organisms were isolated. As a precaution the other inmates of the house were inoculated with a *paratyphosus* C vaccine, and, as stated above, no fresh cases of infection arose.

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THE RELATIVE CONCENTRATION RATIOS OF SOME CONSTITUENTS OF THE URINE.

(Abstract.)*

BY

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THE "filtration-reabsorption" theory of renal secretion states that the glomeruli filter off from the plasma its dialyzable constituents, some of which are reabsorbed, together with water, by the tubules, thus leading to a concentration of the remainder: the former are described as "threshold," and the latter as "no-threshold" bodies respectively.¹ If two "no-threshold" substances are present together in the plasma, are filtered off by the glomeruli, and are concentrated in the tubules by the absorption of water, the degrees to which they are concentrated should be the same in each case. The "concentration ratio" is the ratio of the amount present in the urine to that in the plasma (expressed in percentages).² The experiments to be described were performed to determine the relative concentration ratios of some of the so-called "no-threshold" constituents of the urine; urea, inorganic phosphate, and creatinine were chosen for this purpose. They are present in a "free" (that is, dialyzable) condition in the plasma, and appear to be equally distributed between plasma and corpuscles *in vivo*, so that estimations made on the plasma give the amounts available for filtration through the glomeruli at that moment. As, however, it has been stated that urea and inorganic phosphate are present solely in the plasma *in vivo*, entering the corpuscles after the blood is shed, unless precautions are taken to avoid injury to them,³ some preliminary experiments were performed on the distribution of these three substances between plasma and corpuscles in the blood of the cat, since this animal was used for the later experiments. It was found that urea and creatinine were evenly distributed under normal conditions: after injection intravenously, the same even distribution might be found, or there might be more in the plasma, while there was always more inorganic phosphate in the plasma than in the corpuscles after it had been injected into the blood stream.

The experiments on the concentration ratios were performed on decerebrate cats: a solution of the substances about to be examined was injected intravenously, a blood sample taken shortly afterwards, and a further sample after a variable length of time, during which the urine was collected; in some experiments further samples of blood and urine were obtained. The blood samples were oxalated and centrifuged, and the plasma used for the analyses. For calculating the concentration ratio the plasma percentage was taken as the mean of the analyses of the two samples obtained at the beginning and end of the period during which the urine was collected.

It was found that the creatinine was always concentrated to a greater extent than the urea—from two to five times more in the different experiments. This occurs whether the plasma or urinary concentrations vary or remain constant during the course of an experiment. The fact that the results obtained when the plasma concentrations are constant are comparable with those obtained when these concentrations vary, suggests that taking the mean plasma concentration for calculating the concentration ratio introduces no appreciable error.

Comparing the concentration ratios of inorganic phosphate and urea, it was found that the phosphate might be concentrated to about the same extent as the urea, or up

to about three times more; but it was always found to be concentrated to a less degree than the creatinine in the same experiment. The degree of concentration seemed to depend directly on the amount of phosphate injected, being greater when the kidney had to remove a larger amount from the blood stream. If the concentration ratio of urea is taken as unity, those of creatinine and inorganic phosphate relative to this as standard can be plotted on a chart, and it is seen that there is a general parallelism between the ratios of the latter two, which possibly depends on the rate of blood flow through the kidney.

The fact that urea, inorganic phosphate, and creatinine may be concentrated to different degrees by the kidneys during excretion is difficult of explanation on the filtration reabsorption hypothesis. If the modification be accepted that the urea is partly reabsorbed in the tubules,⁴ these experiments show that the hypothesis must be still further modified to admit of partial reabsorption of the inorganic phosphate also. It seems more probable that the tubule cells secrete these substances independently, according to the requirements of the moment. On the other hand, the facts are consistent with the view that selective reabsorption takes place in the tubules, the fluid absorbed varying in composition at different times. A fairly definite relationship was found between the excretion rates of the three substances in experiments in which several samples of urine were obtained: the factor correlating the excretion rates is presumably the blood flow through the kidney.

The experiments give data for calculating the rate of disappearance of the injected substances from the blood stream, taking the initial blood volume to be 5 per cent. of the body weight. Within two minutes of the completion of the injection, 75 to 90 per cent. of the injected urea and creatinine is no longer to be found in the blood; the loss is most rapid at first, and then the rate slackens until a balance is struck between the concentrations in the blood and in the tissues; the concentration in the blood may now remain fairly constant in spite of excretion in the urine, since the tissues act as a reservoir of the substances in question. The injected inorganic phosphate leaves the blood more slowly, and only about 65 to 80 per cent. disappears into the tissues.

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THE DIFFERENTIAL DIAGNOSIS OF SMALL-POX AND CHICKEN-POX.

OBSOLETE DIAGNOSTIC CRITERIA.

BY

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ANYONE who wishes to gain a really sound grounding in this subject will be well advised to assign beforehand their true value to certain traditional criteria with which he may have been equipped as a student. There are certain phenomena which frequently occur indeed, but are not pathognomonic in the hour of trial for the purpose of distinguishing between small-pox and chicken-pox. Into this category fall umbilication, shottiness, palatal and buccal lesions, lesions on the sole of the foot, cropping, uniloculation. Not one of these is a real friend in need; and the first four may prove to be very unreliable in the matter of the assistance which they offer.

In my article in the Jenner Memorial Number of the *BRITISH MEDICAL JOURNAL*, dated July 5th, 1902, on "The differential diagnosis between variola and varicella," the following observations were made: "It is well at once to say that the words 'shotty' and 'umbilicated' are mere broken reeds to lean upon in the matter of diagnosis. The expressions are pernicious in two ways: Their absence in a case suspected to be small-pox is taken as evidence against that disorder, and their presence in a case otherwise suggestive of varicella is held to negative that disease. The words are fetishes, or objects of superstitious worship, which help their devotees as little as do most idols, and

* The full paper is published in the *British Journal of Experimental Pathology*, 1923, vol. 4, pp. 87 and 117.

mislead them far more. They should be treated as such and discarded. In the differential diagnosis of small-pox there are no more fertile sources of error."

Twenty-one years have run by since those words were penned—years constantly spent in consultations upon the numerous remarkable examples of cases simulating small-pox which a metropolitan area and its neighbourhood can show, drawn as they are from a population of about 8,000,000 persons of all grades, classes and races; and this experience has emphasized the general truth of the above mentioned comment. That comment applies in fact to all six of the above-mentioned points. There is this much to be said by way of modification: in proportion as the newer criteria are coming to be known as being more reliable, so less is now heard than formerly of shottiness and the rest.

Once more it must be said that there is no royal road, no beaten track, no measuring tape or touchstone, which will infallibly rescue the perplexed. Any case under review should be cross-examined, so to speak, on four main points: (1) What is the distribution, general and local? (2) What is the degree of prostration? (3) What is the depth of the lesions in the skin? (4) What is the rate and manner of maturation of the lesions?

The whole of the evidence must then be summed up and the verdict given accordingly.

Memoranda :

MEDICAL, SURGICAL, OBSTETRICAL.

DYSTOCIA DUE TO SHORTENING OF UMBILICAL CORD.

THIS interesting complication of labour occurred in a case in Belfast Union Maternity Hospital. The patient, a primipara, was admitted to hospital at full term. The foetus was in first vertex presentation, but the head was very freely movable above the brim. As she was a small woman contracted pelvis was suspected, but the pelvic measurements were found to be normal and no obstruction of any kind could be found, nor did the head seem abnormally large. The only thing noticed out of the ordinary was a very distinct funicular soufflé.

Labour came on next day, and after the cervix was fully dilated the head still remained at the brim. My house-surgeon applied forceps tentatively, and at first could not get the head to advance materially. Suddenly a loud snap took place, which was heard by nurses at the side of the bed, and after that the head advanced easily enough, a little bleeding taking place continuously. On delivery the cord was found to be twisted round the neck of the child, and was completely torn through about three inches from the umbilicus. The child of course was stillborn, but the mother made a normal recovery.

Evidently the foetus was suspended in the liquor amnii by the shortened cord, which prevented the head from engaging or advancing after labour started. The placenta followed the foetus very quickly, and there was no *post-partum* haemorrhage.

It is rather interesting to consider what would be the proper procedure in such circumstances. Obviously a living child could not be delivered as a vertex presentation. I should think to introduce the hand right into the uterus might make the diagnosis clear and decide whether version would give sufficient length of cord. This would have to be long enough to reach from the placenta to the vulva or thereabouts in a breech. In the case I have described I do not think version would have given sufficient length of cord. Brickner, who has carefully studied the subject, states that delivery cannot occur in such circumstances unless one of the following accidents occurs: separation of the placenta, inversion of the uterus, umbilical hernia of the foetus, or rupture of the cord—the last two being of infrequent occurrence. I should think it might be possible to free one of the loops with the hand in the uterus, or Caesarean section would give the greatest certainty of a living child if one were sure of the condition.

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Reviews.

THE SURGICAL DYSPEPSIAS.

OF the making of books on abdominal surgery there appears to be no end. A short while ago Mr. Sherren published his lectures to students at the London Hospital on the surgery of the stomach and duodenum, and a very excellent little book it was. Now Mr. WALTON, from the same hospital, has brought out a large book on the *Surgical Dyspepsias*.¹ And this, too, is a good book, and well planned. Included in it are not only diseases of the stomach and the duodenum, but also those of the biliary passages, liver, and pancreas, with an account of visceroptosis to round it all off. The title is a challenging one, challenging criticism as well as interest. For not all of these conditions are truly or essentially dyspepsias, nor are they all essentially surgical. But the title has one thing in its favour: it suggests that the author has considered the clinical side of his subject, and has written something more than a book on operative surgery. As a fact, Mr. Walton has been at very great pains to furnish as clear a picture as he can of the various affections of the upper abdomen as met with in practice. And he has skilfully used histories of actual cases to show, as it were, what these conditions look like in their natural state.

Mr. Walton has his own views as to the meanings of symptoms. He emphasizes the importance of the clinical history and the relative lack of importance of the physical examination in the diagnosis of ulcers of the stomach and duodenum. This is an important point, though not, of course, an original observation. It is the most probable explanation of the omissions in diagnosis made by the so-called (and often actually) busy practitioner, who has no time to listen to his patients' often meandering account of their ailments. There is no doubt whatever that in order to be a good and trustworthy diagnostician of abdominal diseases, the practitioner must be a patient listener, ready to seize on important facts which are usually not realized as important by the patient and as frequently clouded over with unimportant detail. The days of the old aphorism, "Eyes first and much, hands next and little, tongue not at all," are as dead as Queen Anne. Major surgery to-day deals less with lumps and tumours obvious to the eye than ever. Nor, we think, should too much reliance be placed on our new eye, the x rays. The corroborative value of the opaque meal is great, but it is a gross error to base one's diagnosis, positive or negative, preponderatingly upon it.

Mr. Walton describes well the symptomatology of acute and chronic ulceration of the stomach and duodenum. It is interesting to see the author attacking again the question of acute ulcers, which have fallen into surgical disrepute. Indeed, in one sense Mr. Walton drives another nail into their coffin by stating it as his belief that such ulcers rarely perforate, and that the acute perforations are usually of chronic ulcers. He produces some evidence which gives this view a look of truth, but we cannot quite accept it, for the duodenum at least. He states that all except one of his cases of perforated duodenal ulcer gave a history of previous dyspepsia. His experience in this matter must differ from that of other surgeons. He is no believer in Hale-White's gastrostaxis, and many will agree. He is a believer in the frequent derivation of malignant ulcers from chronic. This is still rather a faith than a demonstrable fact; there is much parallel fact and little direct demonstration. W. C. McCarty, who is as it were the high-priest of this creed, is an unsatisfactory preacher. A close study of his writings leaves the reader mystified as to his real conclusions. Mr. Walton describes a symptomatology which to his mind denotes the malignant degeneration of a previously chronic and benign gastric ulcer. This must at present be accepted with reserve.

The operations on the stomach are very well described and illustrated. The sketches of the various manoeuvres associated with the great names of gastric surgery are very clear and valuable. The author naturally has a predilection for the operation which he has helped to popularize—wedge-

¹ *A Textbook of Surgical Dyspepsias*. By A. J. Walton, M.S., M.B., B.Sc.Lond., F.R.C.S.Eng. London: Edward Arnold and Co. 1923. (Med. 8vo, pp. 728; 272 figures, 2 coloured plates. 42s. net.)