excited by the act of attention." Of a similar character are many sympathetic phenomena."

In making these corrections as to dates, I by no means wish to do more than set right what appeared to me to be oversight. I have no doubt Dr. Carpenter is sincerely anxious to do me justice, and I am sure no one has so clearly and fully developed my views as he has, and so has greatly facilitated the research. I hope, when I can secure leisure, to develop my views in relation to the will.

THE RELATION BETWEEN ARTERIAL TENSION AND ALBUMINURIA.

By GEORGE JOHNSON, M.D., F.R.S.,

Professor of Medicine in King's College; Physician to King's College Hospital.

I LEARN from Mr. Mahomed's paper in the last number of the JOURNAL, that I had mistaken his explanation of the relation between arterial tension and albuminuria. I now understand his theory, but I cannot accept it as a satisfactory explanation of the facts. While I admit that there exists a physico-chemical, or, as some would say, a vital attraction, between the tissues and certain of the blood-constituents, I deny that Dr. Carpenter or any other physiologist has demonstrated the existence of a "capillary power", the mere diminution or suspension of which would so increase arterial tension as, when long continued, to cause hypertrophy of the muscular walls of the minute arteries. This hypothesis assumes the existence of a mysterious "capillary power", while it ignores the regulating influence of the muscular arterioles. And yet this regulating influence has been conclusively demonstrated by Bernard, Brown-Séquard, and others; and the doctrine has met with very general acceptance. We believe that, while the propulsion of the blood, and its regulated distribution to the various tissues and organs, are effected by the muscular contraction of the heart and of the arterioles under the influence of the vaso-motor nerves, the interchange of materials between the blood and the tissues during the processes of nutrition and secretion are the result of subtle chemico-physical or so-called vital attractions.

A true theory must be consistent with all ascertained facts. There are many facts which are not consistent with Mr. Mahomed's hypothesis. Let the following suffice for an illustration. In no cases of chronic Bright's disease is there greater arterial tension, in none are the minute renal arteries more constantly and conspicuously hypertrophied, than in cases of the small red granular kidney. This state of the arteries would, according to Mr. Mahomed, imply a great increase of capillary engorgement and pressure, and therefore a copious transudation of albumen; yet we find that in many of these cases the amount of albumen in the urine is comparatively scanty, and it may be entirely absent. These facts are intelligible if we admit that the contraction of the hypertrophied arterioles lessens the pressure on the capillaries in front; they are intelligible on no other hypothesis.

That a merely passive distension of the minute arteries, consequent on an obstruction in front, is not sufficient to cause hypertrophy of their muscular walls, is shown by the fact repeatedly observed by Dr. Kelly and myself, that in cases of chronic incompetence of the mitral valve, involving long continued passive engorgement of the lungs, the muscular walls of the pulmonary arterioles present no appearance of hypertrophy. More than twenty years ago, when I first discovered the hypertrophy of the renal arteries in the granular kidney (*Med.-Chir. Trans.*, vol. xxxiii, p. 107), I endeavoured to explain the phenomena by a hypothesis similar to that which Mr. Mahomed now adopts; but I soon found that my explanation was inconsistent with pathological facts and with the established doctrines of physiology.

The main difference between Mr. Mahomed and myself is this. He believes that the increased arterial tension in cases of Bright's disease is the result of an impediment in the capillaries—a loss or diminution of "capillary power". I, on the other hand, maintain that it is mainly due to the resistance offered to the passage of the blood by contraction of the muscular arterioles under the influence of the vaso-motor nerves.

With reference to Mr. Mahomed's concluding remark, I would suggest that there is the same "unity of action" in the propelling heart and the regulating arterioles as in the detrusor urinæ and the sphincter vesicæ; the same, too, as in the muscular coats of the large intestine and the sphincter ani. It is incredible that the muscular walls of the arterioles have no other function than that of resisting the pressure which results from a diminution of "capillary power" in front.

Mr. Mahomed's explanation of the pathology of acute Bright's disease appears to be this. Poisoned blood destroys the attraction between the blood and the tissues. The result is an increase of arterial tension; then follows congestion of the capillaries, and, "from the fact of in-

creased blood-supply, increased cell-production". Hence, according to this doctrine, an increased blood-supply to the tissues results from the suspension of the "capillary power", which is believed to be a motor force; and an increased tissue-formation is determined by a diminution of the vital attraction between the blood and the tissues. Surely these propositions are inconsistent with each other, and the supposed physiological mechanism would not work.

With regard to the proximate cause of albuminuria, it is unquestionable that blood-serum may transude through the walls of the Malpighian capillaries as a direct result of excessive pressure on the walls of the vessels, whether caused by an increased afflux of blood through the arteries, or an impeded efflux through the veins; but there is also good reason to believe that, while certain morbid materials are passing from the intertubular capillaries into the gland-cells, modifying their nutrition and causing a rapid desquamation, the same morbid condition of blood may result in an abundant passage of serous fluid through the Malpighian capillary walls, apart from mechanical pressure either on the arterial or the venous side of the capillaries. I have no doubt that the albuminuria and the intratubular structural changes which are characteristic of acute Bright's disease, are the result of something more than mere mechanical pressure on the walls of the blood-vessels.

CASE OF HYDROPHOBIA IN A CHILD.

By F. RHODES, M.D., Bradford.

CASES of hydrophobia have hitherto been so unfrequent, that few medical men have seen more than two or three cases, while the great majority have not even seen one. Notwithstanding this, it is apparent that the disease is now sufficiently common to make "Painful Death from Hydrophobia" a very constant heading in the newspapers, and to cause the intractability of the disease to be a most serious consideration.

At the onset, the diagnosis is no doubt difficult, from the rarity of the disease, and thus appropriate treatment is delayed until the pathognomonic symptom—dread of water—is fully developed. This, however, eventually comes on, the history of the bite is revealed, and the case draws quickly to a conclusion, giving scant opportunity to the physician to employ his "remedies".

The reports of cases of hydrophobia should especially state in what way the original wound was treated, and how long it was after the bite before such treatment was adopted. The following case, which occurred in my practice recently, may be of interest.

On the evening of January 27th last, I was called to see a little boy between sixteen and seventeen months old. I was informed that he cried, and had a "shivering fit", when moved. I examined him carefully, to detect tenderness about the spine and other parts, but did not succeed in finding any; yet, upon the slightest movement, he started and cried. The child would not drink, and, supposing that there might be disease of the cervical vertebrae, pharyngeal abscess, or some disease about the fauces, I endeavoured to obtain a good view of the back of the throat; but, as he struggled violently, the mother would not permit me to persevere, and I merely saw that there was phlegm at the back of the mouth. I also examined the teeth, but the child held its mouth so firmly closed that this was difficult. The pulse was 160, and the temperature in the axilla about 101 deg.

On December 26th, 1873, the child was found, after having been left a short time, lying crying upon the floor, and the dog beside him, according to the supposition at the time, licking a wound upon the child's cheek, inflicted by a fall which the little boy had apparently had. The parents, however, seem to have had some misgivings about the nature of the wound, and in a few days obtained the advice of a surgeon in Halifax, where they then resided. The wound was then covered with a scab, and nothing could be made out regarding it. Upon inquiry, the father of the patient told me that the dog was his own, and that it was not mad. The marks upon the child's face were now clearly those caused by a bite. Two days before I saw him, he began to show signs of restlessness, and, according to the father, appeared as though he had "caught cold". There had been slight cough, but not of a barking character, and throughout it did not assume that form. Upon the morning of the day before I saw the child (the 26th), it drank some tea all right, but did not eat, and it was noticed to start when asleep.

On the morning of the 28th, the dread of movement continued, and that of fluid also. The child had not slept during the night. The mother then, apparently more anxious for the safety of the child than desirous of vindicating the character of the dog, gave me the following account: The dog, a valuable smooth fox-terrier, had always been very savage, and was much disliked by the neighbours. It did not bite at

everything in its way, but the day after the child was bitten, when out with the child's father, it attacked every dog it met, and, among others, bit a cur dog kept at the next house. This dog had a great deal of hair on, and perhaps owing to this cause had, on January 28th, when I caused inquiries to be made, shown no symptoms of rabies. On the same day that the child was bitten, they found the cat dead in the cellar, whether worried or not, they did not know. The doctor advised that the dog should be poisoned, and this was accordingly done.

The child passed urine normally. There was no frothing at the mouth, but the tongue was coated with viscid saliva. In the evening, the same symptoms were apparent, the child going into paroxysms of fright if a cup were brought near him, and yet he could swallow when forced to do so. The pulse was 180 in the minute. The child died early next morning, death appearing to result from exhaustion. There were no convulsions. The period of incubation was thirty days, and the duration of the disease about four days.

The treatment adopted consisted of an aperient powder, containing rhubarb and calomel, which acted upon the bowels, wet sheet packing, and a mixture containing bromide of potassium and tincture of conium in ordinary doses. Ice was ordered, but, I believe, was not obtained.

I have nothing new to offer with regard to the treatment of this affection. Whether large doses of *succus conii*, or the application of electricity in some form, might allay the painful spasmodic action of the muscles of deglutition, I cannot say. Chloroform and sedative hypodermic injections would, no doubt, be useful in some cases. Whether or not successive transfusions of blood from some animal not subject to hydrophobia, might be of service as a palliative and restorative until the crisis were passed, without experiment it would be rash to speculate, especially as we do not know when such crisis would occur; indeed, from the invariably fatal result in the human subject, we scarcely know that it would occur at all, except that, by analogy, we may infer that it does, as cases of recovery in the dog are said to take place.

In the ordinary preventive measure of applying caustic to the wound, I have little faith. If the dog be not mad, it is a needless cruelty; and if it be, from what we know respecting the rate of the circulation, and the rapid action of drugs used hypodermically, we may be sure it is of no use if the true skin be penetrated, unless the virus takes its stand temporarily in the tissues around the bite, and requires to undergo development there. If we can, by excising a hard chancre, prevent constitutional syphilis, we may hope similarly to prevent hydrophobia by excision of the wounded part, and the subsequent application of caustic. Very probably, inflamed and suppurative glands are often produced by the free application of caustic to wounds, thus inducing, in debilitated subjects, a cachectic condition, if not so fatal as the disease we hope to prevent, still much worse than what would really have ensued.

A CASE OF APHASIA.

By E. SCOTT JONES, L.R.C.P., Honorary Medical Officer to the Weston-super-Mare Sanatorium.

MR.—, a gentleman 32 years of age, had always been a hard drinker, and led a very dissipated life. He had occasionally been under my care for bilious attacks, etc., brought on by his indiscriminate way of living.

One afternoon, in the month of August 1870, he came into my consulting-room, complaining of a pain in his right eye. He evidently had been drinking. I accordingly did not pay much attention, and ordered a mustard plaster behind the ear, and a little aperient medicine.

The next day, I was sent for, and was informed by the wife that she considered her husband very queer; that he had been tipsy the night before, but did not appear to recover. He had slept well. He appeared to me very much the same as I had seen him on former occasions, when under the influence of alcohol; but, on closer examination, I found him suffering from aphasia. On my showing him my watch, and asking him the hour, he told me quite wrong, and, in fact, could not talk sense.

Were I to relate the whole history of this case, I fear it would be tedious to my readers. Suffice it to say, he continued in much the same condition until the commencement of October, sometimes getting better, and then again falling back. He was able to walk out, and looked well, and recognised his friends, but always addressed them by the wrong name. If asked to repeat the alphabet, he invariably mixed the French and English together, and, in fact, presented all the aphasic symptoms. At the commencement of October, he was obliged to take to his bed, and gradually sank into a low typhoid condition, death being ushered in by convulsions.

At the necropsy, all the thoracic and abdominal viscera were healthy, but the brain, although I made the examination only six hours after death, showed general softening; in fact, the whole of it was in such a state of degeneration, that it was impossible to make any proper examination.

THERAPEUTIC MEMORANDA.

TREATMENT OF ROUND-WORM.

IN the JOURNAL of May 23rd, several cases of round-worm are reported. A peculiar case came under my care as an out-patient at the Bristol General Hospital in February last. The patient, a child aged one year and eight months, had been suffering for three weeks with a very irritable state of the bowels, so much so that it could not be kept clean; and during an evacuation a round worm was passed. The child looked pale and ill; it had been fed with the ordinary diet of the house and pork. A mixture of a grain of santaline and syrup was ordered to be taken three times a day. On the next visit (four days), the child had voided forty-six round-worms. It seemed more cheerful, and was to continue the mixture. At the next visit, fifteen more had passed, making in all sixty-two worms. Afterwards the child improved rapidly.

THOMAS E. CLARK, M.D., C.M., Clifton.

SURGICAL MEMORANDA.

TREATMENT OF PALMAR ANEURISM.

MR. CRIPPS's interesting, but most troublesome, case of traumatic palmar aneurism illustrates the serious nature of the disease when treated on the established principle of laying open the sac in order to secure the wounded points. Five cases are mentioned by Mr. Cripps as having been treated in this way; three died of secondary hæmorrhage, and two were cured, one of these being Mr. Cripps's own case, in which, after several recurrences of bleeding, the ulnar radial arteries were first tied, but without avail, then the brachial with success. Nine cases were cured by pressure, two others were treated by ligature, and one by injection. I can add another case to the list cured by pressure, which will, perhaps, be of interest in connection with the preceding one. On May 1st, a healthy-looking man, aged 30, was admitted to St. Thomas's Hospital under my care. He had cut the palm of his hand a month previously with a saddler's knife, near the ulnar border. The wound, which was strapped up by a doctor, soon healed, and for three weeks he experienced no trouble. Then the hand began to swell, and he experienced much pain. On admission, a week after, the tumour was of the size of a walnut, occupying the ulnar border of the palm close below the wrist. The skin over the most prominent part was thin and red in colour; the pulsation was very strong, and the tumour very tense. Compression of the arteries at the wrist stopped the pulsation, and the sac could be partially emptied. Subcutaneous injections of ergotin, with instrumental pressure on both radial and ulnar arteries near the wrist, were tried until the following day. No good effect followed; the tumour was larger, more tense, and the skin seemed likely to give way. The forearm was now carefully bandaged with flannel, placed upon a posterior splint, and well elevated upon pillows; the tumour itself was left uncovered, and an ice-bag laid upon it. Digital pressure was now applied to the brachial artery, the directions given being, that no drop of blood should be allowed to pass. After an interval of forty hours, the aneurism was found to have become harder, and without a trace of pulsation. The following day, it was imagined that slight pulsation could be detected; this became next morning somewhat more distinct, and was perceptible during the next eleven days; but it was always very faint, and the tumour, nevertheless, continued during the whole time to diminish steadily in size. By way of precaution, digital pressure was applied afresh for twenty-four hours without altering the condition of matters, and the arm was constantly kept upon the splint. On the 16th, a fortnight after the pressure was commenced, it was observed that, during three or four hours at a time, the pulsation wholly disappeared; would then return for a few hours, and again disappear. During this interval until the final disappearance on the evening of the 16th, it was clear that the cause of the pulsation was a supply of blood from the radial artery; pressure on that artery stopped the pulsation; pressure on the ulnar artery produced no effect whatever.

To-day, May 26th, I may pronounce the patient cured. There has been no pulsation in the aneurism during the last ten days. The tumour is being gradually absorbed, and is now about one-third its original bulk. I conclude, that the return of pulsation was due to the contraction of