perience a sudden pain in the loins and lumbar mass of muscles,—pain so very severe, that they immediately go home in a state of alarm, as if they had received a violent stroke of a stick or a thrust of a sword.

The reason why I am inclined to admit the existence of a similar cause in this way is, because he is a wine merchant's boy, obliged to descend every day into the cellar, where he remains for a long time, and, moreover, because he has had before several rheumatic pains. We in consequence ordered him to be cupped on the left side of the neck. The next morning the improvement in the patient was really astonishing, and a person who had not seen him the day before, would not have believed that he could have presented the symptoms we have described. Although the neck was a little stretched, and slightly deviated to the left, the patient could flex and extend the limb, bend it laterally, or twist it on the neck. It is essential for you to remark, that no attempt had been made at reduction, and yet the numbness of the right arm was gone. We immediately applied warm poultices and other emollients to the neck, and fifteen days after the accident you saw this youth freed from every trace of the affection.

CASE 2.—*Distention and Engorgement of the Intervertebral Ligaments.*

A man entered the Hôtel Dieu for a pain which he experienced in the cervical region. He pretended that he had received, during the three days of July, a stroke of a carabine on the neck; but he also acknowledged, that a few months before, he had injured himself by a fall, after which he felt a severe pain, but he continued to work. On his entering the hospital, the neck presented a convexity behind, and a concavity in front, which changed its form considerably. The superior part of the cervical column was so prominent, that we could readily distinguish the spinous processes. He could not execute any rotation of the head, and when he attempted to look behind him, you would say the head was soldered on the trunk. Pain was felt, especially on attempting any sudden motion. This disease was aggravated by the patient's occupation. The uniting ligaments of the vertebræ became engorged. A single extraction of blood by cupping-glasses was attended with much benefit: they were applied a second time to the opposite side without the same good results. A moxa was then placed on the neck, and this gave such relief, that the patient himself demanded the repetition of the remedy.

I have no doubt but that these means will effect a cure; in fact he has now no pain, and can execute easily several movements of the head.

CASE 3.—*Disease of the Vertebræ (?) simulating or threatening Luxation.*

An old man, 60 years of age, presented himself a few days ago at the morning consultation, with an affection of the vertebral column, which at first simulated a luxation in this patient. The cervical column was deformed, being concave in front and convex behind. There was also such a stiffness, that motion was impossible, and we also remarked a curvature of the spine, accompanied with pain on the least motion, and immobility of the articulations. Hence we judged the ligaments of the cervical vertebræ were so far diseased, that a spontaneous luxation was to be apprehended. Leeches and emollient cataplasms were applied, and two moxæ were placed on either side of the gibbosity. These means, simple as they were, were accompanied by a good deal of improvement; the patient could execute some movements of the head to right and left; but it is clear that a long time must elapse before we can expect to accomplish a radical cure.

OBSERVATIONS ON

DR. HOPE'S EXPOSITION

OF THE SOUNDS, IMPULSE AND RYTHM OF THE HEART.

By E. L. BRYAN, Esq., Surgeon,
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ALTHOUGH the above distinguished author's opinions on the motions of the heart, and the causes of its sounds and impulse, are very generally received; and, from their having been adopted by the learned editor of the Dictionary of Practical Medicine, and promulgated in the Cyclopædia, they have acquired great weight and authority; I have, for various reasons, been induced to question their correctness, and submit the following observations to the consideration of the readers of THE LANCET.

On the causes of the sounds, Dr. Hope remarks:—"All the phenomena of the heart's action, both in health and disease, lead me to believe that the sounds are produced by the motions of the contained fluid, and the mechanism of their production I conceive to be, according to the laws of physics, as follows. When

"the ventricles contract, an impulse is given to the particles of fluid in contact with them; and this being propagated by collision from particle to particle generates sound. This irregularity of the interior surface is calculated to favour this formation of sound; for, on the very first contractile movement, the stratum of fluid next to the surface, and involved in the sinuosities of the columnæ car-næ, is thrown into an infinity of conflicting currents; whence the collision of particles is more extensive and violent than if it were occasioned merely by a simple direct impulse. It will be understood that these conflicting currents do not neutralize each other by reciprocal reaction, because, as the blood finds egress by the arterial orifices, the reaction is not equal. If the contraction be gradual, the sound will, for obvious reasons, be subdued and prolonged; and if it be sudden, the sound will be proportionably short, loud, and clear. While this is going on at the surface of the fluid, the central mass is tending toward the aortic and pulmonary apertures, and as its current is compounded of a multitude of conflicting streams, reflected on every side from the ventricular walls, and converging towards the orifices, the vibratory collision thus occasioned amongst the particles of blood occasions sound."

If the first sound of the heart be thus produced, it should last with the duration of the conflicting currents and streams, i. e. during the time occupied by the ventricles in contracting, or, according to Dr. Hope, half the time of one beat, which is not the case.

If, on the very first contractile movement of the ventricles, the blood be thrown into a number of streams, must not these streams be first directed toward the mitral and tricuspid valves, and close them before they can make their way into the arteries? And, according to the common laws of physics, would not those streams close the valves with sufficient force and quickness to make them vibrate and produce sound? In accordance however with the physics of Dr. Hope, the primary directions of the streams must be suddenly changed by the closed valves, which must reflect, resolve into new streams, concuss, split, and multiply them, *ad infinitum*. Should Dr. Hope have left unnoticed the closure of the valves, when entering into a minute detail of the currents and streams which, in his opinion, produce the first sounds?

The assumption that the collision of conflicting currents in the blood would occasion sound, has, thus far, been granted.

Dr. Hope is of opinion that it is in accordance with the laws of physics, but he has left his reader totally in the dark as to his authority on the subject of acoustics, mentions none of the laws referred to, and adduces no parallel phenomena to support his views. I shall not attempt a regular refutation of this opinion; but content myself with offering an explanation of the first sound, founded on a simple law of physics, examples of which are so frequently before us as to have rendered it familiar to every one.

Any flexible solid, suddenly brought from a state of relaxation to a state of tension, vibrates, and its vibrations are sonorous or not, i. e. audible or not, according to its physical structure.

At the commencement of the systole of the ventricles, their auricular valves are flapped into play, and at the instant of their closure, the whole substance of the ventricles, and the valves, are suddenly brought to a state of tension, and then, consequently, vibrate. I leave it to the reader to determine, according to the laws of physics, whether the vibrations of the valves, floating freely in fluid, or of the muscular substance of the ventricles, trammelled by the contact of surrounding solids, would most contribute to the formation of the first sound.

The blood, it is true, in common with everything else in contact with a vibrating solid, would take on the vibrations originating in that solid, or, in other words, would conduct the sound; but there is a vast difference between *conducting* and *forming* sound. Anything would conduct the sound of the fiddle-string of a violin; but the string only would produce it. Would it be said of the string, that it caused currents and conflicting streams in the air, and that their collision generated sound? The abnormal sounds of the heart, also, Dr. Hope attributes to the motions of the blood. It is true they arise from the motions of the blood, but not, I conceive, from the collision of its streams, as Dr. Hope supposes, but simply in accordance with the well-known law, that no fluid can pass over the surface of a solid, with a certain velocity, without producing sonorous vibrations in that solid; and as the nature of the sound is determined by the physical structure of the solid, the sound is attributed to the vibrations of the solid and not to the collision of the streams of the fluid; but not only does the sound depend upon the nature of the surface but upon its attachments also; *cæteris paribus*, if the surface be fixed, the vibrations are fine and the sound smooth; if loose, the vibrations are

more extensive, and the sounds louder and less smooth.

On the second sound Dr. Hope writes:—

“When the diastole takes place, the “blood shoots with instantaneous velocity “from the auricles into the ventricles, “and the reaction of the ventricular walls “on its particles, when their course is abruptly arrested by the completion of the “diastole, is, I conceive, the cause of the “loud, clear, and brief sound.”

If this were the case, the auricles, by their contraction, which occurs after the second sound, could not throw any additional portion of blood into the ventricles, when they had just been distended to such an extent as to cause their strained tissue to react on their contents with a force sufficiently strong and instantaneous to produce the second sound. And if it be the sudden concussion of the blood by the surface of a solid suddenly made tense, that produces the second sound. Why not attribute the sound to the semilunar valves, on the arterial aspect of which, this phenomenon of concussion takes place with much greater intensity? Dr. Hope assumes, without proof, that certain phenomena do take place, and produce sound; while, to exactly similar phenomena, which it is well known must take place, he attributes no sound whatever.

All that Dr. Hope advances against the opinion that the second sound is produced by the semilunar valves, I here give in his own words:—

“That it was not ascribable to the retrocession of the semilunar valves, I entertained a strong presumption, from “having found the sound unimpaired, “though the valves on one side of the “heart at least were rigid with ossification; and the presumption amounted almost to certainty, from my having “found the sound not only undiminished, “but increased, in cases of enormous dilatation of both ventricles, in which it was “impossible the cavities could ever empty themselves; and where, consequently, “the motion of the valves must have been “impeded by the constant pressure of “fluid on both sides.”

With regard to the first of these objections, the ossification of one valve could not impair the action of the other, and, whilst lessening slightly the intensity of the sound, would add to its clearness. On the second objection I have only to observe, that the semilunar valves would act most freely with fluid on both sides, and were the pressure such as Dr. Hope assumes it to be, it would entirely overthrow his own opinion on the subject: for if, at the instant of cessation from contraction,

a dilated ventricle helps up considerable and constant pressure upon its contents, how can the blood flow into them, from the auricles, with sufficient freedom and velocity to concuss the walls of the ventricles in such a manner as to produce a clear, loud, second sound? If the pressure of a dilated ventricle be sufficient to trammel the action of the arterial valves, which are acted upon by the maximum force of the circulation, i. e. that of the arteries, how can the same cavity be dilated by a minimum force, that of the auricles or veins? Of the mitral and tricuspid valves, Dr. Hope observes, that they were sometimes, one or both, so diseased, as to have been incapable of acting, while the first sound remained audible. Were only one valve, or even a portion of a valve, in such a state that it might be flapped into tension, it would produce an audible sound. This objection is not valid, unless it be found that in cases where both valves have been so extensively diseased as to have prevented their action, the first sound has existed. Of such a state of the valves, the thirty-one cases of disease of the heart detailed by Dr. Hope afford but one example. The stethoscopic signs and state of the heart were as follows:—

“*Case 18.*—Sounds.—*The first was a very loud fling murmur, or that of obscured “and subdued sawing; it commenced “abruptly with a flap. The second sound, “short and flat, was so weak as scarcely “to be audible. It concluded the first “murmur. The same sounds existed on “both sides of the heart, but were more “subdued and indistinct on the left.*

“*Autopsy.*—The heart was nearly twice “its natural size. Right auricle and ventricle much dilated; the latter larger “than an orange. Parietes of both of “natural thickness, but the ventricular columnæ carneæ enlarged. Muscular “substance firm but pale.

“*Left Ventricle.*—Its cavity enlarged to “the size of a goose’s egg. Walls of a “natural thickness, but pale, flaccid, and “easily lacerable. Tricuspid valve an uneven, thick cartilaginous ring, which admitted the middle-finger. Mitral valve “was a similar ring admitting the end of “the little-finger. Pulmonic and aortic “valves natural.”

Dr. Hope remarks on this case, that “the fling murmur proceeded from regurgitation into the auricles.” This observation is quite correct, I conceive; but why was the true first sound of the heart absent, when it ought, according to Dr. Hope, to have been loud, smart, and clear? Simply, I believe, because the mitral and tricuspid valves were not in a state to produce it. With regard to the

second sound; if it were produced, as Dr. Hope affirms, by the diastole of the ventricles; how could it terminate, *i. e.* follow, without any interval, a filing murmur produced by regurgitation into the auricles? In such a case the blood must change its course, return through contracted orifices into the ventricles, and dilate them, so as to leave no interval between the sound of regurgitation, and the sound occurring at the completion of the diastole; consequently, in the above case, the diastole of the ventricles was an act without duration, or the opinions of Dr. Hope on the second sound are incorrect.

The Impulse.—Dr. Hope assumes that the aorta and pulmonary artery are the fixed points to which the fibres of the ventricles tend, during their systole; and conceives that the tense and rounded body of the ventricles is for this reason drawn upon the auricles behind; that, consequently, the apex is tilted forward; and that, in proportion as the ventricles contract to their extreme, the apex is thrown more and more forward by the auricular distention advancing in the same progression to its extreme.

This explanation of the impulse implies imperfection in the mechanism of the heart; for if, when the ventricles contract, they make pressure on the auricles, the former must waste a portion of their power in impeding the action of the latter. But is it at all certain that the arterial orifices are the fixed points to which the fibres of the ventricles tend? If these parts be examined, it will be observed that they are constructed so as to allow of motion. The impulse is not always produced by the apex. When, from effusion into the left cavity of the thorax, the heart is displaced, the impulse is felt to the right of the sternum, far from the apex, while the true impulse is absent. Nor would this movement, in excess, answer to the impulse of hypertrophy, or account for the impulse at the epigastrium, where it is frequently observed.

On the duration of the different actions of the heart, Dr. Hope writes:—"This is the same as indicated by Laennec. The ventricular systole occupies half the time, or thereabout, of a whole beat.

"The ventricular diastole occupies one-fourth, or at most one-third.

"The interval of repose occupies one-fourth, or rather less."

It must here be remembered, that, according to Dr. Hope, the systole of the ventricles commences with the first sound, and their diastole terminates at the second sound; therefore the first sound and first interval include the time in which the two actions of the ventricles

take place; consequently, if Dr. Hope be correct, the first sound and first interval should occupy three-fourths of the time of one beat, and the second sound and interval the remaining fourth. Now it is well known that the interval between the first and second sound is much less than that between the second and the first of the following beat; therefore, the first sound and first interval occupy much less than half the time of one beat. Dr. Hope must therefore be in error as to the time at which the systole and diastole take place, or extremely inaccurate with regard to their duration.

Stowmarket, Aug. 26th, 1833.

EFFECT OF

IODINE IN HYDROCEPHALUS.

To the Editor of THE LANCET.

SIR,—Having some time ago read, in *THE LANCET*, a case of hydrocephalus, which was reduced by complicated treatment, I herewith send you one speedily subdued by iodine.

The case is that of Thomas Dixie, aged eight months, residing at No. 4, Rumbals Buildings, Sun Street, Finsbury Square, who was admitted at the City Dispensary, on the 15th of January last, with the following symptoms:—Physiognomy idiotic, very irritable, the frontal bone much projected, the skin and subcutaneous veins of the head remarkably distended, the pupils greatly dilated, occasional constipation, dysuria, habitual vomiting, convulsions, &c.

The mother stated that she had already lost two children by water on the brain, the one at three months, and the other at five months of age, and the child now presented had been similarly unwell from its birth. As it appeared to suffer much, I proposed that the water should be evacuated, as the speediest means of affording relief, but to this the parent decidedly objected.

Having previously employed iodine in some cases of morbid increase of the aqueous humours with good effect, especially in a case of enlarged eye, I was induced to try iodine in the present instance. In order to ascertain the results, the head was first carefully measured by Messrs. Honeywood, Tyser, &c., and its circumference found to be eighteen inches and a half, and from meatus to meatus, over the summit, twelve inches and a half.

Treatment.—℞ Iodin. ʒi; Cerat. Cetaei ʒi. M. This ointment to be rubbed in upon the open part of the head three