at an increasingly early period as the patient goes downhill, and the kidney fails.

4. Lastly, and lest my criticisms should be all destructive and not helpful, I would point out that the possibility of a (normal?) full-time pregnancy with living child, in cases of *known* extensive renal disease, has been often observed, showing, perhaps, that the renal disease is not *necessarily* the whole factor in these cases.

I have ventured to encroach on your space so far, as I have just finished collating and analysing a series of over a hundred cases, not yet published, some of which have been watched during a long period, some over thirty years, and part of the results do not appear to support Dr. Young's deductions. I am sure all workers in this difficult field will welcome his paper, and that it will be helpful in many ways, one of which will be the criticisms that it may evoke.—I am, etc.,

Norwich, Feb. 5th. ARTHUR CROOK.

ASTHMA.

SIR,—On first perusing the letter of the Asthma Research Council their statements appeared to be merely platitudes; but on reading the assertions made by some of your correspondents, I am inclined to agree that these apparent axioms are in still further need of emphasis.

I have every reason to know that the claims about curing asthma made by certain chemical pathologists are disappointing. There is a manifest fallacy in the theory that foreign proteins which give a reaction when injected hypodermically, or scratched into the skin, will therefore cause disturbances when swallowed, even if they do so sometimes. I venture to suggest that if some of those who are in the habit of eating beef or mutton were to inject themselves hypodermically with 10 c.cm. of beef or mutton serum, they would have some reason to remember the occasion.

Though I practise the nasal method of treatment myself, I am the first to realize that exaggerated claims have been made for it, based upon statistics containing large numbers of untraced cases. Mr. Herbert Tilley has pointed out in his textbook how often these relapse months or years after an apparent cure.

My own experience is that only those patients who have no gross lesions, and who give a positive tear-reflex test (*Journal*, December 1st, 1928, p. 985) are likely to do really well by nasal treatment. Such patients should, in my opinion, be treated in a very conservative way, efforts being devoted to rendering the hypersensitive mucous membrane insensitive by means of the galvano-cautery or other caustics. This is my experience after reviewing a series of cases from four to six years after treatment. The only operation which has not disappointed me has been removal of tonsils.

But whatever the experience of individuals may be, there can be little doubt that some co-ordination of knowledge and sifting of evidence is essential.—I am, etc.,

London, W.1, Feb. 9th. H. MORTIMER WHARRY.

THE DICK TEST IN SCARLET FEVER.

SIR,—In reply to Dr. Balmain, who asked, on January 26th (p. 175), what was the size of the Dick reaction which I considered positive, I never regarded anything less than 1.5 centimetres as positive. The nurse to whom he referred was found to be Dick-negative at two examinations, one being two months before and the other three weeks before her attack of scarlet fever.

Until I had read the article by Dr. Burton and Dr. Balmain in the *Lancet* of May 26th, 1928, I was surprised that Dr. Balmain disagreed with me when I asserted in my article that it was impossible, in the present state of our knowledge, to say that a positive Dick test in a patient who had had scarlet fever indicated a failure to develop immunity to this infection. In that article Drs. Balmain and Burton stated as follows: "We would suggest, however, that the Dick-positive reactors in convalescent scarlet fever cases are persons who have failed to develop immunity." I would point out to Dr. Balmain that among the high proportion of Dick-positive reactors in convalescent scarlet fever patients in my series not one developed scarlet fever, and they were all treated in an acute ward. The late Dr. Zingher, in the admirable article referred to by Dr. Balmain and myself, said "Positive Dick reactors are susceptible to scarlet fever as far as not having antitoxin is concerned," and again, "Positive Dick reactors, however, need not necessarily develop scarlet fever when exposed to the disease." I was very pleased to notice that Drs. Balmain and Burton referred to their eighteen cases as true relapses, because it has always seemed to me to be of the greatest importance in investigating any case of a relapse or a second attack of scarlet fever to make certain that the first attack had been genuine.—I am, etc.,

Doune, Feb. 4th. WILLIAM BROWN, M.D.

"DEBILITY" IN CHILDREN AND CYCLICAL VOMITING.

SIR,—Dr. Osman writes an important and interesting article in your issue of January 26th (p. 150) on debility and cyclical vomiting. Dealing with the latter subject only, I am unable to accept unreservedly his sugar deficiency theory, as it does not seem to cover all the facts.

It is not uncommon to see a child who has exhibited all the symptoms of cyclical vomiting quite cured after adequate treatment with santonin for round-worm.

Dr. Osman draws attention to the similarity between cyclical vomiting and recurring appendicitis. Differing from him, I consider that frequently the latter is the starting point of all the symptoms. Time and time again I have seen the attacks finally stopped by removal of an offending appendix; and it does not convince me when I am told that extra sugar in the diet might have produced an equally favourable result.

Having seen, as a practitioner, many cases of cyclical vomiting, the impression I have formed is that it is not an absolute clinical entity, but a symptom group resulting sometimes from one cause and sometimes from another. If Dr. Osman's theory turns out to be even partly correct, we must thank him for providing us with a potent weapon against the malady.

In the excellent description of signs and symptoms given in the article two rather constant signs are missing—namely, a dusky malar flush and the so-called acetone odour in the breath.—I am, etc.,

Lerwick, Feb. 4th. JAMES CAMPBELL, M.B., Ch.B.

SIR,—It would be almost impossible to reply in detail to every one of the many interesting points raised by Mr. R. Chalmers and Dr. J. Thomson Shirlaw on February 9th (p. 269) in their friendly criticisms of my article on the above subject. To do so would occupy a great deal of space, and most of the points have been covered in a further paper on the subject which will appear in due course.

I would like to make it clear, however, that the statements in my article were not based simply on the finding of acetone bodies in the urine of these children, but on a very comprehensive clinical and biochemical study of a large group of cases, carried out over a considerable period. The observations made included estimations of the blood bicarbonate, chloride, inorganic phosphates, calcium, sugar, and total acetone bodies, together with careful chemical investigations of the urines of these cases at the same time. In a number of instances also bilirubin estimations on the blood, fractional test meals, alimentary x-ray examinations, and glucose and levulose tolerance tests were also employed. Further, corresponding observations have been made on a series of "debilitated" adults; the results of these observations will be published in the paper referred to.

I should like to take this opportunity of discussing in a little more detail the position with regard to "acidosis" in relation to "debility." For this purpose the term "acidosis" may be defined as a reduction of the plasma bicarbonate. Such reduction may, or may not, be due to, or associated with, an excess of ketone bodies in the blood; that is to say, a condition of acidosis may be present, and frequently is, without acetonaemia, etc. As a matter of

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