

Original Communications.

NOTES AND OBSERVATIONS ON DISEASES OF THE HEART AND LUNGS.

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[Continued from p. 381.]

In the preceding paper, the contraction and expansion of the ventricles of the heart have been spoken of as the main agents of the circulation of the blood. It is now proposed to consider the rationale of the sounds which result from this vital process thus produced.

It is generally admitted, in accordance with the statement of Dr. C. J. B. Williams, that the series of phenomena which are associated with each complete movement of the heart (as the impulse, sounds, pause, etc.) occupy a cycle of time, divisible into five periods; two of these being occupied by the impulse, together with the first or systolic sound, which is dull and prolonged, the "rub"; one by the diastolic or second sound, sharp, short and abrupt, the "dub"; and the remaining two by the pause. Some modern observers, however, speak of a third sound, the presystolic (Vesal, Spring) occupying the latter half of the pause; but looking to practical results, any consideration of this latter need not detain us.

Though the above division of the heart's action into five periods be adopted, as most consonant with general observation, yet, doubtless, very many hearts that one examines will warrant a division of the cycle of events into four periods only, in which case the pause is diminished by one half: the first sound occupying two periods, the second sound one period, and the pause one period.

The first and second sounds are now generally, if not universally, admitted to be synchronous with the closure of the valves; the first or systolic sound with the closure of those at the auriculo-ventricular openings, the second or diastolic with the closure of the semilunar valves.

The conclusion as to the immediate cause of these sounds, is not so unanimous. Some writers attribute them solely and entirely to the sound emitted by the contraction of muscular fibre, and therefore due to the act of contraction in the heart itself; some to the blow of the heart on the parietes of the chest; some to the vibration of the valves;* and some to the effect of the heart's contraction on the fluid it circulates; and some regard these sounds as due to a combination of many of these causes. My own conviction is, that the sounds referred to are mainly, if not entirely, due to the natural interference with the even flow of the blood by the various mechanisms of the heart, and that they are to be referred to the ordinary laws of Hydraulics, and little, if at all, to be attributed to the contracting act of the muscle of the heart, or to any vibratory motions in the substance of the valves.

This branch of our inquiry presents itself under difficulties; and I only approach it with diffidence, for many of the English school, as Billing, Halford, Fuller, etc., and nearly all the writers belonging to

* Spring (*Annales de la Société Médico-Chirurg. de Bruges*, 1861), who refers the sounds to the contraction of the heart and valves, attributes the presystolic sound to the lowering of the auriculo-ventricular valves.

the foreign schools, refer the sounds to the act either of muscular contraction or of valvular vibration.

Doubtless certain sounds may, under certain circumstances, be emitted during the contractions of muscular fibre; but these sounds are neither identical with nor even analogous to the audibly distinct phenomena presented during the heart's action; moreover the intensity of the sounds emitted by the heart is not, *pari passu*, in accordance with the existing development of the muscular structure of the heart; on the contrary, disease in this organ, whether of hypertrophy or of attenuation, rather shows the opposite fact to be the rule.

The valves of the heart, whether considered in relation to muscular contraction or to vibration, certainly do not present *per se* those physical conditions adequate to afford a satisfactory explanation of the production of the sounds.

These valves are not, on the one hand, muscular in their structure, nor even closed through the immediate instrumentality of muscular contraction; but solely under the influence of the direct mechanical operation of hydraulic pressure; nor, on the other hand, is their size, under slow vibration adequate to the production of so loud a sound; nor supposing there were such adequate rapidity of vibration, is the condition of their floating in so dense a fluid, as is the blood, one thus likely to generate, or if generated, to transmit sound.

Again, when, on the closure of the several valves, the passage of the blood through their aid is not completely effected, though the valves may, and the ventricles must contract, the former thus enjoying full scope for their vibratile qualities if they ever possessed them, and the latter for those muscular contractions which undoubtedly belong to them, the peculiar sounds said to be referable to their contractions and vibrations are no longer to be heard. If true, sounds are heard; but these are of a quality which cannot be referred to muscular contractions or to membranous vibrations, though they can be equally with the normal sounds, accounted for by hydraulic laws. In fact, as is so well known, a very slight disturbance of function, or alteration in structure in these valves, so as to effect a condition either of slight obstruction, or of imperfect closure, may be sufficient to banish entirely the normal sounds, and convert them into others, very different in their character; moreover, both the normal sounds in question are emitted at the precise moment when the valves have become closed, and not at the time during which the process takes place, whereby their closure is effected; while any connexion of the second sound with the muscular contraction of the ventricles, is at once set aside by the fact of its not being synchronous with it.

Can these sounds be attributed to the heart's impulse, to the blow of the projected apex against the anterior walls of the thorax? As just stated, this certainly can not be the cause of the second and louder sound, for this latter takes place at a period of time, sensibly later than that which is occupied by the impulse or blow against the thoracic parietes. The dependence, therefore, of the second sound on this cause, may be at once set aside; and it will then only remain to consider what may be the connexion of the first sound with it. That it is not entirely dependent upon it, is obvious from the often repeated experiment of so exposing the heart that it has no chest parietes to strike against. Dr. Markham, in his practical and most excellent sketch of the diseases of the heart, addresses himself to this subject (*vide* Appendix III, *On the Sounds of the Heart*); and advances, as an argument for assuming this sound to be partially influenced by this cause, the com-

monly observed evidence of the beat of the heart of a nervous, hysterical female. He states this to be conclusively demonstrative of the fact, that a heart can "rap like a hammer against the inside of the thorax." Granting it to be the fact that under certain circumstances the heart's impulse against the parietes of the chest is increased both in force and rapidity, no argument can be deduced therefrom, for concluding the origin of the first or systolic sound to be due to this act, while there still remains the undoubted evidence of the presence of the first sound, though no thoracic wall may exist for the heart to impinge against; and while, as may be too frequently observed, this sound, provided only the valves be imperfect, may be lost and changed into another of a totally different character, notwithstanding the impulse, the *ictus ventriculi*, be more violent and distinct to the feel than natural.

As it is not the purpose of these papers to discuss largely physiological data, but rather to state the practical conclusions arrived at, after many years of observation, I shall not pursue the discussion further, but at once venture here to suggest that, as it appears to me, on the one hand, the various theories commonly adduced in explanation of the normal sounds of the heart, and of their disturbance in disease, are not adequate to explain them; so, on the other hand, their production finds every explanation in certain of the laws that govern the flow of fluids through tubes; and that, therefore, to the recognised laws of Hydraulics we must look for their true, safe, and consistent interpretation.

Having premised this, I will at once proceed briefly to consider the source of these sounds in relation thereto.

One of the familiar and well acknowledged laws of hydraulics is that, the momentum of a liquid being as great as that of a solid, anything which opposes this momentum will receive as severe a blow as if from a solid. Dr. Arnott, in stating this law (*Elements of Physics*, second edition, vol. i, p. 458), gives an example of its application, so analogous to, and happily illustrative of, the whole question under discussion, that I venture to transcribe it here.

"It has long been observed, in household experience and elsewhere, that, while water is running through a pipe, if a cock at the extremity be suddenly shut, a shock and noise are produced there. The reason is, that the forward motion of the whole water contained in the pipe, having been instantly arrested, and the momentum of a liquid being as great as of a solid, the water strikes the cock with the same force as a bar of metal, or a rod of wood, having the same weight, and moving with the same velocity. A leaden pipe, if of great length, is often widened or burst in this experiment. Lately, this forward pressure of an arrested stream has been used as a force for raising water; and the simple arrangements of parts contrived to render it available, has been called, on account of the shock, the water-ram."

The circumstances attending the circulation of the blood through the heart, will, on consideration, be found to present every condition necessary for the application and illustration of this law; there is the current of blood passing through tubes, and this current suddenly and forcibly arrested by the closure of the valves.

It has been stated by Dr. Fuller (*Diseases of the Heart and Great Vessels*, p. 33) that the impulsion of the blood against the semilunar valves is not adequate to produce the second sound, because the vessel behind is full; but his reasoning here is not consistent with the above stated law of hydraulics, nor is it borne out by fact. In order to test this, it

is only necessary, by way of experiment, to check, by the sudden closure of a stop-cock, the stream of water passing through a filled pipe; the vessel behind is here quite full, and yet the blow takes place. The evidence of this is made sufficiently manifest in the noise thereby produced, and in the obvious vibrations induced in the surrounding materials.

Another law of hydraulics is equally applicable, and found to be complete in those cases where interference arises to the normal even flow of the blood, in consequence of the existence of certain departures from a perfect condition of the organs immediately concerned in its circulation.

Hydraulics teach us that, if the even flow of fluids in tubes be interfered with, audible indications of this interference are the immediate result. The law here is, that sounds are produced by eddies and by obstructions of flow, and are dependent for variety in the proportion of fluid to force and velocity. Hence, when the valves of the heart are diseased, the normal sounds are found to be modified, or even in some cases to be so entirely superseded by other and very different sounds, as to lead to the inference that they scarcely perform the functions of valves. We have in all these instances, then, presented to us certain qualities of sound, variously designated as murmurs, bellows-sounds, *bruit de soufflet*, etc., which are also presented to us on the passing of fluid through tubes, under certain circumstances, whereby the flow is interfered with, though these tubes be not furnished with valves. Aneurisms, arteries abnormally pressed upon, etc., offer themselves to the medical observer as frequent illustrations of this law. Dr. Watson (*Lectures on the Practice of Physic*, vol. ii, p. 235) says the whole matter may be briefly thus expressed: "The blowing sound may be occasioned by any change which alters the due proportion between the chambers of the heart and their orifices of communication with each other, and with the blood-vessels that respectively enter or leave them; it may also be occasioned by a preternatural velocity in the passage of the blood through a healthy and well adjusted heart. Dr. Elliotson, I think it is, who has offered this apposite illustration of the phenomenon. If the arches of a bridge have a certain relation to the quantity of water in the river, and to the force of the current, the water passes through them quietly and without any noise. Diminish the size of the arches, and the water begins to go through them with an audible rushing or roaring sound. The very same thing will happen if the arches remain unchanged in size, but the quantity of water in the river, and therefore its velocity and force, be augmented by heavy rains. So it is in the heart. If one of its orifices—say the aortic orifice—be narrowed, by disease of the valves, or in any other way, the blood will not, as before, glide through it smoothly and without noise, but will yield that sound which we call a bellows-sound. So also, if the orifice retain its natural dimensions, but the capacity of the cavity from which the blood is driven be augmented. Nay the same blowing-sound may be produced though the cavities and orifices are all healthy, and duly proportioned to each other, if the velocity of the circulating blood be increased beyond a certain measure."

The above passage has been quoted as well and forcibly exemplifying the law of murmurs, so frequently to be met with in diseases of the heart, and when it necessarily becomes the office of the physician to pronounce upon the condition of the heart in relation thereto. Given the flow of blood and the existence of a murmur as evidence of its normal relations being interfered with, there is then required to be ascertained the precise cause whereby this in-

terference is effected; whether it be through the means of obstruction in, or by inadequacy of, the conveying-tube; whether there be an increased or a diminished calibre in the chief propelling vessel or disproportion in the quantity or quality of the transmitted fluid, etc.

Having now set forth these two laws of hydraulics, and assuming their applicability to explain, as the case may be, the normal sounds and the murmurs occurring to the circulation of the blood during its passage into, through, and out of the heart, it will be useful to examine whether as theories they prove, experimentally, consistent with the various phenomena that occur.

[To be continued.]

REMARKS ON SYPHILISATION.

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CERTAIN lectures having appeared in the *Lancet* from the hand of the well known surgeon and syphilographer, Henry Lee—lectures which in an equal degree seem intended to extol the efficacy of the calomel bath in the cure of syphilis, and to depreciate the claims of syphilisation to the favour and study of the profession (for it is difficult to say which of these two is the principal aim of the writer)—it has appeared unbecoming in us, as friends of free inquiry, to remain silent under such a challenge; since, as intimates sharing in the same observations, and students in the same field, we might be accused of acquiescence in these views and complicity in these results, did we permit so narrow an interpretation to be placed upon what we have seen done during the last few months towards elucidating, through this process of syphilisation, many difficult and curious points in the natural history and treatment of the venereal disease. And, as Mr. Lee has given currency to his ideas in a periodical so much in favour as the above, no less with the medical profession than with the general public, so we esteem ourselves fortunate in being permitted to address the profession through the pages of the BRITISH MEDICAL JOURNAL, which was the first in this country to direct attention to the importance of these questions, in their double relation to medicine and pathology. It would, indeed, seem to us that, in penning these lectures, Mr. Lee must have had it in his mind to exert a reply, in order that he might have before him at once those arguments which he will be compelled to meet whenever and wherever he is brought face to face with the subject: or did he hope to strike away from indecision its last remaining prop, and to influence the public mind against views so menacing as those with which we are concerned, to conservatism in this branch of practice? It is undoubtedly more easy, and more in the fashion of the day, to learn from the observation of others, than to spend time in observing for ourselves; and we think that Mr. Lee has committed a mistake, when he believes that, by relying on his great experience and independent resources of experiment, he can dispense for this once with the observation of facts occurring in a constant series. Such experience as we have gathered is fairly at the service of all; we have only to regret its immaturity, from the small opportunities which have hitherto been afforded in this country for studying the subject. It may be that we are not without self-reproach, as having brought too little earnestness into this field

—a fault which is too common in our days not to be admitted as a plea. The blindness which we have indulged, as to the amount of resistance which these novel and ingenious views were likely to encounter at the hands of our countrymen, has left us more unfurnished than we otherwise should have been, had we anticipated (which was far from our thoughts) such an entire difference of opinion before the evidence of facts, and so active an opposition to the spirit of improvement on the part of our talented friend.

It seems to us, however, very strange that a purely scientific question (for such is the aspect which syphilisation wears in this country) should be brought forward side by side with a particular empirical method, which of itself has as little to do with syphilisation as the Goodwin Sands with Tenderden church-steeple. Whether syphilisation can ever vie with mercury as a remedy for syphilis, and especially as a secret remedy, is entirely subsidiary to the scientific questions involved; and these ought not to suffer prejudice because Mr. Lee has discovered a convenient method of administering mercury for the cure of the disease. It is true that, in common with others of his countrymen, the chief promoter of syphilisation is a decrier of the mercurial treatment—an opinion which has been arrived at among a scattered population in a country where the disease is endemic; and, moreover, in regard of doctrine, Dr. Boeck has ranged himself under the flag of the unicists, though perhaps between him and the dualists the difference may not be so wide as conjectured; and Mr. Lee, who is, as it seems to us, a dualist in all but the name, from which he shrinks, may stand opposed to Boeck on a double plea of dualism and cure by mercury. There are many among us, however, who, as neither unicists nor dualists, are fain to say, "A plague on both your houses"; or, perchance, viewing dualism with a favourable eye, as being of the latest advance, and recognising in mercury a potent remedy against syphilis, they may still feel interest and curiosity of the highest kind in regard to syphilisation. It cannot escape their apprehension, that mercury is a remedy of old date. We know its power, and we are not without some knowledge of its defects. Syphilisation, on the other hand, is of recent introduction; it is even yet under trial; and as yet, amongst us and elsewhere, it has been chiefly employed in the sense of pure experiment—i. e., without adjuvants and palliatives of any kind. There is plausibility in the argument that, as a means of cure, it may be susceptible of vast improvement. If we view our most received methods by the light of their history, we shall find that they were perfected by the slow hand of time. It is not wise to judge of a thing in the first cast and in the rough. Even as regards the principle of any thing, if new, we should refrain from deciding suddenly or with too violent an emphasis. No better proof of this could be given than in the instance of the mercurial vapour-bath—a remedy which has been tried and discarded repeatedly, after enjoying for a season a certain amount of favour. Many times it has been abandoned, and resumed again. But if, as Mr. Lee would have us to believe, it is constantly successful, faultless, universally applicable, and ever unattended with regret, then we cannot but admire the perseverance of those who have brought it to perfection, and the lenient spirit of society which has been favourable to so many trials of its virtues.

With the detractors from the merits of mercury, however, and the devotees of rationalistic medicine, it will ever remain a subject of accusation and a stigma of reproach, if syphilisation be not allowed a fair chance. Some margin of deference must also be