Correspondence

Thiouracil in Toxic Goitre

SIR,—While congratulating Dr. Harold Cookson and Dr. F. H. Staines on their meticulous and unbiased record (Nov. 15, p. 759) of their four years' experience of the use of thiouracil in toxic goitre, we wonder whether they have really provided evidence for their claim that "there is no doubt that the antithyroid drugs now available have established themselves as a most valuable addition to the methods of treatment of toxic goitre, and in certain types as the method of choice." It would, indeed, appear to us that their experience constitutes the most devastating indictment of the use of these drugs in treatment that we have yet read. We do not for a moment question their findings, for they are in line with those of many other observers both in America and in Great Britain, but we do question their conclusions.

The facts appear to be as follows: (1) 95 patients were treated. (2) In 6 no control was obtained. (3) Remission of the disease lasting from 2 to 25 months was obtained in only 40 patients; 15 of these had relapsed. (4) 14 patients suffered from toxic effects of the drug with its abandonment on this account in 2 cases and with one death from agranulocytosis. (5) *No fewer than 10 patients died while under treatment.* Items (3) and (4) in this list contain sufficiently formidable implications of wasted time and effort on the part of both doctors and patients. But it is a mortality, while under treatment, of over 10% that constitutes the most damning evidence of all.

Although 8 of their patients had to be submitted to surgery without a fatality, the writers of the article do not permit themselves any general comparison between their results and those of surgery. They can scarcely be unaware, however, since it has been stated on many occasions, that subtotal thyroidectomy, given good surgery and good management both pre- and post-operative, can be performed on hundreds of consecutive patients with a mortality of less than 1%. These series include patients of all ages and with every degree of severity of the disease (none are rejected), and it is recognized with gratitude that thiouracil is of the greatest service in the preparation of severely toxic patients for surgery.

Dr. Cookson and Dr. Staines have treated 16 patients who had reached the stage of auricular fibrillation. In only half of these was a normal rhythm restored with thiouracil, and 4 died while under treatment. Surgery, on the other hand, claims many of its most brilliant results from among this very seriously ill group of patients, and with the occasional help of quinidine less than 20% fail to obtain a persistently normal rhythm.

In view of these well-established claims we do not understand how the writers of this article can conclude that thiouracil can be the best method of treatment in any type of the disease. It may be argued that the best surgery is not available for more than a fraction of the sufferers from thyrotoxicosis. If this be so, our answer is that opportunity is available for any surgeon to learn the technique and management of the operation. There is no excuse at this date for the plea of ignorance, and it would be bad surgery indeed that resulted in the loss of over 10% of its patients.—We are, etc.,

GEOFFREY KEYNES.
J. W. LINNELL.

London, W.1.

Diet and the Nation's Health

SIR,—No cautious man would willingly descend into the dusty arena where the gladiators contend about the state of the nation's nutrition, and I formally disavow any expression of opinion on the subject here. I am moved to commentary, however, by the account in your pages (Nov. 29, p. 882) of a recent debate of the Hunterian Society. Sir Jack Drummond, it would seem, expressed the view that there is "no objective evidence showing with clarity and precision" that we are less well nourished than in 1938, and implied that he is not prepared to accept "merely subjective impressions" in a contrary sense.

His antithesis, suited perhaps to the occasion, is resounding rather than substantial, yet he may be right in the first part of his statement. But surely to assume that in so saying he has

disposed of the problem under discussion is to beg the question and to display a misunderstanding of the nature of the problems facing the clinician, the obligations laid upon him, and the means by which he has to deal with both. "Subjective" impressions, and with all respect to Sir Jack Drummond I confess that I know of no other kind, are surely the beginning, if not the end, of that observational approach from which all scientific inquiry must start—an approach, too, that must remain the clinician's principal one.

Every clinician yearns for nature to provide him with "objective data showing with clarity and precision" what is wrong with his patient, and desires whenever possible to measure, verify, and control his observations and to confirm his preliminary impressions. But he knows that too often this is not the order in which things happen in the practice of medicine, that he is constantly being forced to pay careful attention to his subjective impressions and to lay and initiate plans of action in the interests of his patients long before clarity and precision of data are vouchsafed him. If he is not willing to go to work in this way, then he is incompetent to discharge his obligations and were better employed conducting nutritional experiments on rats in a laboratory. There, at any rate, he could afford to wait for clarity and precision, to let time do his thinking for him, and to see his animals visibly pining before he was under the painful necessity of making up his mind. How wonderful it would be did such easy options govern the labours of the physician, who would then rarely be compelled to call upon intuition or to use judgment but could sit back waiting for complete data to drop into his lap.

I suggest, therefore, that where the health of human beings is in question scornfully to reject subjective impressions as part of the material of judgment, and to decline to celebrate in the absence of data of complete clarity and precision, would be as unscientific and negligent as to suppose that subjective impressions are of themselves wholly adequate as evidence and absolve us from the obligation of all further inquiry. In the present instance, perhaps, it is of little importance what the physician thinks or says, since he has no control over what the nation gets to eat; but that one who has been a scientific adviser to the Ministry of Food should take the narrow view of evidence reflected in Sir Jack Drummond's remarks is more serious. He may be right in thinking that the nation is not undernourished, but his criteria of assessment are inadequate to a situation which he appears not to understand.

The biochemist, indeed, has proved notably insensitive to intimations reaching him from medicine, and our bones might easily rattle under our hides before his convention of scientific purism permitted him to see that we were growing empty. If one were at all cynical, one might be tempted to hope that this nation may never be so very hungry that the biochemist comes to notice the fact.—I am, etc.,

London, W.1.

F. M. R. WALSH.

Acute Non-specific Gastro-enteritis

SIR,—In view of the increasing attention directed to outbreaks of gastro-enteritis of unknown aetiology, a description of an outbreak in this mental hospital last summer may be of interest. The first case began on June 9, the outbreak proper on July 17, and thereafter fresh cases appeared for 30 days, involving 65 out of 309 patients.

The outbreak began in an infirmary ward, where, contrary to the experience of Cook and Marmion,¹ it involved 41% of the patients, principally the old and feeble and those debilitated by other diseases. Eight days later cases appeared in two other wards, the day after that in a fourth, the whole of the female block now being affected. Cases of gastro-enteritis were present in the district, but in the hospital were limited entirely to the female wards. Two female part-time nurses developed it; one had had no direct contact with the affected wards, the other had been nursing there. Some members of the male staff living at home contracted it, but did not infect any of their patients. As in other outbreaks, food as a source could be excluded, as both male and female patients ate from a common kitchen. There was no association with Sonne dysentery,² upper respiratory infections³ or chilling.⁴

Clinically the onset was abrupt, with diarrhoea, vomiting, and gripping abdominal pain; it was apyrexial except in 3 cases, where the temperature rose 1°–2° F. (0.6°–1.1° C.) to subside in 24 hours.