The nodules found in the brain consisted of the new growth, and had the same microscopical appearance as the post-orbital tumour.

Time will hardly allow me to make many observations on this singular case. I will, however, direct your attention to one or two important points. The first that struck me as most singular was the immense amount of fat covering the body of this boy, who, up to the last, seemed to be able to assimilate a large quantity of food. The next was the great similarity in naked-eye appearances which the disease bears to cancer; the distribution is also very like that of cancer. One important difference which is worth noting is that, in cancer affecting the kidney, we usually have only one of the organs affected, or, at least, the bulk of the disease is in one; in this case, the kidneys were as nearly as possible the same size and appearance. I am aware that many observations that ought to have been made both during the boy's life and after his death were omitted; but my excuse is that this case was at a considerable distance from my house, and the *post mortem* examination was only granted after a great deal of pressure, and had to be made in the room of a small dwelling-house. I must, therefore, ask you to overlook the imperfections, and accept my paper as a small contribution to the slight knowledge which we already possess on this subject.

Dr. Murchison, speaking of his own cases (*Pathological Transactions*, vol. xxi, page 377), says:—"Although, in their ubiquity and in their implication of the glandular system, these new formations resembled tubercle or cancer, and to the naked eye were much more like cancer than tubercle, yet in structure they differed from both, and were made up of minute spheroidal bodies, which appeared to be lymph-corpuscles, held together by a transparent stroma.

"Dr. Wilks describes the new growths as composed of cells scarcely distinguishable from the normal secreting bodies of the glands, and as to malignancy, holding a place intermediate between cancer and tubercle. He also insists that the blood in these cases does not contain any excess of white corpuscles.

"Frerichs describes these new formations as rounded or rarely elongated nodules, about the size of millet-seeds, and of a greyish-white colour, scattered through the parenchyma of the liver. They are made up of densely aggregated nuclei, and of small, slightly granular, rounded cells, between which, especially at the periphery of the mass, a delicate fibrous tissue may be observed. These nodules are connected with the small blood-vessels, in the walls of which they are developed, and the channel of which is thereby narrowed and occasionally blocked up. Smaller new formations of this nature may likewise be found between the hepatic cells, when they are developed in the connective tissue of the gland and compress the cells.

"Messrs. Cornil and Ranvier, in their *Manuel d'Histologie Pathologique*, vol. 251, call this disease lymphadenoma; and state that the morbid growths are composed of the adenoid tissue of the liver, and vary in size from a miliary granule to the head of a fetus. In the lymphatic glands, they appear to be a simple hypertrophy of these organs; but, when several adjacent glands are invaded, they become fused into a common mass. To the naked eye, they resemble encephaloid cancer; they are soft and greyish, with islands of red, corresponding to dilatations of the vessels or small hemorrhages; and sometimes they present opaque, caseous, or lardaceous patches, and yield a milky juice."

CASE OF DISLOCATION OF THE JAW, REDUCED AFTER AN INTERVAL OF THIRTY-THREE DAYS.

By GEORGE MAY, Jun., F.R.C.S.,
Surgeon to the Royal Berkshire Hospital, Reading.

AMELIA HAYNES, aged 24, a servant living at Weston-super-Mare, had had good health, except that she had suffered for nine months from loss of voice. On Sunday evening, October 24th, she received a slight blow on the right arm, which does not appear to have either hurt or frightened her; but, during the night, she awoke with her mouth open. She consulted a druggist, who gave her some medicine, and, laughing, told her that she had taken cold. At the end of a week, her mistress sent her home, and she saw a medical man, who tried to reduce the dislocation, but, failing, advised her to see me. She called on me on Monday, November 22nd. The jaw was then dislocated on both sides. I tried to reduce it in the ordinary way, and then by placing pieces of cork in the mouth, and using the jaw as a lever, but without success.

Mr. W. Colles, in the *Dublin Hospital Gazette* of July 15th, 1855, describes a mode of reducing this dislocation, which appears to possess many advantages, viz.: the surgeon standing behind the patient, the

head applied to his breast, and the thumbs turned inwards on the corresponding angles of the jaw, the fingers under the bone in front; but this plan also failing, I persuaded her to go into the Royal Berkshire Hospital. The next day, I and my colleagues again tried to reduce it, the patient being under the influence of chloroform, but again without success.

I now obtained a powerful forceps from Messrs. Weiss. It consisted of two horseshoe-blades to fit within the mouth, and these were separated as the handles were brought together. With this instrument, on November 26th, I again tried; and, after using much force, felt some ligament appear to yield; my colleague, Mr. Moxhay, was now able to reduce it without difficulty in the usual way, the left side yielding first. Had we again failed, it was intended to procure an instrument suggested by Mr. Moxhay, namely, a forceps with a horseshoe-blade to fit within the mouth, and a shorter blade to fit outside the chin, thus forming a powerful lever.

The case is interesting, not only from the length of time which elapsed before reduction, but also because it was mistaken for a hysterical affection.

THE TREATMENT OF PUERPERAL (URÆMIC) CONVULSIONS BY NON-NITROGENOUS DIET.

By T. CHURTON, M.R.C.S.

IN the JOURNAL of May 23rd, 1874, I related a case of uræmic convulsions occurring in the seventh month of pregnancy, in a patient who recovered and was subsequently delivered at the full term. I have since had two other puerperal cases, one before, the other after labour, and one non-puerperal case of similar general convulsions; also, a patient who was threatened with convulsions, but in whom they were, or seemed to be, averted by treatment.

CASE I.—Mrs. W., a small pallid woman, in the sixth month of her first pregnancy, was found in a strong fit shortly after dinner. After the fit, she was unconscious; her eyes were closed; nothing could be elicited from her. The fits recurred at short intervals. She was hot; her pulse was quick; she had vomited. She had been out the night before in a cold frosty wind (it also turned out afterwards that she had probably had albuminuria for some months). A large fire was lighted; blankets were thrown over her; all food or drink was warmed before being given; nothing but starchy foods, cream, sugar, and tea (which is said to restrain the waste of tissues and formation of urea) was allowed; no milk, no beef-tea, nor broth. The fits lasted only a very few hours; next day, she was perfectly conscious, and was then confined of a living child, which, though very small, survived a day or two. The first urine that could be obtained was albuminous, and had specific gravity 1,005. Some months after this, and after prolonged treatment by steel and acetate of ammonia in mixture, the specific gravity was only 1,012, and the albumen persisted.

CASE II.—A primipara was seized with violent fits an hour after an apparently ordinary labour. Fifteen hours afterwards, I saw her. She was then unconscious and dusky, but the fits had somewhat abated in violence; she had had sinapisms and bromide of potassium. A warm enema now given seemed to relieve her decidedly; and a small dose of mixture of carbonate and sulphate of magnesia checked a sickness and retching which were troubling her; but a dose of bromide soon afterwards was followed by a very violent fit (perhaps because the dose was given cold). There was no discharge from the uterus, and for some hours no urine. The treatment by warmth and non-nitrogenous diet, then commenced, was in this case unsuccessful, but it was begun late; it is not certain that it was properly carried out; and it is even probable that some cerebral lesion had already taken place. The patient died comatose sixty hours from the time of delivery, not having been conscious since the first hour.

CASE III.—A boy, aged 14, had albuminuria and dropsy after scarlet fever. At the end of a month, violent convulsions and unconsciousness. The weather was very cold. The same treatment was followed, with, in addition, a large sinapism across the middle of the back. In a few hours, he passed a little urine. Next day, he was perfectly conscious, and he ultimately recovered perfectly.

CASE IV.—A primipara, near her full term, complained of giddiness and mental confusion. Her legs were dropsical, pitting rather deeply upon pressure; urine albuminous; she was rather feverish. The treatment consisted of warmth, non-nitrogenous food, an aperient pill, and three minims of antimonial wine every two or three hours. Next day, she felt very much better. In the evening, however, labour came on; nothing untoward occurred.

This treatment of "puerperal" convulsions depending on uræmia is

easy, simple, and, I think, rational; it is adapted to those cases of convulsions which are caused by acute congestion of the kidneys, more especially when the blood already contains an excess of effete materials, as it often does in pregnant women with (sometimes unsuspected) albuminuria, and in persons having chronic kidney-imperfections. Non-nitrogenous food can add nothing to these injurious matters in the blood, during the time when the skin and intestines are being employed in clearing away those which already exist in it.

CLINICAL MEMORANDA.

PECULIAR ALBUMEN.

FROM the notice taken in the JOURNAL of February 26th of my note on "Peculiar Albumen" in the JOURNAL of February 12th, I find that, in endeavouring to be brief, the full meaning of what I intended to convey has not been understood.

I first observed that, at a meeting of the Manchester Medical Society some time ago, I mentioned a form of albumen which could not be immediately detected by heat or nitric acid, separately or together; but that, after the urine so tested had stood in the tube for some time, the albumen began to appear. I consulted a very eminent professor of chemistry as to this form of albumen, but he had never heard or read of such a form, nor could he offer any explanation of the fact observed.

I then briefly recorded the action of heat and nitric acid on the urine in the case which I lately met with, and added, "thus, from the urine, one tube shows one-third of albumen, and another none at all"; and as both results were obtained by methods of testing for albumen which many may employ, and as the negative result was obtained by rendering the urine only very slightly more acid by the addition of nitric acid, I concluded "that the commonly used tests for albumen are unsatisfactory".

Now, Dr. G. Johnson says: "There is nothing peculiar in this. It is a well known fact mentioned in most text-books that, with a small quantity of nitric acid, as with other acids both mineral and vegetable, albumen forms a compound which is not coagulable by heat, but readily coagulable by an excess of nitric acid. It should be borne in mind that urine to which a drop of nitric acid has been added is not 'the same urine' as that which is unmixed with the acid." But, I submit, may not the urine be found in some cases containing the very proportion of acid (whatever acid it may be which causes the acidity of urine), vegetable, or mineral, necessary to render the albumen incapable of being acted on by heat? I inferred that it might; and it was that inference which led me to reflect on the present tests for albumen.

Mr. Charles A. Rayne, in the remarks he makes on my note, says: "It is important to remember that the free acid ordinarily present in normal urine" (and I suppose in abnormal urine too) "will do this; a fact, I believe, never pointed out before: and thus in cases where the amount of albumen is small, heat alone is an unreliable test." Thus Mr. Rayne has, I suppose, met with a case (for he calls it a fact), which I inferred might occur, in which heat failed to detect the albumen. He, however, says the amount of albumen must be small when heat fails to detect it; but if, in the case I have mentioned, the acid had been in the requisite proportion, heat would have failed to detect the albumen, although it existed to the extent of one-third. And though there might be "nothing peculiar" in the action of the tests I recorded, I am glad to find that the inference I drew from that action may now no longer be regarded as an inference, but as a fact.

Further, as regards the urine, the reactions of which with heat and nitric acid I gave in the JOURNAL of February 12th, I may add that I have tested another sample with the following results.

1. It was clear, normal in colour, of specific gravity 1.014.
2. A few drops of nitric acid in cold urine made no change. When a quantity equal to a quarter of the amount of urine was added, it was white and opaque. When the nitric acid was further added, it began to clear; and when a quantity equal to half the amount of urine was added, it was quite clear. On boiling this, no change was produced.
3. On boiling, the urine became white and opaque. On adding nitric acid, it began to clear with effervescence; and, when an amount equal to the quantity of urine had been added, it was clear, with small whitish particles floating in it. These white particles, on standing, were deposited as a mere coating at the bottom of the tube. On reboiling, they again spread through the fluid.
4. Boiling made the urine become white. One drop of nitric acid

added caused the white substance to form small particles, which, on standing, were deposited, forming one-fortieth of the fluid, which was slightly turbid.

5. Nitric, hydrochloric and sulphuric acids, gave the same reactions when used as follows. *a.* To half a tube of urine was added one drop of acid. No change took place. Boiling this made no change. *b.* The same tube was emptied, and, without washing, it was half filled with urine, and boiled. No change took place. *c.* The same tube was emptied, and, without washing, half filled again. Boiling caused it to become white and opaque.

6. *a.* Half a tube of urine, boiled with one drop of acetic acid, gave a slight dimness to the fluid. *b.* Half a tube, boiled with two drops of acetic acid, gave less dimness. *c.* Half a tube, boiled with three drops of acetic acid, remained quite clear.

Whether albumen forms with nitric and other acids a compound which is not coagulable by heat, as Dr. Johnson asserts, I am not aware, never having seen the compound, or tried its reactions; but I should not be astonished if some one should prove that the abnormal ingredient in urine, such as that whose reactions I have given, is not albumen at all.

JOHN HADDON, M.A., M.D., Eccles, Manchester.

As the question of albumen or no albumen in urine appears to some a matter of doubtful definition, allow me to suggest the following practical points for observation. They are essentially practical; and let me add that, as clinical observers, our object should be not to problematise simplicity, which is often done, but rather read Nature as she is—simple, benevolent, and to the letter true.

1. Given a test-tube of perfectly clear urine, it matters not whether it be acid or alkaline, the application of cold nitric acid carefully introduced into the tube (that is by tilting the tube and allowing the acid to trickle gently down the side) will infallibly produce a cloudiness if albumen be present; this might be over the surface of the acid, at the bottom of the tube, or, at the expiration of twenty seconds, at the upper third of the tube.

2. Given a turbid urine of lithiasis, there is no more difficulty (if it be remembered that albumen separates at 170 degrees—that is, 42 degrees below boiling point). Let this urine be placed in a test-tube, and held over a spirit-lamp; the urates will dissolve as soon as the urine becomes warm; then, as it reaches 170 deg., the albumen will be seen to separate at the upper third of the tube. Then with this be satisfied, and do not boil the urine; for if this be done, and the albumen be in small quantity, it will be redissolved. In order to test the accuracy of the experiment, filter away the urates, and test with cold nitric acid as before.

3. Given a semi-turbid, pellucid, pale urine, which one knows by experience to be phosphatic; and here, if the amount of albumen be slight, a little difficulty may be experienced; and the best mode of procedure, if accuracy be required, is to filter away the insoluble material, and to very carefully add cold nitric acid as before. It is a most unsatisfactory mode of procedure to boil the urine and at once to add the nitric acid; for, by this oxidising process, new compounds are undoubtedly formed, and the albumen will be either redissolved, or, perhaps, chemically changed. Let me say that, if nitric acid be added to a cold clear solution of phosphatic urine, and it gradually become homogeneously pellucid, this is not due to albumen, but to saline matter.

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THERE is a practical point in reference to heat and nitric acid as tests for albumen with which I have been long familiar. If a test-tube in which nitric acid has been added to urine be used without washing the tube, for testing the same urine by heat alone, it sometimes happens that no precipitate is obtained by heat, although previously thrown down by the acid. The small quantity of dilute nitric acid adhering to the sides of the tube when its contents were poured out, is the cause of a phenomenon which might prove the source of a serious error. I agree with Dr. Rayne that, in the case under discussion, a definite compound is formed between the nitric acid and the albumen which is not coagulable by heat. There is another phenomenon connected with urine-testing upon which I wish some one would throw light. A copious precipitate of phosphates when urine is boiled is common. But I have occasionally observed that when a second portion of the same urine from which a precipitate has been in this way obtained, is boiled under apparently the same conditions, the liquid remains perfectly free from deposit. As a preliminary inquiry, although several explanations have been offered, it would, perhaps, be well to ask what is the true cause of the deposition of phosphates by heat.

ARTHUR LEARED, M.D.