rise and become cataleptic, and such was the result; that a second touch on the same point would reduce the rigidity, and cause it to fall, and such again was proved to be the fact. After a variety of other experiments, every one of which proved precisely as I had predicted, she was aroused. I now applied the ring of my instrument on the third finger of the right hand, from which it was suspended, and told the doctor that when it was so suspended it would send her to sleep. To this he replied, 'It never will', but I again told him that I felt confident that it would send her to sleep. We then were silent, and very speedily she was once more asleep. Having aroused her, I put the instrument on the second finger of her left hand, and told the doctor that it would be found she could not go to sleep when it was placed there. He said he thought she would, and he sat steadily gazing at her, but I said firmly and confidently that she would not. After a considerable time, the doctor asked her if she did not feel sleepy, to which she replied, 'Not at all'; 'Could you rise and walk?' when she told him she could. I then requested her to look at the point of the forefinger of her right hand, which I told the doctor would send her to sleep, and such was the result; and after being aroused, I desired her to keep a steady gaze at the nail of the thumb of the left hand, which would send her to sleep in like manner, and such proved to be the fact.

"Having repaired to another room, I explained to the doctor the real nature and powers of my little and apparently magical instrument—that it was nothing more than my portmanteau-key and ring, and that what had imparted to it such apparently varied powers, was merely the predictions which the patient had overheard me make to him, acting upon her in the peculiar state of the nervous sleep, as irresistible impulses to be affected, according to the results she had heard me predict. Had I predicted that she would see any flame, or colour, or form, or substance, animate or inanimate, I know from experience that such would have been realised, and responded to by her; and that, not from any desire on her part to impose upon others, but because she was self-deceived, the vividness of her imagination in that state inducing her to believe as real what were only the figments of fancy, suggested to her mind by the remarks of others. The power of suggestions of this sort, also, in paralysing or energising muscular power, is truly astounding; and may all arise in perfect good faith with almost all patients who have passed into the second conscious state, and with some, during the first conscious stage; and with some weak-minded, or highly imaginative, or credulous and concentrative people, even in the waking condition."

ON THE EXTRACTION OF FOREIGN BODIES FROM THE EAR.*

By J. MARION SIMS, M.D.,

Honorary Fellow of the Obstetrical Societies of London, Dublin, and Berlin;
Ex-President of the American Medical Association; etc.

I AM induced to make a few remarks on the removal of foreign bodies from the ear, in consequence of a discussion on this subject a few months ago in the pages of the BRITISH MEDICAL JOURNAL.

In the summer of 1843, Dr. B. R. Jones of Montgomery, Alabama, asked me to see with him a little negro, five or six years old, who had some foreign body in the ear. The doctor had already made fruitless efforts to remove it with a bent probe and forceps. I repeated the operation with the same instruments, and with the same unprofitable result. The ear bled a little, and became so tender that we were obliged to desist, and I went home to get other instruments. I returned to Dr. Jones the next day, with a small syringe and a delicate pair of forceps from my eye-case. We found the child's ear very tender to the touch, and the canal was tumefied and filled with coagulated blood. On the previous day, the foreign body was visible; but now it was obscured by the blood. I at first used the syringe for the sole purpose of washing out the blood, expecting then to remove the foreign body with the forceps. The syringe easily dislodged the blood near the outlet of the canal; but it failed to remove that in immediate contact with the foreign body. For the purpose of dislodging the blood, I caught hold of the lower lobe of the ear and pulled it outwards, so as to straighten the canal, and then injected water forcibly into the ear. Greatly to my surprise, the first jet of water ejected the foreign body from the ear, and it fell on the ground at a distance of fifteen or twenty inches from the patient. I had no thought whatever of removing the foreign body with the syringe. I expected only to clear away the coagula, so as to see it, and then remove it with the forceps. The foreign body was a bit of cut-glass that had formed the ornament of a cheap finger-ring.

* Read in the Section of Surgery at the Annual Meeting of the British Medical Association in Bath, August 1878.

About a year after this, a negro man came to me, saying he had been deaf in one ear ever since childhood, and that with an ear-pick he had just discovered something hard in it. On examination, I found the ear completely blocked up with some dark-coloured substance that, to the probe, seemed to be of bony hardness. It was soon removed by syringing, and proved to be a cherry-stone, which the man now remembered to have pushed into his ear twenty years before, when he was a small boy.

This was the strongest test possible of the efficiency of this means of removing foreign bodies from the ear. For, here was a perfectly spherical body, that completely blocked up the whole tract of the canal; and yet the small jet of water, thrown forcibly in, passed by and dislodged it, so that it was removed with ease. Ever since that, I have never failed to remove with facility any foreign body found in the ear, whether wax, insects, seeds, pebbles, or bits of wood.

The first article* I ever wrote for a medical journal was on this subject, and, as appropriate now, I quote from it the following.

"Whoever follows the loose direction of 'syringing the ear forcibly', will often be disappointed. A sufficient explanation of this may be found in the anatomical peculiarities of the organ. The course of the meatus is curved, running at first forwards and upwards; then downwards and backwards; its concavity looking obliquely downwards and forwards. It is an inch and a quarter or more in length; about a quarter of an inch in its transverse diameter, a little more in the perpendicular direction, its section being oval. Its dimensions vary, being a little contracted at the orifice, still more so in the centre, which is the narrowest part, and again at the tympanum. The anterior semilunar edge of the concha, leading into the meatus, projects considerably below a line forming a tangent with the convexity of the lower wall of the canal. With the obliquity of this canal and its irregular dimensions, it will be found difficult, if not quite impossible, to throw a stream of water with any degree of force against the tympanum, even when the organ is unobstructed; for, if the nozzle of the syringe be directed parallel with the tragus, the force of the injection will be spent against the anterior inferior wall, or convexity of the floor of the canal; if turned more towards the semilunar ridge of the meatus, its power will be lost on the posterior superior wall, or convexity of this tube. But, let us change the direction of the canal by laying hold of the lower portion of the antihelix, pressing the dorsum of the concha forwards while we put the whole organ forcibly on the stretch, pulling it outwards, upwards, and backwards, thus making the meatus a straight channel of diameter, and then it will be found comparatively easy to throw a uniform stream of water to its very bottom. If the foreign body be small, the water will dislodge it as it rebounds from the tympanum; if it be large, plugging up the passage perfectly, two or three injections properly directed (the ear held as described) will be certain to change its relative position sufficiently to allow the next to pass by, impinge on the membrane tympani, and wash it out in its retrograde course, or so far dislodge it as to bring it within reach of the forceps. The syringe that I use holds about two drachms of water. The nozzle is of small diameter, and the piston is perfectly air-tight. Water, thrown from it suddenly and forcibly, does not pass in a long slow continuous stream, but seems to strike, as it were, all at once and with great power. A syringe of small dimensions is preferable to a large one; because, with a small one having a small tube, the stream may often pass undivided by the foreign substance, so that its reflex action is almost as strong as its direct force. On the contrary, in a large syringe with a large tube, throwing a larger volume of water, a small portion only of the stream may pass behind the foreign body, while a larger portion strikes against it in a direct line with greater momentum, thus counteracting the retrograde force."

The principles here laid down are as true to-day as they were a third of a century ago, when they were first penned. So universally successful have my efforts been in removing foreign bodies from the ear, that I was surprised to find that the same unflinching success did not attend the efforts of all other surgeons. And so I felt anxious to ascertain the reason of this. To this end, I went to three of the most renowned instrument-makers in London, and asked to see the best ear-syringes they had; and they showed me large clumsy metal syringes, with large nozzles, throwing a stream of water large enough for a rectum syringe. I then went to some of the best instrument-makers in Paris, and I found just the same sort of ear-syringes as I found in London. It was then plain to my mind that the fault was not in the principle, but in the clumsy and unsuitable instruments used.

The ear-syringe that we use in my country is made of hard India-rubber, holds only about four drachms, is light, and worked with one hand, while the other is used to straighten the canal by pulling the ear

* On the Extraction of Foreign Bodies from the Meatus Auditorius Externus. By J. Marion Sims, M.D. (*American Journal of Medical Sciences*, April 1845, page 336.)

outwards, as above described. But, as said before, the syringes that I find here are of metal, large and unwieldy, with large calibre, throwing a large stream of water, and worked with difficulty.

[Here the writer showed several ear-syringes, and contrasted them with the small hard India-rubber syringe.]

The cut represents the American India-rubber ear-syringe, and the method of using it with one hand. It is two and a half inches long, the same in circumference, and holds about half an ounce. The orifice of the nozzle is one millimètre in diameter. A larger ear-syringe, one holding an ounce, is made by the American Rubber Companies; but I prefer the small one, as it throws a smaller stream of water.

The easy removal of foreign bodies from the ear depends upon two things that are indispensable.

1. A light small syringe, with a small nozzle, throwing a small jet of water suddenly and forcibly, must be used.

2. The auditory canal must be made straight, by traction outward of the lower lobe of the ear, at the moment of injecting water into it.

With these simple directions, failure is impossible.

12, Place Vendôme, Paris.

SOME REMARKS ON DR. JOHNSON'S PAPER ON THE MINUTE ANATOMY OF THE SMALL RED GRANULAR KIDNEY.

BY DR. C. A. EWALD,

Privatdocent in Berlin, formerly Assistant in Professor Frerichs' University Clinic.

It is only now that I find, in the columns of this JOURNAL, a short paper, written by Dr. George Johnson, under the abovenamed title (May 25th, 1878), in which he disputes my theories upon the same subject, published in Virchow's *Archiv* (Band 71, p. 453): "Ueber die Veränderungen der kleinen Gefässe bei Morbus Brightii und die darauf bezüglichen Theorien".

1. Dr. Johnson says that my (Ewald's) observations are incomplete and insufficient, because I have looked for hypertrophy of the arterioles in the pia mater alone, and not in the other parts of the body; and that, in the cases in which I found hypertrophy of the heart without hypertrophy of the arterioles of the pia mater, it is probable that the arterioles in other tissues may have been sufficiently hypertrophied to explain the cardiac hypertrophy; and, on the other hand, it is obvious that hypertrophy of the arterioles of the pia mater without arterial hypertrophy in other tissues would not cause cardiac hypertrophy.

2. He also says that the absence of the cardiac hypertrophy in amyloid and atheromatous thickening of the arterioles is *not* against the

vascular changes as causes of the heart-affection, because here is a mere degeneration of the walls of the arterioles with consequent loss of contractile power, while the arterial contraction and resistance, with resulting arterial hypertrophy, are the main causes of the hypertrophy of the left ventricle.

3. Dr. Johnson further says that the origin of the resistance which causes the arterial tension is not in the capillaries (as I consider), and that my view appears inconsistent with the facts and with the doctrines of vaso-motor physiology. Increased pressure of the Malpighian capillaries would, he says, cause not a copious secretion of urine, but an abundant transudation of albumen. The clinical fact is, that when, as a result of valvular disease of the heart, the return of venous blood from the kidney is impeded, the increased pressure of the Malpighian bodies is associated with a scanty secretion of highly concentrated urine, which, too, is often copiously albuminous.

May it be permitted, in reply to the remarks of this able and competent observer, to state that in general I quite agree with Dr. Johnson as regards the anatomical alterations of the vessels; but on the three foregoing points I have the following remarks to make, upon which I find Dr. Saundby has already partially insisted in his reply to Dr. Johnson in this JOURNAL (June 15th, 1878).

1. Dr. Johnson, as well as Gull and Sutton, always found the state of the vessels of the body identical with that of the pia mater. It was the same in the cases in which I examined, not only the vessels of the pia mater, but also those of the skin and the mesentery, which cases, in fact, were but few. It was permissible, therefore, to adopt the results found in the smaller field of the pia mater for the vessels of the rest of the body. On the other hand, the vascular changes are dependent upon a common cause, and this cause must equally concern all provinces of the body except those between the heart and kidneys. These are Dr. Johnson's own observations, which I followed, and which he now disputes.

2. I did not make use of the absence of cardiac hypertrophy in amyloid, or atheromatous degeneration, directly against Dr. Johnson's theory. They are examples to show that degeneration of the walls of the arteries is not sufficient to cause hypertrophy of the heart, as those would be inclined to think, who believe that degeneration of the walls would impede the flow of the blood, as has been found by Cohnheim in his experiments on inflammation; although, in these experiments, not even a visible degeneration of the walls of the vessels can be observed. But this point is of little interest if the following facts are correct.

3. It is true that the albumen is transuded generally from the Malpighian capillaries, but this only happens in cases where the wall of the Malpighian capillaries has undergone a pathological change. The simple pressure alone is never sufficient to cause transudation of albumen in the glomeruli, but the secretion of albumen goes on through the venous capillaries, when the return of venous blood from the kidney is impeded by valvular disease or otherwise. Nussbaum has shown, in an interesting communication (*Untersuchungen über die Secretion der Nieren*, Pflüger's *Archiv der Physiologie*, Band. xvi und xvii), that the function of the Malpighian capillaries is to balance the quantity of water contained in the blood, independent of the secretory power of the rest of the kidney, and that albumen and sugar pass through the Malpighian capillaries, if they be pathologically changed. The quantity of albumen secreted in the urine depends entirely upon the degree and quantity of the affection of the Malpighian capillaries, and has nothing to do with the pressure in front or behind them (see a very instructive case of Dr. Litten, *Berliner Klinische Wochenschrift*, 1878, No. 22).

I have shown that the view of Dr. Johnson concerning the regulating force of the small vessels is due to a wrong interpretation of a physiological experiment, correct in itself (*i.e.*, the increasing blood-pressure after suspension of the respiration). The action of the heart itself, if it acted in antagonism to the contracted vessels, would tend to counteract the intended result, *i.e.*, to arrest the flow of blood in the affected tissues. Further, I pointed out that, according to the experiments of Heidenhain (Ostroumoff), the vessels contract under a greater pressure, so as to retain their natural lumen quite independently of nerves entering from the outside, which are cut before. Now, as the resistance lies in the capillaries, and causes a higher pressure in the arterioles and arteries, the hypertrophy of the walls of the latter must be a purely mechanical effect caused by the over-tension in the system of blood-vessels, and independent of central influence. Besides, the thickening of the walls of the arterioles found *post mortem* does not prove that there was really a contracted channel during life.

I am not aware of any facts which disprove this theory. I hope that if Dr. Johnson will kindly read my original paper, where all these points are more explicitly related, he will agree with my views.