are attached to these pillars, and a weak battery (say one or two Daniell's) and two telephones are put in circuit.* Such an arrangement, if carefully made and adjusted, is very sensitive; and, although the wings of imagination have carried some writers much too far as regards the possibilities of the instrument, still many of the results described may be obtained by anyone with little trouble.

To hear the cardiac sounds, the best arrangement I have devised is the following. The microphone is fixed on the membrane of one of Marey's tambours placed vertically, and connected by India-rubber tubing with the cardiograph, also of M. Marey.† Thus the sounds may be transmitted to a distant telephone. It is important to note, however, that their character is altered. They have a resemblance to the cardiac sounds, but they are different in *timbre*. This effect may be well observed by fixing the microphone to the key-board of a piano. The sounds heard in the distant telephone, when the piano is played gently, are sharp and tinkling, like the sounds produced by a musical box, and without the resonance of the ordinary sound of the instrument.

Respiratory sounds can also be heard. When the cardiograph is placed over the larynx, a whisper is transmitted quite distinctly.

So far as my experience goes at present, the combination of the microphone with the telephone does not reveal to the ear of the auscultator new sounds with which he was previously unacquainted; but it can effect the transmission of sound to a great distance. I can conceive it quite possible that even such feeble sounds as those produced by the heart and lungs might be transmitted for many miles and be correctly interpreted by an educated ear. There is a limit, however, even to the capabilities of the microphone. The intermediate piece of carbon must be free to move, and any vibration communicated through solids, liquids, or gases must have energy sufficient to move the carbon, or, at all events, to exert a strain upon it. If it be too weak to do either, then the telephone will make no response.

In these days of rapid progress, when facts are observed which startle even our most learned physicists, it is unwise to predict or conjecture; but the success hitherto obtained by others is a stimulus to every one to be up and doing.

A CASE OF CHOREA TREATED BY THE SUBCUTANEOUS INJECTION OF CURARA.

By DAVID DRUMMOND, M.A., M.D.,

Physician to the Newcastle-upon-Tyne Infirmary, and Children's Hospital.

A BRIGHT intelligent girl of seven years was brought to the Children's Hospital as an out-patient about the middle of October last, two or three weeks after the commencement of an attack of chorea. She was seen at the out-patient room several times, and treated with bromide of potassium and arsenic, for general chorea. On November 12th, no abatement of the movements having taken place, she was admitted into the hospital about six weeks subsequent to the first appearance of the disease—a period when many children suffering from this affection begin to improve. I had been for some time anxious to try the effect of curara, or woorara, as some name the drug, in chorea; but, as a rule, as is well known, the attack only lasts about eight weeks, and, consequently, any experiment is apt to be fallacious; for what influence in the cure is ascribed to the therapeutic agent is most probably due to the *vis medicatrix nature*. But, in my present case, the disease was evidently not going to run its most usual course, therefore I resolved to try the poison, known for a long time to experimental physiologists, but only recently, I apprehend, to therapeutists; the fact that it has been recommended in hydrophobia having brought it prominently before the profession. With some trouble, I procured two drachms of a one per cent. solution—all the curara that was to be obtained in Newcastle. I commenced with five minims by the mouth, about one-twentieth of a grain of curara, three times a day, and in a few days increased this dose to one-tenth. This was continued for nearly a fortnight, but by this time no improvement had taken place, and all the curara was used. Whilst getting some more, I put her upon a mixture containing three grains of croton-chloral and ten grains of bromide of potassium for the dose, three times daily, which in a few days was doubled—that is, six grains of chloral and twenty of the bromide

* I have not had the opportunity of seeing any microphone made by Mr. Hughes. The first one I saw was in the laboratory of Dr. Burdon Sanderson, about three weeks ago. Those I have worked with were made in my own laboratory.

† Marey's cardiograph is an instrument designed for transmitting cardiac movements to a recording tambour; but it is so suitable for auscultation, that I am surprised it has not been used for that purpose. By attaching an India-rubber tube of suitable length to it, one may auscultate with precision at a distance of many feet. The advantages are obvious.

three times daily. This treatment was continued for about ten days, but without effect, the choreic movements becoming more exaggerated. I now received a second supply of curara, as before, in solution, but this proved to be quite inert; even a quarter of a grain administered subcutaneously produced no physiological effects. I was about to give up trial as hopeless, when Mr. Bolam, chemist of this town, procured a few grains of the solid curara, and made a solution himself for me. It was now about fourteen weeks since the origin of the attack, and still there was no appearance of the choreic movements subsiding. Of this solution I commenced, on January 5th, injecting one-fortieth of a grain subcutaneously, only giving one injection in the day. On the 7th, I injected one-twentieth, next day one-tenth of a grain. On January 9th, one-eighth was used; she was by this time very much quieter; on January 11th, one-fifth, after which she was nearly steady. Two days afterwards, she got a quarter of a grain; after this, it seemed unnecessary to push the administration of so potent a poison any further.

It is very unfortunate that the specimens of curara vary in their strength and quality. Curara found in commerce is a brown-black resinous mass, soluble in water, but very slightly so in alcohol. The best solution for hypodermic injections is an aqueous one; it keeps well, and is, I imagine, less painful when used than either the alcoholic or the water and glycerine solution. One grain to twelve minims of water makes a good solution. The curara should be left in the water for forty-eight hours, and then filtered. The dose seems to be about one-quarter to one-half of a grain, though, owing to the uncertainty of the specimens, it is probably well to commence with a much smaller quantity. Of course, the rationale of the treatment of such affections as chorea, rabies, etc., by curara, depends upon the researches of Bernard, who, in 1844, discovered that the poison paralysed the ends of the motor nerves; and Schiff observed that the sensory nerves have to some extent their conducting power destroyed and reflex action much diminished, but that they are less affected, and after a longer interval, than the motor nerves. Death would eventually occur through paralysis of the respiratory centres, the circulation remaining almost normal. Sir Benjamin Brodie managed to save animals apparently killed by curara, by keeping up artificial respiration until the poison was eliminated.

Curara administered by the mouth seems to have very little effect, but should be injected under the skin; Ludimar Hermann explained this fact by showing that, when the kidneys were cut off from the circulation, physiological effects could be produced as well, though more slowly, through the stomach as by direct introduction into the circulation, the poison being very rapidly eliminated by the kidneys. A curious fact, and one which was well exemplified in my case, is that, even after the muscles are pretty well curarised, they react to electrical stimulus as perfectly as normal muscular tissue does. I compared the electrical reaction of the muscles of the forearm of the child, the subject of the present case, with that of a healthy child, and there was no difference. Bernard laid stress upon this fact, and showed that the muscles of a curarised frog, stimulated by the galvanic current, reacted perfectly; but such was not the case when the current was applied to the nerve itself, the muscle supplied by it refusing to react.

CASE OF LITHOTOMY, IN WHICH AN ENLARGED MIDDLE LOBE OF THE PROSTATE GLAND WAS ACCIDENTALLY REMOVED.*

By CHARLES WILLIAMS, F.R.C.S.,

Assistant-Surgeon to the Norfolk and Norwich Hospital.

THE specimen which I have the pleasure to exhibit to you represents an enlarged middle lobe of the prostate gland, accidentally removed from a gentleman on whom I performed the operation of lithotomy.

The patient was a tall thin man, aged 72, living about ten miles from Norwich. He had had symptoms of stone in the bladder for upwards of twelve months. A few months before I visited him, he had passed, on one or two occasions, a large quantity of blood in his urine. Of late, he had suffered much from the presence of the stone. He had been greatly disturbed during the night, and had become low-spirited. His urine was found to be quite healthy and free from albumen. His feet were in nowise oedematous; and he could eat freely. There was a loud systolic *bruit* heard over an extended area of the chest, giving rise to no inconvenience. Five years previously, he fractured the neck of his right thigh-bone; ankylosis of the hip-joint resulted from the injury, and he now walks with a perfectly straight and stiff, but shortened, limb.

On July 6th, with the assistance of Messrs. Morton, surgeons, of

* Read before the Norfolk and Norwich Medico-Chirurgical Society.

Aylsham (under whose care this patient had been), I performed the usual lateral operation, and removed a single stone, of an oval flattened shape, weighing five drachms, of uric acid formation. An enlarged middle lobe of the prostate became engaged between the blades of the lithotomy-forceps, anterior to the hinge, and was unconsciously torn off and came away with the stone. There was free arterial hæmorrhage from a deeply seated vessel, which was without much difficulty seen and secured by ligature. A tube was placed in the wound.

On visiting the patient next day, I found him easy and comfortable. There had been no sickness; he had slept fairly well; pulse 64. The wound looked well; the urine was clear, and dropping freely from the tube and abundant in quantity. On the ninth day, he passed the whole of his urine through the penis, and the wound was healing rapidly. Three weeks later, I found him in excellent health, and the wound perfectly healed. He seldom found it necessary to micturate more than once during the night.

REMARKS.—This case presents some points of much interest.

1. The presence of a loud systolic *bruit* is not a pleasant sign in connection with the administration of chloroform, of which my patient inhaled from four to five drachms, and from which he suffered not the slightest inconvenience, either during the operation or subsequently.

2. The fracture of the neck of the right os femoris had resulted in ankylosis of the joint. The limb was immovably fixed in the straight position; therefore it could not be tied up in ordinary lithotomy fashion, but was held by an assistant in a straight direction over my left shoulder. This rendered the performance of the operation somewhat less easy than usual. The parts forming the perineum were lax instead of being tense, and, in order not to wound the rectum, which was large and flaccid, I passed my left forefinger into the bowel, and retained it until the point of the knife was lodged in the groove of the staff; and, by directing the edge of the knife very obliquely outwards, the rectum escaped injury—an event which, in all probability, would have occurred had not these precautions been adopted. If such an accident had happened, taking into account the age of the patient, I fear the termination of the case would have been unfavourable.

3. The removal of a large portion of the middle lobe of the prostate, though quite accidental, was attended with a happy result. The man was relieved of a trouble which, sooner or later, would have been a source of grievous annoyance to him.

I witnessed the same accident in a case operated on by Mr. Cadge. In the forceps, between the stone and the blades, there came away three masses, which were apparently fibrous outgrowths of the prostate, and which weighed one drachm two scruples. In two months, the wound had healed and the patient was strong and well. Mr. Cadge remarks: "It has happened to me twice before to remove small fibrous tumours of the prostate gland during the operation of lithotomy, and apparently without harm to the patient." (*Transactions of the London Pathological Society*, vol. xiii, 1862.) And he gives the experience of an expert modern lithotomist on this subject, who says: "It has occurred to me, eight or ten times, to bring away portions of the prostate and without noticeable injury to the patient. In more than one instance, it was the prominent third lobe which got between the handles, anterior to the hinge, and was torn off entire; and although I have never known unpleasant results to the patient, and that sometimes he has been benefited in after-life, by having got rid of an useless impediment to a natural function. I would not willingly that such an occurrence should happen, and I try to avoid it by turning the blade of the forceps to the lower angle of the wound as I leave the bladder; but when it does occur, I lay no account by it."

THE POLYURIA OF GRANULAR KIDNEY.

By ROBERT SAUNDBY, M.D. Edin., M.R.C.P. Lond.,
Assistant-Physician to the General Hospital, Birmingham.

DR. GEORGE JOHNSON'S short paper, in the JOURNAL for May 25th, seems to me to call for some response from those who differ from him in the explanation of the facts to which he refers. While admitting the importance of the support Dr. Johnson derives from M. Charcot, in believing the changes in granular kidney to begin in the tubular epithelium, I do not consider M. Charcot's reasoning quite free from objection; and I hope, at some future time, to address myself to that part of the subject. For the present, I am concerned with the latter part of Dr. Johnson's article. Dr. Johnson says: "Dr. Ewald appears to have looked for hypertrophy of the arterioles in the pia mater alone, and therefore his observations are incomplete." . . . "It is probable that the arterioles in other tissues, more especially in the skin, may have been sufficiently hypertrophied to explain the cardiac hypertrophy." Dr. Ewald, in common with most of Dr. Johnson's readers, did not imagine

that it was the part of his theory to exclude certain areas from the "stop-cock" function; but he shall speak for himself. He says: "I have restricted myself to this part (the pia mater of the pons) for many reasons: first, the vessels there are under the same conditions as all the other capillaries of the body which do not lie directly between the heart and the kidneys, so that the changes here found, as far as they are of an universal nature, must affect all the other vessels in the same degree. Secondly, because Gull and Sutton, and Johnson, have found the same changes in the vessels of the skin and intestines as they originally found in the cerebral vessels; so that if I could verify their results on one or the other side, in a limited region, the data would rightly hold for the whole vascular area. Finally, the vessels of the pia mater, as has been said, give the best preparations, and are besides more investigated and studied under pathological conditions, so that it is easier to distinguish the process in question from accidental or different pathological changes." (*Virchow's Archiv*, Band lxxi, Heft 4, page 458). In the second place, Ewald's argument, that "the absence of cardiac hypertrophy in amyloid and atheromatous thickening of the arterioles is against the vascular changes as causes of the heart-affection," must be taken as rather directed against the views of Gull and Sutton than against those of Dr. Johnson. Dr. Johnson goes on to say that "it has been suggested that the polyuria of granular kidney is the result of increased pressure in the Malpighian capillaries, consequent on obstruction in front". The existence of obstruction in front of the Malpighian tufts is proved by Thoma's researches. He found, by making careful coloured injections, that the fluid ran well into the tufts, but beyond them the capillary network had undergone destruction, and the efferent vessels were often obliterated (*Virchow's Archiv*, Band lxxi, Heft 3 and 4). Dr. Johnson says: "Increased pressure on the Malpighian capillaries would cause, not a copious secretion of urine, but an abundant transudation of albumen"; this is scarcely a fair assertion, as the transudation of albumen is the result of the high pressure being carried to an excessive degree. It is a generally accepted physiological fact, that the secretion of urinary water is in direct proportion to the pressure in the glomeruli. (*Foster's Text-Book of Physiology*, page 278, *et seq.*) He then enforces this statement by telling us that, "when, as a result of valvular disease of the heart, the return of venous blood from the kidney is impeded, the increased pressure on the Malpighian capillaries is associated with a scanty secretion of highly concentrated urine, which, too, is often copiously albuminous". In this he overlooks altogether the fact that the rate of flow through the renal vessels is much diminished in cardiac disease; while it is at least more than probable that the blood accumulates, not in the capillaries of the tufts, but in the venous plexuses on the farther side of them. It is not pretended that the blood-pressure in the renal glomeruli is the sole cause of the polyuria of the granular kidney; for while, on the one hand, it is impossible to believe that active secretion could be going on from a gland the secreting part of which was in a state of anæmia, the experiments of Heidenhain, Ustimowitsch, and Grützner have proved the influence which urea, urates, etc., circulating in the blood, have upon the renal function, a fact which Dr. Johnson most properly regards as an important factor in the production of polyuria; but which the experiments of the last quoted observer prove to be totally opposed to Dr. Johnson's view, that the arterioles of the kidney are firmly contracted.* Grützner (*Pflüger's Archiv*, Band xi) found that, when he injected saltpetre into the blood, the renal nerves on one side only being divided, copious urinary secretion followed equally from both kidneys; but, when the general blood-pressure was raised by suspension of the respiration (with contraction of the arterioles, of course, throughout the body), the secretion of urine sank at once in the kidney the nerves of which were undivided; proving that the contraction of the renal arterioles, imagined by Dr. Johnson, is altogether inconsistent with the copious diuresis characteristic of this malady.

There is still one more anatomical point to which I must refer, and that is the increased permeability of the renal vessels which Thoma observed. This change was not susceptible of anatomical demonstration, but was very marked, even in parts of the organ apparently little affected by the degenerative process. The observations of Thoma and Ewald seem to weigh equally against the theories of Dr. Johnson and of Gull and Sutton; for their very careful measurements prove that the small vessels of the kidney have their lumina generally widened. If it could be proved, by careful injection, that there is anything approaching the nature of capillary atrophy present throughout the body, it would be a satisfactory solution of the problem; at present, I do not believe we have any precise knowledge of the nature of the general obstruction which leads to cardiac hypertrophy and increased

* "There is evidence that the renal arterioles in these cases are more firmly contracted and more decidedly hypertrophied than those of any other organ or tissue." *Lumleian Lectures on the Muscular Arterioles*, p. 44.)