both sides of the heart, and both sounds were instantly reproduced. The veins were again compressed, and all sound extinguished, notwithstanding that the heart acted vigorously. Blood was again let in, and both sounds restored. All that is claimed for the above experiment, is its exemption from any rude interference with the mechanism of the heart's action. The cavities of the heart are untouched; there is no finger thrust into the auricle or ventricle; no hooking back of valves: in fact, not one source of sound substituted for another. Both sounds are destroyed and reproduced by the same means; the strongest argument for their both depending on the same cause, which is simply the backened current of blood, first against the auriculo-ventricular, and second against the ventriculo-arterial valves."

Now what is the value of this experiment? It proves unquestionably that there is no such thing as *bruit musculaire* in connection with the systole of the ventricles, and it disproves all the theories founded on a contrary supposition; it proves, moreover, that whenever the blood is allowed to play upon the valves, the sounds are produced.

Before Dr. Halford performed his experiments, in the original of which I assisted him, a very ingenious experiment had been performed by Mr. Brakyn, which consisted in propelling, by means of bladders and tubes connected with the left side of the heart, air through the cavities, so as to represent the flow of the blood. His experiment proves that when the auriculo-ventricular and the ventriculo-arterial valves are thrown into a state of tension, by air acting on them in the same way as the blood, sound is produced. This experiment I have frequently repeated—the original apparatus used by Mr. Brakyn being still in my possession—and the sounds resemble in every respect, considering the nature of the fluid in which they are produced, those of the living heart.

These two experiments seem to me to settle entirely the question which has so long agitated the minds of physiologisis. Mr. Brakyn's proves that the tension of the valves is equal to the production of the sounds, and Dr. Halford's that muscular contraction has nothing to do with them.

The pressure of the semilunar valves against the sides of the great vessels is said to assist in the production of the first sound. This requires but slight consideration. Against what are they pressed? Against the *yielding* walls of the vessels, and can there be any element of sound in such an occurrence? I think not.

I think I have now proved that all the phenomena synchronous with the first sound, except the closure of the valves, are unequal to its production, or even to assist in it; and also, that such closure is equal to produce the effect. It is then to *the tension of the mitral and tricuspid valves, produced by the blood being forcibly propelled against them*, that this sound is due.

With regard to the second sound, there is but little difference of opinion; the experiments of Hope proved that it was solely due to the semilunar valves. When the arteries recoil after being distended by the ventricular systole, the blood in them is forced back towards the ventricles, the semilunar valves then come into play, they are stretched across the vessels, and suddenly made tense, and then sound is elicited.

If you listen to the sounds carefully, you will find that they do not differ in *kind*, but only in *degree*. The auriculo-ventricular valves are large, thick, and strong; consequently their vibrations are slow, and the sound they produce prolonged. On the other hand, the ventriculo-arterial valves are small, thin, and comparatively weak, and their vibrations are rapid, and the sound they produce short. Both sounds may be illustrated by making tense two pieces of membrane of different size and thickness.

If the theories I have mentioned require any further confirmation, they receive it from the sounds which result when there is disease of the heart. If you hear a murmur, you know there is something wrong with the valves, you do not think of the muscular walls; you know that there is some deposit in connexion with the valves, which either from its roughness causes a sound as the blood passes over it, or else prevents the valves properly closing, and thus allows of regurgitation.

Moreover, consider the alteration in the sounds, which is the result of a change in the muscular walls. When the ventricles are hypertrophied, the first sound is less distinct, and of a muffled character; it has to pass through the thickened muscle, and necessarily comes less sharply to the ear. Again, when there is dilatation of the ventricles and thinning of their walls, the sound is clear and sharp; it has to pass through a smaller space and is less altered in its character. If the sound were due to muscular contraction, surely, in obedience to the law of physics, that if you increase the cause you increase also the effect, the sound of a hypertrophied heart would be louder and more distinct than that of one in which the muscular fibres are diminished; but the contrary obtains, and this fact affords an additional proof of the valvular theory.

REMARKS ON DR. HALFORD'S EXPERIMENTS CONCERNING THE SOUNDS OF THE HEART.

By W. O. Markham, M.D., Assistant Physician to St. Mary's Hospital, London.

Dr. J. B. Halford has lately performed a series of experiments at different medical schools in London, for the purpose of demonstrating, amongst other things, that the opinions long ago laid down by Dr. Billing, respecting the nature and causes of the sounds of the heart, are correct; viz., that both the sounds are entirely and alone produced by the valves of the heart.

It would appear from remarks, which have been made in some of the medical periodicals, upon those experiments, that they have been by many persons received as positively demonstrative of the opinions above mentioned. As I happened to be a witness of the experiments which were made at St. Mary's Hospital, and as I could not convince myself of the correctness of the above conclusion from what I then and there observed, and as I think them quite untenable on other grounds, perhaps I may be permitted to state, in a few words, my reasons for being so unfortunate as to differ from many others, who have assisted at these vivisections.

The position assumed by Dr. Halford is this:—When no fluid—liquid or gaseous—passes through the cavities of the heart, the valves are not called into action, and no sounds are produced. Hence, the sounds of the heart depend upon the action of the valves. In order to demonstrate this position, Dr. Halford rapidly opens the thorax of a dog under the influence of chloroform, and by the aid of a bellows inserted into the trachea keeps the animal alive by sustaining the respiratory functions. He then skilfully cuts off all the sources through which blood, venous or arterial, can find its way into the heart's cavities. When this has been effectually accomplished, Dr. Halford finds that the sounds of the heart are no longer audible.

In the two cases in which I saw Dr. Halford operate, I could not admit the correctness of his views; and for the reason, that I still heard two sounds associated with the movements of the heart, although he assured me that all sounds were at the moment inaudible.

Certainly, the sounds I heard differed vastly from the healthy and natural sounds of the heart; they were weak, dull, and muffled, resembling rather the obscure flutterings of a heart rapidly and feebly beating in the last agony; but, nevertheless, of such a character, there they were to be heard, at least by the evidence of my sense.

I am well convinced, from the very nature of the experiment, and from the difficulty of rightly manipulating with the stethoscope under such circumstances, that the sounds might be readily overlooked; and I can quite understand that many persons might not be lucky enough to catch them. However this may be, it is clear that the negative evidence of numbers cannot destroy the positive evidence of one's own senses; and therefore I cannot admit, on this ground, that Dr. Halford's experiments prove the correctness of the position which he takes up.

Admitting, however, the incorrectness of my own observation, and assuming the correctness of Dr. Halford's—viz., that in the cases before us no heart's sounds were audible—I really think some objection may be very fairly taken to the sweeping views thence deduced by him. Is it right, one might ask, to assume that in an animal thus mutilated, and subjected to sudden and violent shocks of the nervous, arterial, and respiratory systems, *all* the elements which may possibly conduce to the formation of the heart's sounds can be left in undisturbed action? Surely, all the other possible causes which may form, or assist in forming, the heart's sounds, as they are heard during healthy life and under a natural condition of existence, must be eliminated from the calculation, before Dr. Halford can assume, as he does, that to the motion of the valves, and that to it alone must be ascribed the cause of the sounds in question. What becomes of the impulse of the heart against the thoracic walls? How is the muscular bruit to be got rid of? How is the rush of blood through the heart's orifices and

over the roughened surfaces of its cavities to be explained away upon such an assumption as this? Dr. Halford may ignore the muscular bruit, and declare the impulse of the heart's apex against the thoracic walls a creature of the imagination; but I am sure that he will not persuade many stethoscopic observers to agree with him herein.

Dr. Halford's, as well as Dr. Billing's views, are surely in this matter too exclusive. No one can doubt that the valves have a most important share in the formation of the heart's sounds; but certainly Dr. Halford's experiments do not prove that no other causes may contribute to their formation; most assuredly, they do not disprove the possibility of the existence of a sound from the impulse, of a sound from the muscular contractions of the heart, and of a sound from the rush of blood through the heart.

It must be admitted, that we have gained a great deal of knowledge concerning the sounds of the heart from observation of their alterations in diseased conditions of the organ; and we may, I think, safely assume from a fair consideration of the same class of facts, that much obscurity still involves the question of the nature and origin of the sounds. The pathologist at the bedside of the patient is continually meeting with facts which seem repugnant to his received theories, and even to what seem to be demonstrated facts. I will here mention one, because it seems to show in a very striking manner the great difficulty which hangs over the subject.

It is generally admitted by observers, that the second sound depends altogether upon the falling together, during the heart's diastole, of the semilunar valves of the aorta and pulmonary artery; this is a fact, which would appear to be placed almost beyond the reach of discussion, being readily demonstrable and decisively so. Notwithstanding, however, the demonstrations of physiological experiments, pathology steps in to throw doubts upon their correctness. How comes it, for instance, that in certain cases of mitral valvular disease, very frequently to be met with, no second sound at all is heard at the apex and about the left side of the heart? A loud *bruit* is heard accompanying the first sound at the apex, but no second sound or *bruit* is audible there. How comes this, when at the same time the second sound is heard loud and clear over the situation of the aortic valves; and loudly intensified, it may be, over the base of the pulmonary artery? Why is the second sound, as usually heard over the apex of the heart, not heard now, if its cause is always to be sought in the semilunar valves? What should prevent the sound being conveyed downwards to the apex during the heart's diastole in this case, as well as during health? I certainly cannot see.

How, again, are we to account, on Dr. Halford's theory, for the reduplicated beat so often heard over the ventricles during their contraction? How also are we to explain the fact, of the second sound being occasionally loud and clear over the ventricles, when at the same time it is very weak and almost inaudible at the base of the heart, if we are in every case to attribute its formation entirely to the closure of the semilunar valves?

Pathological and physiological considerations thus justify us in believing that the causes of the heart's sounds are complex, not simple; that they are produced by no single act, but that several acts are associated in their formation; and that in fact, there is much truth in the assertion made by Skoda, viz., that the ventricles, the aorta, and the pulmonary artery, severally contribute in the production both of the first and of the second sounds of the heart.

CLINICAL OBSERVATIONS ON THE SPECIAL APPLICATION OF LIQUOR PEPSINIÆ IN CERTAIN DISEASES.

By DAVID NELSON, M.D.Edin., formerly Physician to the Queen's Hospital, and Professor of Clinical Medicine, Birmingham.

SINCE my last remarks upon this subject, I am glad to observe, from the discussions called forth, and the attention paid to the preparation of the medicine by chemists, that it is now taking that hold upon the professional mind which its value deserves. In papers previously published, I have adverted to the successful treatment of certain cases of diabetes several years ago by means of this remedy, and also to its general value as a natural solvent of the food within debilitated stomachs. In regard to diabetes, I have now about a dozen patients in whom there has occurred a complete arrest of that usually deadly malady; but I do not intend at present to treat of that particular disease, or to enter upon any theoretical discussion thereon, but to lay before my brethren of the BRITISH MEDICAL ASSOCIATION some evidences of the special value of this agent in chronic complaints, and in those forms of disease which are viewed as malignant, or which are of so obstinately destructive a kind that their ultimate results are quite as bad. I would wish it to be understood at the outset, that, in citing such evidence, I have no intention of making perfect reports of the cases, in their rise, progress, treatment, and termination; but mean simply to mention such salient features of the ailments as may tend to illustrate the beneficial action of pepsine in arresting or retarding their advance.

I would also premise, that the form of the agent employed has been that of the *liquor pepsiniæ præparatus*, or *medicinal rennet*, as made in Birmingham, the ordinary dose of which is one fluid drachm. I shall also endeavour to classify the cases so as to exhibit its effects methodically, beginning with those of a milder character, and proceeding to others of a more formidable or fatal tendency. With this view, I shall illustrate its effects, *seriatim*, upon patients labouring under simple chronic dyspepsia; upon others labouring under dyspepsia associated with affections of the heart, lungs, uterus, or brain; upon some with abdominal tumours; upon some with ulceration of the stomach; upon others with malignant disease of the liver or stomach; upon others of a tuberculous habit, but especially young children with tabes mesenterica; upon one with diabetes, recently treated with success, but not included in any former report; and on another with albuminuria.

A. SIMPLE CHRONIC DYSPEPSIA.

CASE I. Mr. G. G., of Staffordshire, was a farmer, advanced in life, and had suffered for many years from indigestion; otherwise, as regarded all his vital functions, he seemed perfectly sound. His circulation and respiration were good, his complexion was ruddy, and he was strong and active in all his limbs; but he never ate a meal, however simple, without suffering from distension, severe pain, protracted eructations of flatus, and sometimes vomiting. I had formerly prescribed for him vegetable bitters, with alkalies, bismuth, and hydrocyanic acid, with only limited and temporary benefit; but, on taking the three latter medicines with the liquor pepsiniæ, an immediate and continued favourable change was reported. Afterwards, he took the peptic liquor only, with a little alkali; and, as he expressed it, he "ceased to know he had a stomach."

CASE II. Mrs. S. C., of Walsall, corpulent and unwieldy, 45 years of age, had been troubled with indigestion for years past. She never had pain; but her bowels were irregular in their action, and she had constant eructations day and night. She was muscular, but her muscle was imbedded in fat; and the abdomen was immensely large, pendulous, and tympanitic. She had used ordinary remedies without much change, and had never restricted her diet, as she had a good inclination for food, and regularly ate four full meals a day. On using the liquor pepsiniæ with an alkali, and taking two aloes and assafœtida pills at night, instead of supper, her uncomfortable feelings left her, and, with continuing the occasional use of the medicine, and the avoidance of pastry and beer, and the adoption of a meat and bread and wine diet, she has ceased to complain.

CASE III. Mrs. A. C., of Staffordshire, was a lady of tall and firm frame. She consulted me for an obstinate attack of rheumatism, which had specially seized on the tendons of the neck, so as to induce stiffness and great anguish on moving the head or arms. On inquiring into her general health (the state of the stomach included), she said that, eat what she might, for some time past, the food not only lay heavy, but turned sour, and produced at least once a day, and very often in the night, a violent and protracted attack of spasm. She added, that hot water and mustard plasters, with brandy, ether, opium, and chloroform, had all been used, with only transient benefit; and therefore concluded that she must put up with it for life. A soothing and absorbent liniment was prescribed; and she took first fifteen grains of bicarbonate of soda, ten grains of bismuth, fifteen drops of wine of colchicum, one drachm of liquor of pepsine, and twelve drops of solution of muriate of morphia in water, three times a day, after meals. Her rheumatism subsided entirely in from two to three weeks, all the fibrinous swelling having left her neck; and, from the time that she took the first dose of her mixture, she had no spasm of the stomach. She sent her cook, a few weeks ago, to me, requesting the favour of my attending to her for sundry anomalous symptoms, which disappeared after the expulsion of a large round worm eight inches long; and, on my asking how her mistress was, she