that, with intestines at the sides and the fluid in the centre, the woman lying on her back, the fluid must be enclosed in a cyst. On the other hand, here the intestines are floating, giving a clear sound on percussion, and the fluid gravitates to the most depending parts of the cavity, and you will see at once the fluid must be free; the intestines are floating about and the fluid is beneath. As a rule, I suppose, in nine cases out of ten, that would probably be the correct diagnosis. You would say here was a case of ascites, and here, where the intestines are pressed backwards and the fluid is in the centre, that will be encysted fluid; and, in a large number of cases, that will be certainly true. But there will be exceptions. There may be so much fluid in the peritoneal cavity that the intestines do not touch the front of the abdomen, or the intestines may be fastened backwards by adhesions; and fluid, which appears to be in front and is in front, is there, not because it is enclosed in a cyst, but because the intestines are held back. And, on the contrary, one may get a case where fluid is enclosed in the front by coils of intestine round it, and is circumscribed in that manner.

I have had a number of preparations brought down to show the way in which adhesions of different portions of the intestine to each other, and to the abdominal wall, may simulate cysts—may become false cysts. The fluid is encapsuled, not by a cyst, but by surrounding portions of intestine or omentum adhering to each other, or to some part of the abdominal wall. Those are our most puzzling cases in diagnosis at times. In the book on *Diseases of the Ovaries* which I published some years ago, there is rather a full account of all this, with which I need not trouble you now. I ought, however, to have mentioned that this fluid, when it is free, will move from side to side as the patient is moved. If she be turned on to her left side, the intestines go to the right; and when she is turned back again, the fluid sinks to the right side, and the intestines float up to the left. So when she is sitting up more or less, and her shoulders raised, the fluid sinks down towards the lower portion of the abdomen, leaving the upper free. If you put a pillow under the hips and lower the shoulders, it at once goes upwards towards the sternum. So, occasionally, one may be deceived by a cyst of moderate size sinking, if it be movable, from side to side as the patient is moved.

The alteration of the position of the patient makes considerably greater and more rapid changes in cases of ascites than in patients with ovarian cysts; the cyst moves about much more slowly than fluid. The superficial veins are more commonly enlarged in ascites than in ovarian dropsy, and, on measurement, the enlargement of the abdomen in ascites is more likely to be symmetrical than in the ovarian cyst; and then, on feeling the abdomen, you are more likely to get a flaccid abdominal wall in ascites, and a more tense wall in a cyst; and very often, in ascites, the greatest girth is at the umbilical level; whereas, in ovarian dropsy, it is sometimes considerably lower.

The way in which the fluctuation is felt is of some importance in ascites, the wave being a wave felt more immediately beneath the hand and not limited so distinctly by the cyst as in ovarian dropsy. Then, a very important question is the way in which the movements of respiration affect the dullness. Supposing one finds that, as the patient breathes deeply, resonance descends, it will often guide you correctly in a doubtful case by a careful examination as to how far the line of clearness on percussion varies or corresponds with the line of fluctuation. When you get distinct fluctuation beyond the limit of dullness—when you get evidently a wave of fluid moving about amongst the intestines—it is pretty certain that the fluid is free. However it may be limited, if one find that one has a distinct wave of fluctuation where one has a resonance indicating the presence of intestine, that must always certainly be free peritoneal fluid, and it is the same whether it is ascitic fluid or fluid escaped from a cyst. The fluid is free, with the intestines floating about. In the other case, you feel the wave of fluctuation limited exactly by the outline of the dullness. One can very seldom go wrong by carefully observing these two points.

I have said just now a doubt may arise where the quantity of fluid free in the peritoneal cavity is so very large that the front of the abdomen is pushed forward far beyond the reach of the intestines, the intestine cannot reach the surface of the abdominal wall, and so one has the fluid in front.

Another point of doubt and difficulty may arise from air having entered an ovarian cyst; for, as an ovarian cyst contains a certain amount of fluid, it may also contain gas, and either the fluid is decomposed after tapping, or there may have been some possible communication set up between the intestine and the interior of the cyst. Sir Thomas Watson records a case in which a patient had a cyst filled alternately with fluid and with air; when the fluid collected to a certain quantity, it seemed to open some valve-like communication with the intestine, emptied itself, and became filled with air. As the fluid gradually reformed, the air was displaced, and the same series of

changes went through again. I have known a case in which air distinctly entered an ovarian cyst through the Fallopian tube—at least, we could find no other means of entrance; and, after death, it was found there was a large and distinct communication between the interior of a large ovarian cyst and the uterus through an enlarged Fallopian tube; there would have been, at any rate, no difficulty in air entering in that way.

Supposing doubt does arise as to whether fluid is free or not, one has always the resource of tapping; and a chemical and microscopical examination of the fluid that is removed will settle any doubt as to whether it is free peritoneal or ovarian fluid, or fluid of some other cyst. Years ago, before Scherer's test to distinguish albumen in its true form from the forms of albumen that chemists do not seem very accurately to determine, it was sometimes extremely doubtful to arrive easily at this determination. But Scherer showed that the albumen which, when coagulated by heat, is redissolved by acetic acid, is a very different albumen from true albumen. Paralbumen and metalbumen are forms that, when coagulated by heat, can be dissolved by acetic acid boiled. One takes a test-tube and boils the fluid, and the albumen is coagulated. You add some strong acetic acid, and give it a good boil, and shake it up, and if the albumen be true, the coagulum does not redissolve in the acetic acid. But supposing it to be paralbumen, then it either dissolves, or forms a whitish transparent fluid, or breaks up into a kind of jelly-like transparent mass, which is quite easily distinguishable from undissolved albumen coagulated by heat. This was supposed to be, for a long time, a pretty certain means of distinguishing between ascitic fluid and ovarian fluid. If the coagulated albumen were dissolved in acetic acid, it was said to be ovarian; and if it did not redissolve, it was said to be ascitic; and that was frequently right. Sometimes part would redissolve and part would not; and then the supposition was that it was a mixed fluid, some ovarian and some peritoneal, that an ovarian cyst had burst and some of the fluid was in the peritoneal cavity, a certain amount of fluid had been poured out, and there was a mixed fluid, which contained some true albumen and some paralbumen.

After a little further examination, one saw that there were exceptions to this rule. For instance, Dr. Schetelig of Hamburg once examined the fluid from a large renal cyst that I had emptied, and he found paralbumen and cholesterine, and no trace of urea, because the proper structure of the kidneys seemed to have been completely destroyed. He found in another patient that the fibrine found at the first tapping of the patient disappeared after another tapping, and he concluded that, at the first tapping, the fluid was removed from the peritoneal cavity, and, at the second tapping, from an ovarian cyst. It is quite possible, in tapping the abdomen, to remove a certain amount of peritoneal fluid and leave an ovarian cyst behind; and that, at another tapping, the ovarian cyst might be tapped and emptied, and the fluid escape.

After the chemical examination of the fluids removed, one has to resort to the microscope, and this we will consider at the commencement of the next lecture.

NOTE ON THE MICROPHONE AND TELEPHONE IN AUSCULTATION.

By JOHN G. MCKENDRICK, M.D.,

Professor of Physiology in the University of Glasgow.

IN common with many others, I have been deeply interested in the recent remarkable inventions of the telephone, phonograph, and microphone, not only because these instruments promise to be useful, but also because I feel convinced that every advance in the obscure region of molecular physics is destined to have an important influence on our interpretation of many physiological phenomena. The results already obtained have given us a new conception of the delicacy of the organ of hearing and of the action of the membrana tympani, and it is interesting for the physiologist to note that the secret of success with the telephone and phonograph is to imitate as nearly as possible the conditions in which the living organ of hearing works.

The ingenious invention of Mr. Hughes, the microphone, has already been employed by Sir Henry Thompson for the detection of minute calculi in the bladder, and not a few are attempting to apply it to ordinary auscultation. During the past fortnight, I have been trying a number of experiments, the results of which may be of interest, and possibly they may suggest improved methods to others.

The arrangement of Mr. Hughes which I have used consists of two little pillars of gas-carbon, having between them a spindle-shaped piece of the same material free to move in sockets in the pillars. The wires

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.