ON ALBUMEN TESTS AND PEPTONURIA VERSUS ALBUMINURIA.

By GEORGE OLIVER, M.D.Lond., M.R.C.P.Lond., Harrogate.

DURING my absence from home, I find that the question of peptonuria *versus* albuminuria in connection with the tests for albumen now on their trial, has been raised in the JOURNAL and talked about. Before reading the article on "Picric Acid as a Means of Distinguishing Albumen from Peptone" which appeared in the JOURNAL of March 31st, I had performed several experiments on albumen and peptone, with a view to discriminate these bodies by the reagents which, last January, I introduced to professional notice in the form of test-papers. The results of these observations will, doubtless, just now be interesting to many, who are working with these and other tests, so as to decide for themselves their comparative range and power, and I will briefly record them without delay, before proceeding to further inquiry.

The peptone was derived from digesting at a temperature of 100°, with pepsina porci or pepsin-essenz (Liebreich), and an acid (hydrochloric or lactic), one or other of the following forms of albumen: (a) White of egg boiled for half an hour, or (b) albumen purified after the method of Wurtz, (c) albuminous urine (ser-albumen). After several hours, eight or ten, a final boil enabled me to filter off a clear solution, which, judging from the negative results obtained by boiling and by nitric acid, was free from ordinary albumen. Peptonised albuminous urine appears to me to be a valuable guide in the inquiry as to whether peptones can be discovered in the urine as well as albumen, for it provides, as a starting point, peptone, derived from serum albumen in the urine itself, and the behaviour of it to the tests can be more safely compared with that of albumen in ordinary albuminuria, than when the peptone or albumen is added to normal urine; and surely, to note accurately the operation of the tests upon the mixtures in different proportions of the same urine—one portion albuminous and the other peptonised—should be the right preparation for the study of this obscure corner. Speaking merely from my own observations, I must confess to have derived a ray of light from these experiments, for they explain certain results in the testing of some urines, which must otherwise have remained unintelligible to me. Two drachms of liquor pepticus (Benger), which requires no acid, or one ounce pepsin essence (Liebreich), with one drachm of lactic acid, should be added to eight ounces of urine. The process of conversion can be followed step by step. With Benger's preparation, the coagulation by heat and nitric acid steadily diminishes, until it ceases entirely; but, when lactic acid is used, even in the small proportion of .12 per cent., heat falls from the first to induce coagulation; but nitric acid (after Heller) is still available as an indication of the progress of the digestion. It is best to neutralise all acid by carbonate of soda before boiling and filtering. Inasmuch as liquor pepticus, diluted as in the urine, after boiling, gives reactions like those of a peptone, it is necessary to boil and filter two drachms of it in eight ounces of water, or normal urine, and to use it side by side of the peptonised urine, as a check in estimating the peptone contained in the latter; but this is not necessary when the digestive selected is pepsin essence with lactic acid.

1. *Peptone*.—An immediate turbidity was induced by the potassio-mercuric iodide, sodium tungstate, and picric acid test-papers (the corresponding solutions were also used for corroboration), but it was most dense with the first. In every case it remained as an opacity without coagulating, and it completely and quickly dissolved up, without collecting into clots, on applying heat, even short of boiling, and as readily reappeared as the liquid cooled. Much less heat was required to clear up the mercuric and picric turbidity than the tungstate one. On the other hand, the potassium ferrocyanide paper gave negative results.

2. *Albumen*.—The precipitates which all these tests give with serum-albumen can be readily distinguished from the peptonic opacity by the application of heat; for, while the latter merely dissolves, the former gather up into clots, and will then either float to the top like clotted cream, or will fall; in either case, the urine is left quite transparent, until the coagula are diffused through it by shaking. After warming—for the result described can be reached without boiling—the dense coagula remain as they were left by the heat, while, as it cools, the urine becomes turbid; then the turbidity collects into flocculent coagula, which, with the heavier ones, fall, and the fluid above is left quite transparent. But the result is different when the peptonised is mixed with the albuminous urine: then the coagula form on heating, and fall, but the supernatant

liquid does not become clear; it remains opaque, and the opacity is proportionate to the amount of peptonised urine which has been added. Can we thus readily detect the presence of albumen only in one case, and of albumen with peptone in another? Perhaps, before positively deciding this question, it will be safest to wait for further clinical and experimental evidence. It may appear a small thing to mention—but it may be a matter of practical convenience to be reminded—that about an inch column of urine, or the upper part of a longer column, may be readily boiled, *without the slightest smoking*, by a wax match or candle, providing the tube is held a quarter of an inch above the tip of the flame; the spirit-lamp—cumbersome, and so apt to get out of order—is quite unnecessary; and, besides, the heat can be more carefully graduated, according as warming or boiling is required, by a wax match than by the spirit flame.

Of the four reagents which form the series of albumen test-papers, there is but one which, though precipitating albumen, leaves peptone untouched,—viz., potassium ferrocyanide. I take it, this fact may be turned to useful account: for it is pretty clear that serum-albumen cannot be distinguished from peptone by the use of one reagent only without calling in the aid of heat; while such aid may be dispensed with entirely by employing the ferrocyanic as well as another precipitant; thus, the presence of peptone may be inferred if the potassio-mercuric iodide gives an opacity, while the ferrocyanide test does not, and the latter, though negative as to peptone, is positive as to albumen. Should both bodies coexist, it will be easy to compass that fact, by first precipitating all the albumen by the ferrocyanic test, filtering, and testing the filtrate for peptone by the potassio-mercuric iodide; or, using the latter test only, a match may perhaps decide the matter without the necessity of filtering.

The mercuric test is undoubtedly quite as sensitive for peptone—or the group of bodies that go by that name—as I have proved it to be for albumen in all forms; in both respects, it is unsurpassed or even equalled by any other reagent with which I have worked. Experiments teach me that peptone is not a single body: I have, for instance, obtained evidence that picric acid can leave something which potassio-mercuric iodide can still throw down; while in every case the latter reagent completely cleared away all the peptones, so that, after using it, picric acid induced no further precipitation.

Keeping in view this matter of peptone and the forms of albumen—acid and alkaline—met with in urine, all requiring to be discriminated in various ways and by different tests, very few trustworthy observers will, I think, feel disposed to pin their faith to one or even two reagents, until, at any rate, they have carefully and without bias worked with all of them side by side for some time, and deliberately chosen the most trustworthy in all the various kinds of urine.

ON THE DELIGATION OF LARGE ARTERIES BY THE APPLICATION OF TWO LIGATURES AND THE DIVISION OF THE VESSEL BETWEEN THEM.

By JAMES BLACK, B.A.Cantab., F.R.C.S.,

Demonstrator of Anatomy and Surgical Tutor to the Westminster Hospital.

IN reference to the very interesting communication upon the above subject which appeared the BRITISH MEDICAL JOURNAL, for April 7th, and in which Mr. Walsham cited three cases of popliteal aneurysm which he conducted to a successful issue, where he had adopted the practice of dividing the femoral artery between two points of ligature placed at rather less than an inch apart, it occurred to me that one possible objection to this practice becoming habitual is that a large branch may be given off from the part of the trunk between the ligatures. Of course, the objection does not hold good where deligation is performed on an artery from which no branches other than the terminal arise, as, for instance, the common carotid; but, in other instances, the exact position of the giving off of even large branches is so extremely variable, that it is quite impossible to be sure that such may not arise from the part of the artery between the points of ligature; the profunda femoris, for example, is said by Quain to most frequently arise from the common femoral at an inch to two inches below Poupart's ligament; and yet, in the comparatively small number of subjects which I have myself seen in the Westminster Hospital dissecting-room, this large branch was found, in one instance, arising as high as the ligament, and in another fully three inches below the same structure. In the latter case, it would be quite likely that the large branch would communicate with the part of the trunk between the double

ligature, when the femoral was deligated at the apex of Scarpa's triangle. It is not uncommon to see some little inconvenience occur from a like practice being adopted by some surgeons in operating on varicose veins, where, after tying the vessel in two places quite close together, the division of the intervening portion is accompanied by hæmorrhage, due to some tributary communicating with this part of the trunk. I have myself seen this arise in an operation on a varicose saphena vein. I do not wish to say that this possible contingency should contraindicate the above method of deligation, or that the advantages to be gained do not more than outweigh the objection; but I cannot but think that the collateral circulation through a large branch, communicating with the part of the vessels divided, between two ligatures, would cause, at least, some embarrassment; for, when an artery is neatly tied, such a very small opening is made in its sheath, that it would be quite impossible to tell whether a branch was given off from the posterior aspect of the trunk; and the first indication that such were the case might be at the moment of division of the vessel.

PATHOLOGICAL MEMORANDA.

CONGENITAL MALFORMATION OF BOTH KNEE-JOINTS.

The following case has recently occurred in the Guy's Lying-in Charity. M. B., aged 48, the mother of two children alive and healthy, and one child that died young, was delivered on February 28th, 1883, of a full time male child, which was very fairly developed. On looking at the lower limbs, the first thing that struck one was, that the patellæ on both sides were apparently missing, and in their place was, on each limb, a depression. The description of one limb will suffice for both. The movements of the hip-joint were complete. The thigh, instead of being convex, was slightly concave on its anterior aspect. The tibia and fibula were in their normal relative positions. The foot looked forwards naturally, and the ankle-joint was normal. The patella was placed at the back of the joint instead of in front. In the normal position of the patella was the depression above mentioned, which apparently represented the popliteal space, as the continuation of the femoral artery could be felt pulsating here, and there were some tendons on either side of it, analogous to the hamstring tendons. There was also, running down a little to the inner side of the centre of this space, a strong tendon, which was continued downwards to the head of the tibia, and may have been the representative of the quadriceps extensor tendon. Posteriorly, the patella was rather firmly fixed, nearer the femur than the tibia, in a strong tendon, so that the impression at first was that it (the patella) was attached by osseous tissue to the femur. This idea was, however, dispelled on moving the joint. The movements of the knee-joint were perfect but reversed. The tendon in which the patella was fixed was exactly like the ordinary extensor tendon of the thigh. The artery could not be traced below the head of the tibia. It may, I hope, be seen from the above description, that the leg could be flexed on the thigh anteriorly, as much as it would have been posteriorly in the normal state. When the child was first seen, it had the toes of one foot almost in its mouth, which, of course, it could reach easily without flexing the thigh. The child died on the third day after birth, and, unfortunately, the parents would not consent to a *post mortem* examination.

HUBERT SELLS,

Senior Obstetric Resident, Guy's Hospital.

INTRACARDIAC AND PULMONARY CLOTS.

In the JOURNAL for February 10th, which we have but just received in this colony, there is reported a discussion at a meeting of the Pathological Society of London, on the formation of intracardiac and pulmonary-arterial clots. Dr. Mahomed referred to the scepticism with which most accounts of the formation of clots before death were received; and the ensuing discussion did not apparently result in any further definition of opinion on the subject generally. Intracardiac and arterial concretions are common, on *post mortem* examinations, here as elsewhere; and the examination of them shows that, putting aside true pulmonary embolism, and embolo-thrombosis, slow *ante mortem* production of fibrinous clot takes place in some cases.

In a recent case, the right auricle was occupied, but not occluded, by a firm white, quasi-fibrous mass, elastic and tough. It had no processes into the veins, but was continued into the ventricle by a diminishing prolongation. This embedded the left flap of the tricuspid valve; it had no further connection with the ventricle, but turned

up, still diminishing in size, into the pulmonary artery; gradually becoming red and friable, it appeared to end a little beyond the primary division of the artery. It had no intimate connection with the lining membrane at any point. When it was disengaged from the valve, the latter was normal.

In a case of this kind, apparently no part of the clot is of *post mortem* production, or it would extend into the veins rather than the artery, and would occupy the ventricle as well as the auricle. It also appears unlikely that a *post mortem* clot could so squeeze out all colouring matter as to leave the entire mass of a pearly whiteness; while the striation points to successive coagulations rather than such a formation as occurs when blood is whipped with twigs. There appears to be no room to suppose that a clot, as described, could be due to aggregation behind a precedent embolism; while, the valve being normal, the clot is not produced by accretion on a valvular growth or ulcer. From the condition of the terminal, and probably most recent portion of the clot, it seems likely that, on any increase of the heart's action, fragments might be detached, and form true emboli in the smaller branches; either causing rapid death, with the usually attributed symptoms, or, if life were possible, producing embolo-thrombi, which might extend backward and join the original clot.

A cause for the formation of the clot, in this rather than any other part of the cardio-vascular system, might be found in the aneurysm-like form and condition of an auricle, producing more or less blood-stasis in that cavity, further accretion taking place in the direction of the blood current.

The case described was not seen during life. More recently, in a case of simple diarrhoea, death took place slowly, during twenty-four hours, from gradual failure of the heart's action, there being little disturbance of respiration. Here there was white clot filling and adherent to the whole of the right ventricle, and extending similarly in the artery, but not in the auricle. In a case of acute enteritis, dying a week after onset, the similar clot occupied the right ventricle and artery and left auricle; while, in a like case, there was nothing abnormal about the heart at all.

Berbice, March 12th, 1883.

A. D. WILLIAMS, M.B., etc.

OBSTETRIC MEMORANDA.

TWO CASES OF MISSED LABOUR.

ABOUT two years ago, I had a case of "missed labour." Another similar case having occurred in my practice, I will try to recall the former, and incorporate the information.

The first case occurred in Scotland. The precise dates of the menstruation, etc., I have now forgotten; but exact particulars were given by the patient, from which I reckoned the pregnancy to have lasted three hundred and twenty-three days—in fact, nearly eleven months. The curious circumstance was that, at the natural end of term, the patient supposed herself to be in labour, and sent for me; but, as the pains were only false, I ordered a dose of tincture of opium, and heard no more of the case for nearly two months, when I was sent for, and delivered the patient of a child, apparently healthy, after an easy labour. The child, after some slapping and artificial respiration had been used, both breathed and cried; but, after working an hour with it, I had to desist, as it failed to respond to any stimuli. After death, it rapidly assumed a black hue all over; and, besides this, there was considerable desquamation of the cuticle. The placenta was putrid, but came away quite whole. As far as I can remember, opium-eating was supposed to be the cause of the retarded labour.

The second case, which I attended on March 31st, was not so prolonged, but certainly may be taken under the same category. The patient continued to menstruate after becoming pregnant, and thus could only count from the quickening. On January 27th, 1883, she sent for me, supposing she was in labour. However, as I could barely get my finger into the os, I left her. From that time till the labour was artificially induced, she suffered from vomiting and pains in the head and body, and consequently became very emaciated. She expected to be confined in February. However, as the vomiting still continued, I determined to induce labour, as she seemed to have passed the end of term. Accordingly, on the evening of the 31st, I introduced my hand into the vagina, and passed two fingers into the os, which was patulous, but not at all dilated, no "pains" being present. After a little manipulation, I opened up the os sufficiently to insert my whole hand into the uterus; and, finding the foetus small, and plenty of room, I broke "the waters," seized both legs, and