is undoubtedly made louder by metallic contact; but if it be admitted that the noise is partly due to the water and partly to the cock, and the analogy be applied to the second sound of the heart, it does not shake my position as to the mechanism of the sound.

By causing a flow of liquid through the heart and aorta removed from the body, I have succeeded in closely imitating the second sound of the organ. But if great precaution be not used in excluding air, so as to prevent it from reaching the aortic valve, even in very small quantity, no sound will be produced. This is because the elasticity of the air breaks the force of contact between the fluid and the valve.* But supposing, with Dr. Williams and others, the sound to be caused by the vibration of membranes in fluid, the presence of air would tend to increase the sound instead of destroying it altogether.

Now, Dr. Williams says that he had in his garden, at Cannes, "a water-pipe exhibiting remarkably this phenomenon" of noise on turning its tap. "The noise", he says, "on stopping the tap was not single, but was followed by two or three diminishing repetitions, which I ascribe to the rebound and successive vibrations of the column of water.† This implies a slight tendency to a vacuum and momentary separation of the surfaces by gas or vapour, which could take place only in a rigid tube, and never in blood-vessels which are everywhere exposed to atmospheric pressure."

I have shown that the separation of the surfaces by gas or vapour, instead of multiplying the sound, would destroy it altogether. If the aorta were a metallic tube, the heart-sounds would undoubtedly be very loud sounds; but I deny that the elasticity of vessels or atmospheric pressure are sufficient to prevent the formation of low sounds by the heart in the bodies of animals.

A simple device, which has a direct bearing on what I have just stated, is sometimes employed by plumbers to prevent the noise made by water turned off by a tap. It is to insert one end of a small tube into the pipe, just above the cock, while the other end of the tube is connected with an air-chamber. The concussion between the water and the cock is thus prevented as effectually as it would be by the intervention of a layer of "gas or vapour" between them.

I have devised an experiment, founded on this practice, which circumstances have not allowed me to put into execution. It is this: Having rendered a large animal insensible, to fix in the aorta, close to the valve, a tube fitted at one end with a properly adapted screw-flange, and at the other with an air-chamber. If the result were (what I expect) that both sounds of the heart were extinguished, my explanation of their cause might be regarded as demonstrated.

Dr. Williams recapitulates the observations made in his vivisection experiments; but most of the conclusions drawn from them are not antagonistic to my theory. One only calls for special remark; namely, that whether the heart was empty or full, whether it was moving feebly or forcibly, some (so-called) muscular sound was always present. My explanation of this circumstance, deduced practically from my own experiments, is as follows. When a stethoscope is applied directly to a contracting heart, it is *absolutely impossible* to distinguish the sound caused by the friction of the heart's surface against the stethoscope from any allied sound. The first sound of the heart may, indeed, be closely imitated by movements of the palm of the hand applied to the bell end of a stethoscope, when the other end is applied to the ear.

Instead of attributing the various sounds of the circulation to widely different causes—such as muscular contraction, valvular vibration, and flowing blood (murmurs)—all the sounds, according to my views, are closely related. Under certain conditions, one sound changes into the other; and, therefore, all are formed essentially by the same mechanism.

It results from this, that pathology gives to my theory an amount of support which no other theory can claim. Many phenomena could be adduced in support of this statement, if my limits allowed. I will, therefore, conclude with a question, because, when put on a former occasion, it remained unanswered.

Why is it that, in anemia, the first sound of the heart only, and the second sound never, becomes replaced by a murmur, unless it be that, according to the views stated, the blood loses in this disease one of the conditions necessary for the formation of the normal sound, namely, sufficient viscosity?

If Dr. Williams, or any one else, will explain this one point, in accordance with any muscular or valvular theory of the first sound, I shall begin to feel doubtful about the conclusions herein set forth.

* See my thesis, page 15, in which I have pointed this out as the reason why Sir D. Corrigan was unsuccessful in imitating the second sound of the heart. I beg here to say that I shall be glad to forward a copy of the thesis, on application, to any one specially interested in the subject of the heart sounds.

† I have elsewhere sought in this phenomenon for an explanation of reduplication of the second sound of the heart (*Lancet*, October 17th, 1868).

A CASE OF CARCINOMATOUS TUMOUR OCCUPYING THE RIGHT POSTERO-PARIETAL LOBULE OF THE CEREBRUM: BLINDNESS FROM DOUBLE OPTIC NEURITIS.

By JAMES RUSSELL, M.D.,

Physician to the General Hospital, Birmingham.

REFERRING to the recent observations by Dr. Ferrier on the Functions of the Cortex of the Cerebrum, it appears (*Proceedings of the Royal Society*, 1875, No. 161) that, after stimulating the postero-parietal lobule or the superior extremity of the ascending parietal convolution in monkeys, he obtained results which led him to regard this particular region as a centre for the movements of the hind leg, and apparently for those concerned in walking. Further, in two experiments, iv and v (*Philosophical Transactions*, 1875), after destruction of the postero-parietal lobule (foot-centre), the ascending parietal convolution (hand and wrist) with a portion of the ascending frontal (arm and leg centre)—the latter convolution to a different extent in the two experiments—in the one case, hemiplegia of the opposite side was produced; in the other, paralysis of the muscles governing the ankle and wrist. In the case I have to narrate, the only distinct symptoms referring to the condition of the motor function, which could be eliminated from the general depression of all the nervous functions which prevailed, were occasional dropping of the arm from brief temporary paralysis, and unquestionably a state of permanent weakness of that member, appreciable by comparison with the opposite limb. Muscular energy was generally in so low a condition that it was difficult to say to what extent the very feeble power of locomotion as regards the lower extremities was to be assigned to each of the several elements—general hebetude, awkwardness from recent loss of vision, and local muscular weakness. Brief attacks of loss of speech must also be noted, together with abnormal sensation in the lips and tongue; these attacks, together with the passing arm-paralysis, were doubtless to be referred to irritation (vascular contraction?) and not to destruction of tissue. Had the disease been of an irritative rather than of a destructive character, a syphiloma rather than a carcinoma, we should probably have to note more characteristic phenomena.

POST MORTEM EXAMINATION.—The sinuses of the upper part of the dura mater were empty, the surface of the brain was pale, the convolutions were much compressed, and the sulci were narrowed to mere lines. There was no ventricular effusion. A tumour, having the microscope characters of carcinoma (Dr. Saundby) of oval shape, two inches in its antero-posterior diameter, was buried in the superficial part of the right cerebral hemisphere, occupying nearly exactly the site of the postero-parietal lobule. It was clear of the ascending parietal convolution in front; and behind, it did not quite touch the external parieto-occipital fissure; below, it extended just short of the intraparietal fissure; and, on the inner face of the hemisphere, reached downwards in the quadrilateral lobe for half an inch. It extended into the cerebral substance for the depth of one inch. On its surface, it presented some traces of the convolutions it had supplanted; below, and especially towards the ventricle, the whole substance of the hemisphere was extensively softened, shreddy, and of a yellow colour. The opposite hemisphere was quite healthy throughout, and so were the pons Varolii and medulla oblongata. The cord was typically healthy till we reached the lower three inches of the dorsal region, where it was very soft, though white and without any vascularity or intermixture with blood-material; nor could Dr. Saundby or myself detect any foreign matter in its composition. As this part of the nervous centres was not examined till four days after death, we should unhesitatingly have referred the softening to *post mortem* changes, but for the contrast afforded by the other portions of the cord. The lumbar enlargement was quite healthy. All the other organs of the body were quite healthy. The cerebral tumour was primary.

The patient was forty-nine years of age, and was kindly sent to me, as an in-patient of the Birmingham General Hospital, by Mr. Eales, Surgeon to the Eye Hospital, on September 28th. He was then almost completely blind, and exhibited the typical characters of double optic neuritis, as indeed had been previously determined by Mr. Eales. His symptoms dated entirely from June of the present year. First, his left hand dropped for a short time, and the dropping recurred several times, but loss of power was always regained in an hour's time or less. He had also had brief attacks of loss of speech, and had felt numbness in his lips and tongue; but asserted very clearly, in reply to questions, that the peculiar sensation affected *both sides* of the mouth and tongue. He had had numbness in the tips of his fingers and the left hand. It was found quite impracticable to ascertain how often these attacks had occurred, or what was the relation of the different attacks to each other;

all he could be got to say was that "they were pretty much together". None of them occurred under our observation, though he had two or three slight paroxysms of *general rigidity* whilst residing in the hospital. He was sure that the right hand never dropped. Frontal headache had been present throughout, often so severely that "he did not know where he was". He had never vomited. Vision began to fail in July; lately (an indeterminate period), he had been quite blind, hardly distinguishing light from darkness. He spoke of having passed a large amount of urine at one period of his illness; in the hospital, the urine was chiefly passed in bed; specimens examined were found free from albumen, sugar, and phosphates. He was much emaciated, but had been living badly. He was in a state of perfect hebetude. It appeared a matter of great difficulty to him to exert his intellectual functions; though, when he replied, which was only after an interval from the question, the reply was quite pertinent and probably correct. He kept his bed entirely, noticing nothing, often being found with his hand to his forehead. The left upper extremity was manifestly more feeble than the right, though he retained the power of grasping. When placed erect, he could only creep along for a few paces, but he was quite able to stand. Sensation was perfect, so were hearing, and probably taste; some doubt existed as to smell. He became more and more dull, and finally imbecile; rambled at times; passed everything under him. He died on October 25th, sinking rapidly after a rather severe fit of general convulsions, no fresh symptom having presented itself.

ABSTRACT OF LECTURES ON ELECTROTHERAPEUTICS.

Delivered at the Westminster Hospital.

By THOMAS BUZZARD, M.D., F.R.C.P.,
Physician to the National Hospital for Epilepsy and Paralysis.

[DR. BUZZARD has recently delivered a short but very interesting course of lectures on the above subject, at the Westminster Hospital, at the request of the staff. It was much appreciated, and we have pleasure in giving an abstract of the main points that were illustrated.]

He commenced by showing, as an elementary fact, and by effects upon the compass needle, that single plates of metal in diluted acid gave rise to a current; and explained that this simple form was not very available, because of the hydrogen, which soon coated the copper-plate: hence the arrangement of cells, like those of Bunsen and Leclanché, was adopted, in order to secure a more constant current.

Certain terms and formulæ were then defined. *Intensity* was taken as practically synonymous with the *quantity* of electricity, and it equalled the electro-motive force (E), divided by the battery resistance (R), and the body resistance (r). The former being practically nil, the accepted formula to express the intensity was $I = \frac{E}{r}$ (Ohm). Now, intensity was increased in proportion to the *number* of elements. Thus, if six pairs were used, it would be $I = \frac{6E}{r}$; but increase in the *size* of the elements added nothing to the intensity. The resistance of the body, on the other hand, much diminished the intensity at every point; by a sort of backward pressure, it prevented the current from being freely given off, even at its source.

The term *tension* expressed a *quality* of electricity, a kind of expansive force, which varied in degree in different parts of the currents. The explosion of thunder illustrated the result of tension. Faraday calculated that the chemical action of one grain of water on four grains of zinc would represent as much electricity as, in a state of very high tension, would make a thunderstorm.

"*Voltaism*", not "*Galvanism*", was the proper term for the constant current, as "*Faradism*" was applied to the induced.

The making of a coil was then illustrated; and the fact that a current passed through the thick primary wire generated a secondary current in the outer thin wire, at the moment of joining and breaking contact, was very clearly demonstrated by the galvanometer. This secondary current was the "*induced current*" of electro-therapeutics.

Stohrer's batteries and Dr. Tibbitts's combined battery were then shown, and the characters of induced and continuous currents contrasted. The continuous current: 1. Produced continuous chemical action, as shown by decomposition of water, hydrogen being given off at the cathode or negative electrode, oxygen at the anode or positive. 2. It had considerable diffusibility when applied to the body, so that, *e.g.*, a flash would sometimes be produced in the retina during applications to the legs. 3. It produced a sensation of warmth, and in palsied limbs really increased the temperature. 4. It became increasingly felt

with the continuance of the application, owing to some delay in penetrating the skin. 5. It relieved muscular fatigue (Dr. Poore).

The induced current, on the other hand: 1. Gave rise to no perceptible chemical action, but to marked muscular contraction. 2. It diffused itself but little; passed in the most direct manner between any two points of the body to which it was applied. 3. It did not produce warmth, so much as numbed and jarring sensations. 4. It was felt in its full force at first application, as it readily penetrated the skin.

It was noted that the power of the induced current depended less upon the absolute amount of its tension, than it did upon the modification of such tension from one moment to another.

The *modus operandi* of the constant current was still obscure; it was supposed to modify, in some way, the natural electricity of the body. At present, we could only speak empirically of some of the effects. The ordinary nerve-irritability is modified by the constant current, so that it is markedly *increased* at the *cathode* (negative pole), and *lessened* at the *anode*. The former is felt to cause more burning and smarting. This was illustrated upon the arm of a student, the anode being placed over the olecranon and the cathode over the median nerve. At the latter, marked burning was felt, and strong contraction took place on closing the circuit. The poles being then reversed, only moderate pain was felt, and no contraction occurred on closing the circuit, but a slight contraction on opening it. Irritability may be increased in the early stage of rheumatic affections, of hemiplegia, etc., or it may be diminished or lost in later stages, or as in facial palsy. Malingers believe that no paralysed muscles ought to contract with any current, and do their best to control such contraction.

As to *choice of currents*, in palsy cases, it is well to use the one with which the muscles best contract—if with the induced current, to use that only. Two illustrative cases were shown—one of hemiplegia and one of ordinary facial palsy. The former was of ten months' duration, connected probably with cerebral embolism, and the patient had very little power over the left limbs, and considerable palsy of the left side of the face; yet the reaction of the muscles to the induced current was as good as that of the sound limb. In the second case, the same current produced no effect at all upon the right (the affected) side of the face, whilst the sound side reacted perfectly. The Voltaic current, however, from only four cells, when slowly interrupted, acted decidedly upon the right facial muscles.

It is evident from these examples that, if a lesion occur in a higher (cerebral) centre, there is impairment of voluntary power over muscles supplied from that centre, but electro-contraction remains. But, if a lesion, whether from injury or cold, etc., occur in the trunk of a nerve, contractility to both currents is much diminished or lost; only the muscles themselves sometimes retain contractile power, when stimulated by the constant current slowly interrupted. Such muscles tend to pass into fatty degeneration, or to assume the condition of unstriated tissue, and their special reaction is known as "*degenerative reaction*".

Slow interruptions of a constant current are liable to induce severe and continued spasm, as has happened to the urethral muscles, when a sound had been passed into the bladder for Voltaic applications.

Other cases illustrative of treatment were then shown or described.

In a case of wrist-drop from lead, the extensor communis gave no response to induced current, whilst the supinator preserved their irritability; the former, however, contracted with the constant current slowly interrupted. A patient found himself unable to write properly. When the reaction was tested, there was no contraction in the muscles supplied by the ulnar nerve; but the constant current slowly interrupted caused contraction. On inquiry, the man had written for several hours in a very tight coat, which had pressed on the ulnar nerve. He recovered under treatment with the constant current. In another, the muscles of the thumb would not contract to either current; and, on inquiry, lead was found in the drinking-water. In another, with difficulty of writing and spasm of the hand, the irritability of the median nerve was found to be heightened; a clear history of gout was obtained, and the patient recovered under alkalies and iodide of potassium.

A case of protopathic muscular atrophy was shown; and the condition of electro-contraction was contrasted with that of a patient with muscular atrophy secondary to lateral sclerosis, as well as with that occurring in lead-paralysis.

Dr. Buzzard was unable to speak strongly of the benefit to be expected from the application of the constant current to the brain; he thought there was more evidence of its benefit when applied to the spinal cord. In *neuralgia*, on the other hand, it was frequently enough of such signal service that its use should not be omitted; but his experience had not taught him what were the special indications for its employment. It was, perhaps, most beneficial in sciatica.

In *infantile paralysis*, where contraction to interrupted voltaism was preserved, there was considerable advantage from using this form of