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.,

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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.,

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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.

The vomiting ceased within 24 or at the most 48 hours, while the diarrhoea, which was of frequent, offensive, watery stools without blood or mucus, lasted two to three days, exceptionally four or more. In a few the onset was severe with marked prostration. One-fifth of the cases had no vomiting. Each case had a minimum of 3 stool cultures, 201 specimens in all being examined. No organism of the enteric, dysentery, or salmonella groups was found except in one case. This patient's illness lasted longer than usual—namely, 7 days. On the first, fourth, and eighteenth days from the onset her stool was negative; on the twenty-fourth day *Salm. typhimurium* was isolated. She was the only patient to have a further attack (not shown in these figures), which occurred two months after

the first, when her stools were again negative. Widal tests from this patient and 6 others on Sept. 17 were negative. The following organisms were also identified, the numbers in brackets referring to cases: Morgan's bacillus 1 (4), Morgan's bacillus 2 (1), paracolon-bacillus type (4), *B. faecalis alkaligenes* (1).

All cases were immediately isolated, discovery frequently depending entirely on the nurses' observation. In all but the infirmary ward strict isolation of individual patients could be enforced. Treatment was on the usual lines, with initial starvation and gradual return to normal of the diet by the fourth or fifth day. Attempts to increase the diet more quickly led to recurrence of the diarrhoea. Some cases were given phthalylsulphathiazole 3 g. daily without marked effect on symptoms or duration. However, sulphaguanidine and phthalylsulphathiazole were tried as prophylactics, as shown in the table and diagram. The results were encouraging.

Ward	Population	Type	No. of Cases	Duration	Prophylaxis	% Affected
1	90	Infirmary; aged and sick; overcrowded	37	July 17–Aug. 5	Phthalylsulphathiazole, 1 g. b.i.d. for 2 days	41
2	88	Refactory; overcrowded	15	July 25–Aug. 8	Phthalylsulphathiazole, 1 g. t.d.s. for 3 days	17
3	98	Overcrowded; patients out to work	10	July 24–Aug. 4	Sulphaguanidine, 2 g. t.d.s. for 2 days	10.2
4	33	Parole; not overcrowded	2	July 24–Aug. 16	None	6

When sulphaguanidine was given to all patients in Ward 3 the incidence of fresh cases stopped abruptly; one case appeared on the second day of prophylaxis, but her symptoms were mild and lasted only 24 hours. In Ward 1 phthalylsulphathiazole could be given to only a proportion of patients owing to low stocks. Two cases occurred on the second day of treatment and one the day following, all in patients who had been given the drug. Probably the dosage was too small. Fresh cases continued to occur in Ward 2 until phthalylsulphathiazole in a somewhat higher dosage was given to all patients.

An interesting point is that in Ward 4, where no measures beyond isolation of the patients affected were taken, only two developed gastro-enteritis. Here there was no overcrowding and the patients were out most of the day. In Wards 1 and 2 there was close all-day contact between patients, and in Ward 3 close contact all night and part of the day.

The course of the outbreak suggests that the initial cases were caused by one or more carriers from outside and that infection

thereafter spread directly from case to case, probably air-borne. Social functions were in abeyance, and so there was no contact between male and female patients. The sulphonamides appear to have a definite prophylactic value, but further trial would be necessary before a positive statement could be made. Similar claims were made for mepacrine⁵ which have not been borne out.¹ Even allowing their value, the incidence was directly related to the degree of overcrowding. The exceptionally high incidence in the infirmary ward may be explained by the difficulty of completely isolating so many, of "spotting" early cases among the totally demented by an already overworked nursing staff, and of controlling the excretions of the faecal incontinents.

The clinical course of the illness suggested a rapid invasion with most of the damage at the outset, which may explain why the sulphonamides were effective in prophylaxis but not in treatment.—I am, etc.,

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- 3 MacGregor, I. (1946). *British Medical Journal*, 2, 225.
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"Q" Fever

SIR,—I was more than interested in the article on "Q" fever by Drs. J. E. Caughey and J. A. Dudgeon (Nov. 1, p. 694). Well do I remember reading the article in question on "Primary atypical Pneumonia" which has turned out to be an epidemic of "Q" fever. The lesson to be learned from this is of prime importance in my opinion. To my mind it is a great pity that the term primary atypical pneumonia was ever introduced, because at the best it is vague, confusing, apt to be misleading, and falls readily from the lips of those who are satisfied with a diagnosis which usually depends on so-called typical radiological appearances in the lungs or, in their absence, on so-called typical findings on auscultation of the chest together with the general symptoms of a febrile virus infection, a white count usually below 10,000 per c.mm., and the occasional presence of a rash, splenomegaly, lymphadenopathy, etc. The presence of cold agglutinins in the serum is said to place the diagnosis beyond all shadow of doubt, but greatest stress is usually laid on the radiological findings and the physical signs in the chest.

Physicians who have much experience of febrile diseases, especially prolonged febrile illness in tropical and subtropical countries, realize how often there are present abnormal signs on auscultation of the chest, but these as well as positive radiological findings in the lungs do not necessarily result in a diagnosis of pneumonia of some sort—e.g., primary atypical pneumonia, "pneumonia," or "pneumonitis." Such cases, with or without evidence of radiological findings in the lungs and with a white-blood count of less than 10,000–12,000 per c.mm., are invariably investigated along the lines of a case of P.U.O., which includes blood films and thick drops for malarial parasites, blood cultures, repeated total and differential white-blood counts, appropriate agglutination reactions when facilities are available, and the other relevant investigations. I wonder how many cases of enteric-group fever, other septicaemias, and typhus-group fever, etc., have been diagnosed as primary atypical pneumonia or just "pneumonia."

I never make a diagnosis of primary atypical pneumonia until the last resort, as I believe that such a diagnosis should not be made until all relevant investigations have been carried out and all other relevant causes have been excluded. In this way errors in diagnosis should be lessened. Too often are certain febrile diseases excluded on the degree of the severity of the signs and symptoms or on the absence of some so-called typical sign or symptom. The converse is also true. This attitude is indicative perhaps of book knowledge and is characteristic of self-confidence in the inexperienced. Not infrequently I have seen physicians on clinical grounds alone exclude, with what appeared to be confidence and to their own satisfaction, such diseases as enteric-group fever, typhus-group fever, leishmaniasis, hepatic amoebiasis, and even plague, while later laboratory evidence revealed the true nature of the diagnosis. Surely it is about time that it was universally appreciated that specialist investigations are not just carried out to confirm a clinical diagnosis. Happily that day is at least passing—although not quickly enough—because so often such specialist investigations are necessary to make the diagnosis. Unfortunately some young physicians go to the opposite extreme