affected with fatty degeneration, appears to act as an immediate depressant, and, after a few faint attempts to continue to beat, the diseased heart ceases to act, and cannot be aroused again.

From experiments on animals, from my own experience, and from the much greater experience of many American surgeons with whom I have spoken on the subject, I believe that these are just the cases in which ether may generally be safely administered. The experiments made by the committee appointed by the Royal Medical and Chirurgical Society in 1864, showed that ether acted much less strongly and directly upon the heart than chloroform: and I have often had the opportunity of noticing, whilst giving ether at the Hospital for Sick Children, that even at the end of a long and severe operation, the pulse has increased in force and frequency rather than diminished.

When we possess some easy method of diagnosing these affections of the heart, we may then employ ether only in those cases in which they are supposed to exist; but, until then, would it not be wiser to employ ether generally, at least for adults? If no other good results be gained, the fear of chloroform which at present prevails will be removed—a matter of no small consequence.

The inconveniences of ether are not nearly so great as is imagined—