Correspondence

Because of the present high cost of producing the Journal, and the great pressure on our space, correspondents are asked to keep their letters short.

Blood Groups in Relation to Peptic Ulceration

SIR,—The paper by Professor Ian Aird *et al.* (*Journal*, August 7, 1954, p. 315) induced us to investigate the ABO blood groups in patients who had been treated for peptic ulceration in the two surgical departments of Ullevål Hospital, Oslo, in the years 1945–54. The results of the investigation will be published elsewhere. They were in full agreement with those of Aird *et al.* In particular, it is of interest to note the difference in the proportion A/A+O between patients with gastric ulcer and patients with duodenal ulcer. The data from both investigations are as follows :

	No. of Patients			
	Gastric Ulcer Blood Group		Duodenal Ulcer Blood Group	
	O	A	O	A
London	314	228	535	311
Manchester	122	90	225	148
Newcastle	102	58	288	157
Oslo	179	189	282	250

The difference between the two ulcer types is not significant if either investigation is considered alone. If, however, we combine the data according to the method recommended by Aird *et al.*, we find χ^2 for the difference = 5.63 with 1 degree of freedom. The probability that such a difference, or an even greater one, will arise by pure chance is less than 1 : 50 ($P = 0.018$). The weighted mean difference in the proportion A/A+O between gastric ulcer and duodenal ulcer is 0.040. A significance test for the combined material according to the method suggested by Cochran¹ gives approximately the same result ($P = 0.021$).

The Norwegian data thus strongly support the observation reported by the British investigators that there seems to be a difference in blood group frequencies between gastric and duodenal ulcer.—We are, etc.,

KNUT WESTLUND.
HELGE HEISTÖ.

Oslo, Norway.

REFERENCE

- ¹ Cochran, W. G., *Biometrics*, 1954, 10, 417.

Fluoridation of Public Water Supplies

SIR,—Though it is evident from the correspondence in the *Journal* that the hazards of fluoridation are a subject of controversy among the scientific experts, the question is not one really of science but of medical ethics and of the freedom of the practitioner to give what he judges to be the best advice in the interests of his patient, and of the patient to carry it out.

It is, at any rate, legitimate for any one, medical or lay, to hold that we already absorb a sufficient level of toxins, and that a small but daily addition through the water supply is inadvisable, especially to anyone in poor health. There is no medical indication whatever for the administration of fluorides to adults, and for local councillors to make the decision to do this, and to impose it upon the patients of those medical practitioners who may consider it inadvisable in individual cases, is a dangerous precedent for interference in the medical field by the political power. I can see no reason why the treatment of dental caries should make an exception to the rule that consent to any form of treatment should be essential, and that no one should be subjected to any treatment which offers him no expectation of benefit.

I understand that enthusiasts for fluoridation will not be content until the benefits of fluorides are made available to children who do not drink the public water supply; and if

this can be done, they can be made available to all children whose parents consent. When the same amount of attention has been paid to finding legitimate ways of making this treatment available to children, without forcing it on everyone else, as has been paid to studying this illegitimate method of dosing everybody, including unwilling people, through the public water supply, I do not believe that the difficulties will be found insuperable.—I am, etc.,

London, S.W.1.

FREDERICK C. ENDEAN.

Relationship Between Menstruation, Urine Acidity, and Ovulation

SIR,—When investigating the pH of the first morning urine specimen of a representative sample of schizophrenics and healthy normals, a significant difference in acidity (measured with the potentiometer) was found between the two groups ($P=0.05$), the urine of schizophrenics being the more acidic.

During the course of these studies I made the following observation : the pH of the first morning urine specimen of a healthy female drops to pH values around 5.2 to 5.0 24 to 12 hours before the onset of menstruation; for instance, pH=5.8 two days prior, pH=5.25 one day prior, and pH=5.1 about three hours prior to the arrival of menstruation. There is also an increase in urine acidity (drop in pH) before the onset of ovulation, though this is less marked than that occurring shortly before the onset of menstruation.

These preliminary observations need, of course, confirmation and extension before their usefulness can be assessed in terms of practical applicability—for example, by use of a special paper pH-indicator revealing the varying degrees of acidity in the first morning urine by different colours.—I am, etc.,

Regina, Canada.

ROLAND FISCHER.

Heatstroke or Malaria?

SIR,—Under the heading "Lowering the Body Temperature," Dr. C. E. H. Milner writes a letter (*Journal*, October 9, 1954 p. 872) which includes some of his experiences of hyperpyrexial heatstroke in Mesopotamia in the summer of 1917 during the first world war.

He states : "About 5 p.m. the ambulances began to roll up . . . full of blue, unconscious, naked soldiers, suffering from alleged heatstroke. These proved in reality to be cases of hyperpyrexial cerebral malignant tertian malaria." His letter, however, does not state the proportion of blood smears that proved to be positive. Also, about 36 years ago, in an article on this same epidemic, Dr. Milner¹ expressed the view "that heatstroke, as a clinical entity, is non-existent; and is nothing more than a symptom, on a hot day, of malignant tertian malaria."

This would appear to mean that, in Dr. Milner's opinion, it is impossible for any person to be attacked by hyperpyrexial heatstroke unless he has previously been infected with the parasite of malignant tertian malaria. This point, because of its obvious bearing upon immunity and treatment, is one of considerable practical importance. The truth of it would be difficult to prove conclusively, and equally difficult to disprove. However, the following evidence may help to clarify the situation. While in Mesopotamia in 1917 I dealt with an exceptionally large number of heatstroke cases during this memorable epidemic, and found many instances where no evidence of malaria could be obtained despite careful search for it. I have no record of the proportion of positive malarial blood smears that occurred in my cases, but, from memory, I doubt if they exceeded 25%. I regarded the malaria merely as a complication which would naturally be expected among a community of soldiers, many of whom had lived for a considerable time in India or Mesopotamia.

In August, 1917, several troopships arrived at Basra from England at intervals of about a week. In the first ship-load an appalling outbreak of heatstroke with a very high mortality rate

occurred during the ten-hour journey up the Shatt-el-Arab river. Following this I received instructions to meet the remaining ships at the head of the Persian Gulf, and to deal with the heatstroke problem during the journey up river to Basra.

The occurrence of fully developed cases of heatstroke on these latter ships was greatly modified by a system of prevention which had already proved thoroughly successful in my 60-bed hut at the hospital in Basra, which I described in 1919.² Nevertheless, on each of these latter ships about four or five actual heatstrokes occurred, and about 40 others certainly would have occurred had the patients not been placed under preventive treatment (all of the prevented cases had complete suppression of sweating, and a temperature of at least 103° F. (39.4° C.)).

Here we have a series of instances where heatstrokes and potential heatstrokes occurred in large numbers at the final stage of a voyage from England before any of the troops had set foot on Mesopotamian soil. And the question arises—Were all or any of these heatstroke patients malarial subjects? There was no opportunity to examine blood films during the river journey.

The S.M.O. of one of these ships told me that no attacks of malaria had been treated on the voyage.

This ship came via the Cape, and the only stoppages were a couple of days at Capetown and Durban which took place in July, during the southern mid-winter. The troops, therefore, could not have picked up malarial infection during the voyage. Consequently, if there were any malarial soldiers among them they could have become infected only through a previous visit to some malarial country. But changes of climate are said to precipitate malarial attacks in infected persons; and as no such attacks occurred on a long voyage, which crossed the Equator twice, and spent some time in the fairly cold region of Capetown in mid-winter, it seems that there were few, if any, infected soldiers on board. Unfortunately, I did not ask the heatstroke patients on my ships if they had ever left England before. Their replies might have produced conclusive evidence regarding their freedom from infection. However, the troops were not British "regulars," and I formed the opinion that this ship, at any rate, provided an example of heatstroke occurring under circumstances in which the influence of malaria could be fairly and reasonably eliminated.

Results of treatment throw some further light upon this subject. All of the heatstroke patients on my ships had recovered consciousness before the ships reached Basra; and none of them received a single grain of quinine, nor was any venesection done. The treatment given served no purpose other than to remove their hyperpyrexia: they recovered quickly. Furthermore, I have been fortunate enough on several occasions to be present (with the means of treating the hyperpyrexia ready at hand) at the moment that the patient became unconscious, and have proved that the immediate application of treatment to such early cases can restore consciousness within 20 to 30 minutes. Such a quick result is possible when the superheated blood has been allowed to affect the brain for only a minimum length of time; but it seems highly improbable that a similar result could be obtained by hyperpyrexial treatment alone (and no antimalarial therapy included) if the unconsciousness was due to cerebral malignant tertian malaria.

A heatstroke country is, generally, also a malarial country. It is therefore not surprising to find heatstroke and malaria co-existing in people who have lived in a malarial district long enough to become infected. But I feel quite sure that heatstroke can, and often does, occur alone without any necessary association with malaria.—I am, etc.,

Ewey Bay, Australia.

K. G. HEARNE.

REFERENCES

- ¹ *British Medical Journal*, 1918, 1, 638.
- ² *Ibid.*, 1919, 1, 516.

Chicken Anti-ulcer Factors in the Treatment of Gastric Ulcer

SIR,—The question whether nutritional factors which counteract gizzard ulcer in chicks may cure gastric or duodenal ulcer in man is of considerable interest to the clinician. We have therefore carried out a series of experiments in order to test this problem. One group of ulcer patients was given a dietetic preparation containing milk and cream powder, vitamins, a decholesterolized lipid extract of calf's brain, and defatted calf's brain. A control group was given a corresponding preparation from which the latter two components were omitted. Calf's brain or a lipid extract thereof together with the defatted calf's

brain have previously been found to counteract gizzard ulcer in chicks.¹ Until now 22 hospitalized patients with roentgenologically verified ulcer have been treated.

In both groups most of the patients became free of subjective symptoms during four weeks of treatment. After treatment x-ray examination showed that ulcer had disappeared in about half of the patients in both groups. This is the same result as that obtained by common dietetic treatment. Thus the presence in the preparation of the above-mentioned chicken anti-ulcer factors had no influence on the results.—We are, etc.,

Copenhagen.

CARL AAGE KROHN.
ERLING LUND.

REFERENCE

- ¹ Dam, H., et al., *Acta physiol. scand.*, 1950, 21, 315.

** An article on this subject by Dr. K. Brøchner-Mortensen and his colleagues appears at page 818 of this issue.—ED., *B.M.J.*

Eclampsia

SIR,—Dr. John Sophian (*Journal*, March 12, p. 666), in his criticism of my letter (*Journal*, February 19, p. 481), flatters me unduly in supposing that I have established a new hypothesis of eclampsia. I have but pointed to new evidence in the aetiology, and—as I have no possibility of testing it—I have asked the profession—that is, the clinicians—to confirm or refute Grauert's claim. Far be it from me to exchange one hypothesis for another one. On the contrary, I fully agree with Dr. Sophian that the right way of combating disease can only be attained by search for aetiological factors. And this is just what I proposed. Unfortunately, the Editor, for brevity's sake, asked me to cut that part of my letter in which I pointed to the contradictory premises on which the endocrinological hypothesis is based.

Dr. Sophian's reference to the well-known cases of eclamptic fits in the absence of a foetus does not invalidate the theory of endogenous toxins. Modern pathology knows by comparative study of other diseases that abnormal reactions can continue automatically even after the "focus" has ceased to act.

In his criticism of the diet I am advising Dr. Sophian has overlooked the much more important items of avoidance of all overheated fats and the increased intake of proteins and roughage (recently also advised by Australian clinicians). The low salt consumption is only one part of the diet.—I am, etc.,

London, W.1.

ALBERT W. BAUER.

Night Cramps

SIR,—I was interested to read the annotation on night cramps (*Journal*, March 12, p. 653). From time to time I have read of various treatments and have often wondered when I would see mephenesin recommended. It has always seemed to me that if night cramps were actually cramps this would be a rational treatment though not a cure.

Since 1952 I have done various locums in general practice and have met with about a dozen cases; unfortunately I have kept no exact record, but every case had complete relief, the dose of mephenesin being one tablet at night or a dessertspoonful of the elixir unless it caused a burning sensation in the throat. All the cases of cramp were in old people. I would be most interested to hear whether I have been just lucky with mephenesin so far, whether it is just a placebo, or whether it has a therapeutic value.—I am, etc.,

Woodbridge, Suffolk.

H. R. ODLUM.

Management of Brain Abscess

SIR,—The article by Mr. Walpole Lewin (*Journal*, March 12, p. 631) on his series of cases of brain abscess was most interesting and instructive, and his wise conclusions based on his excellent results are admirable. However, I cannot agree with his advice on the management of all cases of otogenic abscess. It has been known for many years, and discussed in detail by Macewen,¹ that many of these abscesses