Duodenal Ulcer and Priority Foods

SIR,—I was interested in Dr. A. H. Morley's letter on duodenal ulcer and priority foods (Nov. 8, p. 745) and would say a word on the other side of the picture. It is agreed, of course, that many duodenal ulcers are cured, temporarily or otherwise, or lie dormant for long periods, but surely it is also agreed that the only rational way to treat an ulcer subject is to keep him well, and that a bland diet, especially with regard to the protein intake, is a very important factor, if not an essential one, to that end. He is advised—and rightly, I believe—to avoid tough meat, stews, cheese, sausage, salty and spiced foods, yet the bulk of his rationed food will come fairly under these categories. He does better on white fish, but that is sometimes short, often of poor quality, and can be extremely monotonous. I am informed that an extended course of this can destroy the appetite and undermine the character, that an addict is liable periodically to break out into homicide, divorce, or a debauch of liver and onion, curried beef, kippers, or other flavoursome but deadily comestibles.

No doubt there are some who may not require these priority foods, but I am of opinion that any general curtailment of these would result in an increase, and probably a large one, in the incidence of ulcer breakdown. It is well to remember that such a breakdown may be regarded in these days as a small national disaster. Ambulatory treatment is seldom satisfactory, and the subject, to have any real prospect of cure, must lie up for weeks or months in the care of others at home or in our hard-pressed hospitals.

I feel exception should be taken to Dr. Morley's statement that duodenal ulcer "has, indeed, become a somewhat popular malady—and no wonder!" If this means anything, it implies an unfortunate innuendo. Surely the patient does not elect to have an ulcer—the diagnosis is with the physician. Any claim by an unknown patient can be readily checked as a rule. I have not found that ulcer patients consider themselves fortunate. Dr. Morley's estimation that this priority milk would float a battleship may be accurate, but surely this alternative use need not be considered at the present time. Our old friend Chad has just peeped over my pile of milk certificates with a startled comment on our naval establishment.—I am, etc.,

Sutton, Surrey.

J. CRAWFORD.

SIR,—It must surely cause some astonishment that there are medical men who hold the view expressed by Dr. R. Stuart (Nov. 22, p. 842) that "the symptoms of chronic duodenal ulcer are chronic, recurring at regular intervals every day and every night throughout many years." Can he have in mind some atypical condition such as an ulcer extending to and constricting the pylorus? It is, after all, the alternating bouts and the much longer intervals of freedom from symptoms which constitute so invaluable a diagnostic feature of the disease. In this connexion I would quote Sir Robert Hutchison, when he says in his *Lectures on Dyspepsia* (1927, London): "The History is in many cases perfectly clear and sufficient alone to base a diagnosis upon. It is as follows: The patient will tell you that, often for years back, he has been subject to attacks of indigestion which have been intermittent, that is they have cleared up and he has been well between them." And again, to quote Sir James Walton in his *Surgical Dyspepsias* (1930, London): "When once the disease is well established one of its most characteristic features is the periodicity of the symptoms. This is more marked than is the case with gastric ulcers, and indeed than in any other disease. There will be attacks of acute symptoms persisting for some three or four weeks, which are followed by periods of complete freedom. These periods will nearly always last for some months and may persist for nearly a year."

In regard to the other point in Dr. Stuart's letter, although it may be conceded at once that regulation of the patient's habits—frequent small regular meals, avoidance of irritants and tobacco, worry and fatigue, and so forth—is of great importance in prolonging the periods of freedom from symptoms, a rigid regime of diet is considered by many authorities to be unnecessary. Dr. A. H. Douthwaite (July 12, p. 43) expressed this view.

This brings me to my original contention, to which I adhere, that to lavish 10 pints (5.7 l.) of milk a week (five times the

allowance for the ordinary consumer at present) and priority in eggs on the chronic duodenal ulcers is indeed squandermania—an expressive word, for which I thank Dr. Stuart. It fits admirably.—I am, etc.,

London, N.W.7.

A. H. MORLEY.

Vitamin-D Requirements in Pregnancy

SIR,—Dr. L. J. Harris is to be congratulated on his admirable survey (Nov. 1, p. 681) of the present state of our knowledge as to vitamins. I venture, however, to question the accuracy of one of his statements: "For nursing and expectant mothers 1,000–2,000 i.u. [vitamin D] daily may be prescribed. . . ." I made a search of the literature on this subject about a year ago; I found only vague and contradictory statements. The following two quotations from recent authoritative textbooks are typical: McCune¹ states that during pregnancy "an amount of vitamin D equal to that contained in 5 teaspoonfuls of cod-liver oil—that is, 1,700 i.u.—may be required in addition to a high calcium intake to prevent a negative balance and guarantee calcium retention." Shohl² writes: "400 units of vitamin D and 1 quart of milk daily should be included in the diet of pregnant women." No experimental evidence is brought forward in support of either of these statements.

I have recently completed a four-year survey of normal pregnancy in the out-patient department of the City of London Maternity Hospital. Some of the biochemical findings have been published.³ They cover 226 48-hour calcium and phosphorus balances at various stages of pregnancy. They would seem to show that doses of vitamin D smaller than 10,000 i.u. per day have no influence whatever on calcium and phosphorus metabolism and that doses considerably larger than this (up to 36,000 units per day) exert a definite influence only if the calcium intake is above 1.5 g. per day in the early months and later 2 g.

This subject is a difficult and complex one. My findings, to be conclusive, must be confirmed on a larger number of cases. But this is not a matter of purely academic interest. It concerns us all—vitality. Until further experimental evidence, adequate and incontrovertible, is made available, I submit that we should play for safety. In a climate like that of England every pregnant woman should be given a supplement of vitamin D in doses of not less than 10,000 i.u. per day in the first 7 months, and 20,000 i.u. during the 8th and 9th months.—I am, etc.,

Como, Italy.

E. OBERMER.

REFERENCES

- 1 McCune, D. J., in *Dietotherapy* (Editor, Wohl, G. W.), Philadelphia, 1945, p. 317.
- 2 Shohl, A. T., *Mineral Metabolism*, New York 1939, p. 344.
- 3 Obermer, E., *J. Obstet. Gynaec. Brit. Emp.*, 1946, **53**, 269, 362; *ibid.*, 1947, **54**, 432; *ibid.*, 1947, **54**, in press.

H.T.S.T. Pasteurization

SIR,—As far as we are aware no report has been made of the efficiency of the H.T.S.T. method of pasteurizing milk on a commercial scale. The results of tests carried out by Dr. Porteous of St. Mary's Hospital on samples taken from three of our plants using this process therefore may be of interest outside our own organization.

One hundred and twenty-nine samples of the raw milk used gave a positive guinea-pig test for tubercle bacilli. The 129 samples of commercially pasteurized milk corresponding to the above raw were all negative in the test. All these pasteurized milk samples also passed the official phosphatase test.

The samples covered a period of two years from January, 1945, to January, 1947.—I am, etc.,

E. B. ANDERSON,
Chief Chemist,
United Dairies, Ltd.

* * This matter is discussed in a leading article at p. 914.—
Ed., *B.M.J.*

Thiouracil

SIR,—It is with great interest that I have read the review by Dr. H. Cookson and Dr. F. H. Staines (Nov. 15, p. 759) on their experience with thiouracil. With increasing experience in thiouracil therapy it is evident that the risk of immediate toxic effects is diminishing. I note, however, that the length of treatment is given as "a year or two and may be even more." That this chronic therapy with a drug which poisons the thyroid is not altogether without risk of far more serious consequences is shown by the work of Purves and Griesbach,¹ who produced

thyroid adenocarcinomata in almost 50% of rats to whom they administered thiourea for more than two years.

The histological picture of the human thyroid after thiouracil therapy shows marked cellular activity, and before its general use is recommended further research should be carried out on the possibility of delayed after-effects. The experienced surgeon who resects a toxic goitre knows that 99% of his patients should survive the operation, that about 90% will lead approximately normal lives without special supervision, and that, in the event of an error in diagnosis, malignancy will be revealed and can be treated without further delay. The thiouracil drugs are most valuable in preparing a patient for operative treatment, but caution is advised before suggesting that they should replace it.—I am, etc.,

London, N.2.

HENRI ROUALLE.

REFERENCE

¹ Purves, H. D., and Griesbach, W. E., *Brit. J. exp. Path.*, 1947, **28**, 46.

Measles Prophylactic

SIR,—Following your leading article (Nov. 8, p. 737) on the use of gamma globulin as a measles prophylactic it would seem worth while to stress once again the value of a dose of adult whole blood intramuscularly if given at any time up to the sixth or seventh day of incubation. Gamma globulin is expensive to produce and uses the time of highly skilled technicians. Blood, 4 to 10 ml. according to the child's age, taken from the mother and injected immediately, is cheap and speedy therapy. The marked difference in the severity of symptoms in the unprotected case and the partially protected contact is so constant that the method can be advocated as a routine measure when the first of a family of children is found to have measles or other contacts are known.—I am, etc.,

Stanmore, Middlesex.

HARWOOD STEVENSON.

Dicoumarol

SIR,—I read with interest your annotation (Oct. 25, p. 662) on dicoumarol and subsequent letters. I feel that Dr. Frank Marsh (Nov. 8, p. 748) is on the right lines when he suggests a large dose for the first two days, then an interval, and subsequent smaller doses.

My experience with Quick's method of prothrombin estimation has been singularly unfortunate. The results I obtained were far too varied to permit of any serious consideration. We used the clotting time and attempted to double the original figure. This was done throughout by Lee and White's method. In a number of cases alarming features resulted before the clotting time had doubled. Obviously the drug has a cumulative effect, and the clotting time does not begin to rise appreciably until the patient has been on it for some time, and then it rises with marked rapidity.

I would suggest that the drug be given in maximal doses—i.e., 300 mg. the first day, 200 mg. the second day, then 150 mg. daily until the clotting time begins to rise, and at that stage withheld. When the clotting time has returned to its original value, dicoumarol should be given again. We used fresh-blood transfusion to lower the clotting time and found that it works fairly rapidly. There is at least a 48-hours latent period before intravenous vitamin K begins to show its effect.—I am, etc.,

Birmingham.

M. J. PIVAWER.

Yeast Extracts and Fat Absorption in Sprue

SIR,—For a detailed discussion of the points raised by Dr. K. D. Keele (Nov. 15, p. 794) we would refer those interested to our paper in the current issue of the *Quart. J. Med.* (1947, **16**, 99). In that paper the evidence for the effectiveness of yeast extract is given, and due acknowledgment for statistical help is made to Mr. Dilwali (not Mr. Dilwater). As Dr. Keele rightly points out, we were not in clinical charge of the patients in Poona, and so we have relied mainly on biochemical evidence for the effect of yeast extract; nor were we in a position to "settle the issue" at that time by the use of yeast extract alone. However, we have since treated patients with sprue in this country and have been able to maintain them in clinical remission by yeast extract without any liver therapy.—We are, etc.,

Manchester.

D. A. K. BLACK.
L. P. R. FOURMAN.

Status Epilepticus after E.C.T.

SIR,—Dr. A. Folkson's memorandum on a case of status epilepticus precipitated by E.C.T. (Aug. 30, p. 335) prompts me to record a similar case.

In September, 1944, an African woman of the Yao tribe (who live on the borders of Tanganyika and Portuguese East Africa) was admitted to this hospital. She was about 45 years of age. The certificate stated that she wandered aimlessly among her neighbours, that she slept in the bush, and that she thought people were going to kill her. After admission she was either indifferent and pre-occupied or nonsensically loquacious. She had corneal opacities and septic teeth. The spleen was enlarged, but there were no malarial parasites in the blood. There were no neurological signs or history of epilepsy. The blood Kahn was negative and the C.S.F. W.R. was negative, with no increase of cells or protein. Attention to her general health produced a partial improvement. On Feb. 20, 1945, she was tried with E.C.T. at 10 a.m. (105 volts, 0.4 sec., 160 milliamp.-sec.). There was a delay of 3 or 4 sec. The next day at 3 p.m. she suddenly passed into typical status epilepticus. At 3.45 p.m. she was given 1/2 oz. (14 ml.) paraldehyde in olive oil per rectum. The fits began to lessen in a few minutes and had ceased after 1/4 hour. There has been no recurrence to date.

—I am, etc.,

Dodoma, E. Africa.

E. J. FOLEY.

Routine Serological Tests for Syphilis

SIR,—At a recent conference of doctors in America surprise was expressed that in this country serological tests for syphilis were not routinely carried out by law on such occasions as marriage, at life insurance examination, or on entry into the Services as is current practice in the States. It was furthermore stated that representatives of this country had indicated that such a proposal would lead to public outcry and would never be permitted, and in any case such a course was unnecessary, because the incidence of venereal disease is so much lower in this country than in the States.

Surely in these enlightened days such a position is indefensible? If we have no serological tests, what reason have we for saying that our incidence is low? Clinical manifestations of the disease are no guide, because they may be dormant for many years and thus be undetected. Is it national prudery, smug hypocrisy, or a fear of exposition which leads us to adopt this rather dubious delicacy on such a matter? I am not a syphilologist, but doubtless there are many with greater knowledge in the matter who can give the answer to this apparent diagnostic obscurantism, and whose views I should like to hear.—I am, etc.,

Bristol.

K. G. BERGIN.

Penile Carcinoma

SIR,—The incidence of cancer of the cervix in Jewish women is relatively low, and this has frequently been ascribed to the observance of laws which combine common-sense hygiene with religion. Mr. W. Sampson Handley (Nov. 22, p. 841) brings forward evidence which suggests that it is the routine circumcision in the male which is the operative factor. If the "transference to the vagina of the mixed bacterial flora that flourish beneath the prepuce" ultimately, by means of chronic infection, leads to cancer of the cervix, then there must be many more cases in which, even without cancerous changes, it results in discomfort and chronic invalidism. Having built up a strong case and drawn the correct inference, your correspondent advises routine preputiotomy in male infants because "it is unlikely that the routine circumcision of male infants would commend itself to English opinion."

What are the objections to routine circumcision and how does preputiotomy meet them? Does the main obstacle to the former lie in the lack of knowledge of a simple operation or in the psychological objection to imitating the Jews? It seems to me that your correspondent's compromise is a bad one. It is true that circumcision, as generally taught, involves general anaesthesia, pressure forceps, scissors, and sutures, while slitting up the prepuce "in the first few days of life can be done without an anaesthetic." Although reference is made to "an experiment conducted on a national scale for two thousand years" (the estimate is a conservative one), there is no mention of the technique which has made it possible. In fact few, if any,