I sum up in the following propositions.

1. We have no specific for rheumatic fever.
2. Salicin and salicylic acid appear to have had more influence over the disease than any other remedies, and they therefore deserve an extended trial.
3. These drugs lessen the chance of complications in the same proportion as they shorten the disease.
4. The value of statistics of limited numbers in acute rheumatism is small, owing to the uncertainty of the disease.

[Since the above paper was read, a wider experience of the effects of salicin and salicylic acid has been in accordance with the views I have expressed as to the mode of action of these remedies—that it is simply antipyretic. In cases of typhoid fever, I have met with as marked an effect on the temperature as in rheumatic fever, but without evidence of any specific action on the disease. To quote one example: in the case of a boy about twelve years old, 20-grain doses of salicin every hour for six hours reduced the temperature in twelve hours from 104.2 to 97 deg.; and a similar effect was produced on other occasions, yet the duration of the disease was apparently not influenced.—In comparing the effects of these drugs in acute rheumatism and in typhoid fever, it is important to remember that; while in rheumatic fever the characteristic lesions are peculiarly erratic and fugacious, in typhoid fever there is a definite local lesion that must run a more or less protracted course.]

CASE OF CÆSAREAN SECTION POST MORTEM: TRANSPPOSITION OF VISCERA.*

By EDWARD BUCKELL, M.R.C.S. Eng., Winchester.

ON June 14th, at 11.45 A.M., I was hastily summoned to attend upon E. G., a single woman, twenty-seven years of age, who had been taken suddenly ill in the public street, and was removed into an adjoining tradesman's shop. On arriving within ten minutes from the time of receiving the message, I was informed that she had been removed in a carriage to her friend's house, whither I at once proceeded. In the meantime, she had been conveyed into the house and placed on a bed on the first floor. On entering the apartment, I found her lying supine, with limbs extended, face, arms, and hands livid, frothy mucus running from the mouth and nostrils; her eyes staring and fixed, pupils dilated, and with no trace of pulsation or respiratory movement discernible; in a word, to all appearances she was quite dead. The person who carried her from the vehicle to the house along the garden-path believes she actually expired before she reached the house, she having given a deep convulsive sigh when midway and shown no sign or movement afterwards. However, I at once endeavoured, by recognised modes, to excite respiratory movements, applying as well sponges dipped in boiling water to the region of the heart. Having spent ten minutes in fruitless endeavours at resuscitation, and finding that the poor woman had been expecting her accouchement very shortly, I examined *per vaginam*; discovering the os uteri to be dilated to the size of a florin, the head presenting, and the membranes, which were entire, easily felt. The vagina was moist with free mucous discharge, but there had been no hæmorrhage. It was clear, therefore, that, during her morning's walk, her labour had commenced.

On applying the stethoscope to the abdomen, the pulsations of the foetal heart were clearly heard. I conferred, therefore, immediately with the friends on the advisability of performing the Cæsaean section, with the view of saving the life of the child. After some delay, it was agreed to leave the matter in my hands. In the meantime, the foetal circulation had lost much of its power and regularity; still it was readily made out. As nearly as I could reckon, the mother had now been dead twenty or thirty minutes when I commenced the delivery by making an incision with a pocket-bistoury from the umbilicus to the pubes, through the abdominal walls, exposing at once the globular surface of the uterus, which was then cut through. The placenta being found attached to this portion of the uterine wall, I passed my hand through it and seized the body of the child, removing it without trouble at once. This operation occupied about three minutes. The pulsation in the cord was scarcely perceptible, and the heart itself beat very feebly. Without delay, I commenced, at about 12.15, endeavours to excite respiratory movements by means of direct inspiration alternated with Hall's and Sylvester's modes. At the expiry of rather more than an hour—for it was 1.30 P.M. before I left the house—I had managed to induce pretty regular breathing and pulsation in the child. I may remark that considerable advantage was derived from the alter-

nate use of hot and cold sponges applied to the chest of the infant. At this date, the child is still living, and has perfectly free action of both heart and lungs. It has also made satisfactory growth, and bids fair to live.

In the *Transactions of the Obstetrical Society of London*, ten cases of the ordinary Cæsaean section are recorded, with two recoveries of the mother; of six living children removed three survived the month, and may be living now. The child in this present case commenced life some time after her mother had ceased to breathe. Instances are rare in this country of the *post mortem* performance of the Cæsaean section; the circumstances under which it is likely to be successful rarely occur. Many diseases fatal to the mother are so to the child. Death from causes not directly affecting the child's life is generally sudden, and the doctor is rarely summoned in time to attempt the rescue, though he should consider this his first duty after finding that the mother has irretrievably gone.

In Cazeaux's *Midwifery* (8th edition, Paris, 1870, p. 1,070), five instances are given of children removed alive from the uterus from ten to thirty minutes after the death of the mother, and it is there said that "after half an hour success becomes very rare". From other less accurately determined accounts, it would not appear impossible that intrauterine life may be maintained under some circumstances for two hours or more after the death of the mother.

POST MORTEM EXAMINATION.—Eight hours after death, I made a *post mortem* examination of the body. The woman, who had been a domestic servant, was of middle height, fair complexion, fairly proportioned, and well nourished. The mammary glands were fully developed and rather hard. On the right side there was a second smaller nipple about one inch and a half below the normal one, but it did not seem supported by any corresponding glandular structure. On removing the sternum and portions of the ribs in the usual way, the heart was discovered to the right side in place of the left. The liver was transposed, occupying the left hypochondrium, the right lobe becoming the left and the left the right. The stomach was found on the right side, with its natural relation reversed, *i.e.*, the cardiac end being to the right of the pyloric. The spleen was placed on the right side instead of the left. The thoracic aorta was on the right side of the spine, with the œsophagus in front and to the left. The small intestines terminated on the left side, the colon ascending from the left and terminating on the right, the sigmoid flexure turning towards the rectum from right to left. Of course, the ilio-cæcal valve and vermiform appendix were in the left side. The time allowed for the autopsy was so limited as not to admit of more minute and careful investigation; but I forwarded the heart and lungs to Dr. J. C. Ewart, the curator to University College Museum, from whom I have since received the subjoined notes. I will only add that there existed a considerable thinning and dilatation of the convex wall of the aorta, rendering the semilunar valves incompetent, and hence probably the sudden death.

There existed considerable pleuritic adhesions, more particularly on the left side, those on the right being more recent, and not by any means so extensive. During life, the woman is reported as having complained of shortness of breath occurring in paroxysms, often preventing her from lying down in bed, more especially so on first retiring; otherwise she had enjoyed good health. Her mother had, it appears, also died suddenly from hypertrophy of the heart, it having been found enlarged on *post mortem* examination, but not displaced.

Dr. Ewart writes:—"The heart is so transposed that the left ventricle is at the right side and in front, the right ventricle at the left side and behind. The right auricle, larger than usual, is at the left side and in front, its auricular appendage overlapping the root of the aorta. The left auricle is at the right side and behind; the pulmonary artery passes from the left side across and in front of the root of the aorta, and then, lying parallel to the ascending part of the aorta, passes to the lungs. The aorta springs from the posterior part of the left ventricle under and at the left side of the pulmonary artery, and, bending first to the left and then to the right, passes backwards and downwards to disappear behind the root of the right lung. The vena cava inferior enters the posterior part of the right auricle in the usual way; but there are two *superior venæ cavae*, the normal right one entering the auricle at the left side, and the other arching round the right side of the left auricle to pass into the right auricle posteriorly. All the valves of the heart are normal in number and position, with the exception of the aortic semilunar valves, which are rough and very much thickened, and apparently only two in number. The right ventricle seems normal, but the left ventricle is dilated and its walls are considerably hypertrophied. This is perhaps the result of the abnormal condition of the aortic valves. At first it might appear that we have been describing a condition of things similar to that mentioned by Mr. Quain in his work on

* Read before the Southern Branch.

the *Arteries*; but, on careful consideration, it is evident that we have not here an example of what is sometimes found; viz., a persisting right aortic arch, but that we have a simple transposition—a complete turning of the heart and large vessels, so that its posterior surface is anterior, and the left ventricle is carried to the right side. It is interesting to note that, although in this case nearly all the viscera had undergone a corresponding change, the lungs were in their normal position, the large three-lobed lung occupying the right side."

The heart and lungs are now in the museum of University College, London. I ascertained that the woman was not left-handed, and there had been no discovery during life of the existence of such an unfrequent arrangement of viscera.

THE PATHOLOGY OF URÆMIA AND THE SO-CALLED URÆMIC CONVULSIONS.

By F. A. MAHOMED, M.D.,

Pathologist to St. Mary's Hospital; Assistant-Physician to the London Fever Hospital; etc.

I HAVE to thank Dr. MacLagan for his criticism of my theory of uræmia, to which, if I may be allowed, I should like to make some reply. Before referring to his arguments in detail, I may state that I was perfectly aware of Traube's theory of cedema of the brain, nor do I claim any originality for the suggestion of *adema* as a cause for the symptoms of uræmia, as will be seen by a reference to my paper, in which I refer to it as a very well known and debated question among pathologists. I was especially anxious to avoid a discussion on mere theories, and to confine myself to the observation of facts; for that reason I forbore to quote any of the old arguments, or refer to the past writings on this subject, but only mentioned the very strong support which this hypothesis had received from the recent observations of Sir William Gull and Dr. Sutton on the hyaline exudation surrounding capillaries in Bright's disease, observations which I cannot but think go far to demonstrate an actual condition of what might almost be called chronic or fibrinous cedema.

Dr. MacLagan divides his objections into general and special, and I will follow his clear and methodical plan and answer them in detail. First, as to the *general* objections.

1. Dr. MacLagan objects to study the pathology of convulsions in uræmia apart from convulsions generally. I think, on the other hand, that we can only form a true theory of the cause of convulsions in those cases in which they cannot be associated with any discoverable lesion, by studying as closely as we are able the cases in which convulsions occur associated with definite and well ascertained lesions; if we can place by future observations the convulsions of uræmia among these, I believe we shall certainly advance our knowledge of the cause or causes of convulsions generally.

2. Objection is taken to my separation of convulsions from all the other nervous symptoms associated with uræmia. This separation is not mine, but is made by disease. It is not a complete one, however; I take it that the conditions of cedema of the brain and capillary hæmorrhage may occur simultaneously in many cases, and therefore the symptoms produced by each must be coincident. Symptoms of localised disease, such as convulsions and transient paralysis, or even aphasia, I should claim as those arising from capillary hæmorrhages more or less numerous; while the more general symptoms of depressed vitality or nerve-force, which resemble those of compression of the brain, are those which I would leave cedema to account for; supposing it to exist, that it should produce both cerebral anæmia and "malnutrition of the nervous centres" are results which few will deny. To return, however, to Dr. MacLagan's objection, it is impossible to treat different symptoms produced by different lesions as due to one and the same cause. Though hemichorea may precede or follow cerebral hæmorrhage causing hemiplegia, would Dr. MacLagan describe hemiplegia and hemichorea as the same disease, due to the same lesions, and decline to consider them apart? or would he not rather take the association of hemichorea with well ascertained lesions as likely to throw more light upon the pathology of that obscure disease chorea than merely leaving it to purely theoretical generalisations?

The *special* objections offered appear to limit themselves to one; namely, that Dr. MacLagan fails to distinguish between the conditions of capillary hæmorrhages and miliary aneurisms. Again, Dr. MacLagan credits me with ignorance of a very generally known condition; but, in this case, he will find that I have particularly, and more than once, referred in my paper to Charcot and Bouchard's miliary aneurisms, and I take especial trouble to distinguish between the two conditions of capillary hæmorrhage and capillary aneurisms: practically, the great

difference between them appears to be that the hæmorrhages produce convulsions, and the aneurisms do not. Why this should be so is not clear, but it is not very difficult to conceive that blood outside a vessel is a somewhat different condition from blood inside a vessel, and that the pathology of the two should be also different.

Finally, Dr. MacLagan sums up the evidence against my supposition under two heads: first, that I do not accept the theory of "malnutrition of the nervous centres" as a sufficient cause for the convulsions in Bright's disease. To this I would reply that there are many other conditions in which malnutrition of the nervous centres occurs, and to a greater extent, without giving rise to convulsions; and that many, if not all, of those conditions in which they are ascribed to malnutrition are conditions in which the term is used in lack of any other discoverable cause for them. Dr. MacLagan must excuse me, however, if I decline to enter into a discussion of the numerous theories of the pathology of convulsions. Secondly, he says that I substitute for malnutrition "an agency whose existence has not been proved, and whose competence to produce convulsions is at least doubtful." This, in some measure, I admit, but bring what I consider powerful evidence in support of it; and I have only to ask all pathologists to put it to the crucial test of *post mortem* observation, by which alone it can be affirmed or denied.

THERAPEUTIC MEMORANDA.

CARBOLIC ACID FOR TINEA TONSURANS.

THE treatment of tinea tonsurans by the local application of carbolic acid, so ably advocated by Dr. Lee, is of older date than Mr. Rhodes seems to suppose; for it has been used, both alone and in combination with sulphur, and also with sulphur and a salt of mercury, for many years, both by the late Mr. Startin, the late Mr. Naylor, and by me, as have also those closely allied bodies creasote and liquor carbonis detergens; and it is a method of treatment very generally recognised, I believe. The application has been made with almost the frequency which Dr. Lee considers essential, and for the reasons mentioned by him. I am bound to confess, however, that these substances, though of indisputable efficacy in the great majority of cases, nevertheless do not seem to have the power of arresting the complaint in those instances in which there exists, as it were, a congenial soil for the development of the disease in some condition of defective nutrition of the skin, often connected with general disposition to low inflammatory action.

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SURGICAL MEMORANDA.

FATAL CASE OF HÆMOPHILIA IN AN INFANT.

HAVING observed in the JOURNAL of July 14th the record of a case of hæmophilia, occurring in the practice of Mr. Mac Cormac of St. Thomas's Hospital, in a man aged 42, I am induced to give the details of the following case in an infant.

On June 23rd, 1877, A. D., aged seventeen months, received a wound, which divided the ala of the left nostril, by falling on the sharp edge of a zinc bath. He was brought to me by his father as soon as the accident happened. On examination, I found he was bleeding pretty freely from the wound, which was stitched up; cold water and pressure by means of a pad of lint and a bandage were applied, whereby I succeeded in arresting the flow of blood; and after waiting some little time without any sign of its recurrence, the patient was sent home. Early on the following morning, however, I received an urgent message to attend at once, as the child was bleeding as badly as ever, and had been doing so for some time. On arrival, I found the patient saturated with blood. I at once removed the dressing, and applied the liquor ferri perchloridi fortior, which for some hours had the desired effect. The hæmorrhage, however, returned during that and the following days in spite of the various styptics which, each in its turn, was tried without success. Seeing that my patient's life was in imminent danger, as a last resource, and after consultation with my partner, the introduction of a hare-lip pin and the application of a twisted suture was resorted to; but notwithstanding this, a continuous oozing was kept up, and eventually the little patient sank from exhaustion on the 28th, five days after the accident. In this case, the hæmorrhagic diathesis was strikingly well marked, and appeared to run in the family of the mother, as she, on two occasions, nearly lost her life from hæmor-