Correspondence

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Integrity of the Profession

SIR,—When the General Medical Council decided to revise its rulings on abortion to conform with the current will of Parliament there vanished almost overnight one of the great safeguards for the integrity of the medical profession. An ethical vacuum has now been created, which cannot be filled entirely by each of us adopting our own particular brand of ethics. A profession is most vulnerable, not from its external critics, but from the permissive minority within its own ranks. Medicine is no exception, and without some authoritative body which can clearly enunciate ethical principles freely accepted by the profession as a voluntary discipline, then we will face intolerable pressures ahead.

Has not the time come for the British Medical Association, together with the Royal Colleges, to set up a body to provide general guidance to the profession, Parliament, and the public on ethical issues, especially those relating to the value of human life? If such guidance had been available two years ago, and had been acted upon, we might have been

spared some of the anomalies of the Abortion Act which are now causing such a conflict of loyalties. A body of this kind, if endowed with sufficient authority, could also play a vital part in protecting the medical profession from being manipulated by politicians, who may be subject to divers pressures, and who in any case do not have the ultimate responsibility for putting their legislation into practice.

With the abysmal lack of moral leadership in Britain today more and more people are turning to their doctors for the kind of wise counselling which they can no longer obtain from other sources. This may be a key to our future role in society. Could it be that the medical profession, and the B.M.A. in particular, by giving a fearless lead in ethical and social issues, may provide a stabilizing factor for the whole nation in the days which lie ahead?—We are, etc.,

R. J. D. BROWNE.
D. C. STURDY.

Birmingham 30.

Heroin Dependence in Britain and the U.S.A.

SIR,—I feel that comment is required on the comparison of the mortality of British and New York City addicts made by Drs. T. H. Bewley, O. Ben-Arie, and I. P. James in their article entitled "Morbidity and Mortality from Heroin Dependence" (23 March, p. 725).

The comparison is a doubtful one, because both the numerator of deaths and the denominator of size of the New York City addict population are uncertain, and in any case not comparable to the Home Office index and the years-at-risk calculations used for the British addicts. These are different sums and give different answers. The authors use as a basis for their statement of the New York City rate Helpner and Rho¹ and Louria *et al.*² Unfortunately they neglect the far sounder study of Duvall, Locke, and Brill,³

which used patients from the New York City area who had been discharged from the Lexington, Kentucky, narcotics hospital. They found a mortality rate of about 2 per 100 per year, a rate equal to that found by the authors among British addicts.

The reasons for the discrepancy between the first two studies and Duvall *et al.* are several. Firstly, in terms of the numerator the 350 deaths (380 in actuality) given for addicts in 1964 in this city were identified addicts only. This number has risen year by year through the 1950s and early 1960s, and then jumped from the 1964-through-1966 level to more than 500 in 1967. During this period the Office of Medical Examiner has become increasingly interested in the subject, and also, coincidentally, has introduced administrative changes centralizing

much of the authority and functions of the borough medical examiners in the Manhattan office of the Chief Medical Examiner. This has resulted in an increase in identified addict deaths and a concomitant shift from an original Manhattan concentration of these deaths (75%) to one representing more deaths from the other boroughs (Manhattan now constitutes 45% of the total). Secondly, with regard to the denominator, the population of addicts in this city is much larger than the number of persons on the Narcotics Registry, but is more in the order of 50,000—a qualified calculation from Dr. F. Kavalier.⁴

The rate given for British addicts' mortality is based on a calculation of years at risk derived from entry to the index or register. However, it is absurd to assume that in Britain people become addicts solely at a point at which they are registered. There must be a preceding period of trial and increasing drug dependence. Therefore the registered addicts are a subgroup of the entire narcotic-using population (proportion unknown), selected because of greater drug dependence, longer usage, etc. This population of heavy users is not comparable to addicts in this city, who enter the Narcotics Registry files on the basis of reports from hospital, the police, and social agencies. To further strengthen this point the causes of death for these two groups are dissimilar. In New York City over 70% die of so-called "overdose"—in reality an unexplained syndrome of sudden collapse after intravenous injection, with the major finding at necropsy being pulmonary oedema.⁵ This is apparently not seen in the British addicts, and they in turn suffer from proportionately more violent death, and deaths due to septic complications. Since it has been noted above that the overall mortality rate may be equivalent in the two populations if deaths due to "overdose" are eliminated from consideration the registered British addicts would then appear to have a far higher rate.

The registered British addicts may well be in poorer physical shape and have more severe illnesses than their New York City counterparts. This does seem to be the case

with regard to a recent report on pulmonary infections in London addicts.^{6,7} These are relevant considerations which I believe to be within the scope of the article by Dr. Bewley and his colleagues, and which deserve brief mention.—I am, etc.,

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Blood Base Changes

SIR,—Dr. J. L. Anderton and others (4 May, p. 279) suggest that it is necessary to use arterial blood for the determination of a patient's metabolic acid-base state (base excess). I would dispute this. It is true that during pancreatic secretion venous blood from the gut should have a metabolic acidosis as compared with arterial blood, but the tissues of the arm normally neither produce nor utilize non-volatile acids, therefore there is no difference, due to this cause, between blood from the brachial artery and from an antecubital vein. However, because oxyhaemoglobin is a stronger acid than reduced haemoglobin, venous blood has a relative metabolic alkalosis due solely to its reduced haemoglobin content. When the Radiometer Astrup apparatus is used to determine the metabolic acid-base state, whether as base excess, buffer base, or standard bicarbonate, the blood is equilibrated with mixtures of carbon dioxide in oxygen, and the haemoglobin perforce becomes fully saturated with oxygen. The buffer line determined by this technique is therefore that of fully oxygenated blood, and, provided that the patient is not suffering from arterial hypoxaemia, it will give an estimate of his arterial metabolic acid-base state.

It is not disputed that the PCO_2 of venous blood is higher than that of arterial blood from the same patient, but the Astrup technique determines the metabolic acid-base state of the blood whatever the PCO_2 . It is perhaps worth mentioning that the difference between the metabolic acid-base states of arterial and venous blood may result in errors when the Astrup technique is used for the determination of the carbon dioxide tension of desaturated blood. In this technique the pH which is used in the interpolation is that of the blood as it was taken from the patient, whereas the buffer line is that of fully oxygenated blood. If the blood is desaturated, therefore, the calculated PCO_2 will be too low by an amount which depends *inter alia* on the concentration of reduced haemoglobin. Errors due to this discrepancy may be minimized by calculation of the position of the true—that is, venous—buffer line and the use of this for the interpolation process. The shift of buffer line may be determined by

means of the approximate factor of Siggaard-Andersen¹ or the more precise factor of Kelman,² which takes account of the influence of pH and PCO_2 on this parameter.—I am, etc.,

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Noradrenaline in the Tropics

SIR,—The catecholamines adrenaline and noradrenaline are well known to be unstable in dilute solutions, being affected by many conditions, such as oxygen, alkaline media, high temperature, and the presence of heavy metal ions.^{1,2} Although it has recently been reported that under temperate conditions noradrenaline is stable for up to four hours in infusion solutions as used clinically,³ it was felt desirable to confirm that this stability would pertain in the tropics. The stability of noradrenaline in infusion solutions under local conditions was therefore investigated, and the preliminary findings are reported in this communication.

Solutions of noradrenaline in physiological saline (0.9% NaCl), in dextrose (5%), and in dextrose (5%) in physiological saline were investigated. The infusions were made up of 4 mg. noradrenaline (injection noradrenaline bitartrate, expressed as salt) in 540 ml. of the solution under test. All were prepared according to B.P. requirements. The flow rates of the solutions were adjusted so that each "drip" lasted four hours. The noradrenaline content immediately after mixing, at the second hour, and at the fourth hour was estimated fluorometrically.⁴ Room temperature at which the "drips" were run was 29–30.5°C.

Noradrenaline was found to be stable in dextrose (5%) as well as in dextrose (5%) in physiological saline (three separate observations were made on each solution). However, with physiological saline there was considerable loss of activity (four separate observations). Activity remaining after two hours was $71 \pm 5.4\%$ (mean $\pm$ S.E.) and by four hours was $36 \pm 12.4\%$. These reductions in activity were statistically significant ($P < 0.02$ for two hours and $P < 0.01$ for four hours). The pH of physiological saline varied from 7.2 to 7.7, that of the other two solutions was constantly at 4.0. After the addition of 4 mg. noradrenaline bitartrate (contained in 1 ml.), the pH of physiological saline fell to 5.7. The pH values of dextrose and dextrose in saline were lowered by only 0.1.

These findings suggest that when noradrenaline is to be given in infusion solutions under tropical conditions the use of physiological saline is best avoided unless steps are taken to modify the pH.—I am, etc.,

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Psychiatric Problems and General Practitioners

SIR,—Having just read the article "Participation of General Practitioners in Community Psychiatry" (20 April, p. 168) I feel that its conclusions could be damaging to the trend for integration of community health care. It is based on a small and unrepresentative sample, compared with the national statistics of general practice, and it is the work of two observers only, in collaboration. I hope, therefore, that these findings will not be taken to represent a true picture of general practice.

In the sample of general practitioners chosen 50% were over 50, and 60% had practice lists of over 3,000 urban patients. In the whole country only a third of general practitioners are over 50 and the average list is 2,470 (Ministry of Health figures 1967). The majority of these doctors, then, were hard worked, or ageing, or both. Many had trained before psychiatry was given any prominence (28% had no psychiatric undergraduate training at all) and before hardly any of the present psychiatric drugs had been developed. With the tripartite health service they would have indeed been detached "from the organization and operation of specialist psychiatric services." It is not surprising, therefore, that this survey found some indifference to psychiatry. Equally, future changes probably will not concern many of them—they will have retired.

Most of the implied criticisms in this article could be removed once the skills of the social and welfare workers are more directly a part of the general practice community—that is, reluctance to take on more psychiatric patients, or to give aftercare for the chronically disabled. The attitude of an older section of general practitioners should not be allowed to discourage such progress. It is a pity that this survey gave such little prominence to such enthusiasm as it did reveal. It would be interesting to compare this survey with another, representing the views of the younger generation of general practitioners. I suspect this would be much more encouraging.—I am, etc.,

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P. R. HATHERLEY.

Group A Streptococci Resistant to Lincomycin

SIR,—We were interested in the letter from Drs. J. Kohn, J. H. Hewitt, and C. A. M. Fraser (16 March, p. 703) describing an outbreak of infection in a burns unit with *Streptococcus pyogenes* resistant to lincomycin and erythromycin.

Since the isolation of erythromycin-resistant strains of *Str. pyogenes* in burns which we reported nine years ago¹ all strains of *Str. pyogenes* isolated from burns in this unit have been tested for sensitivity to erythromycin. No further strains resistant to erythromycin or to other macrolide antibiotics were found between 1959 and December 1967, in spite of the use of oral erythromycin as our standard treatment for infection of burns with *Str. pyogenes*. In 1963 we made a small controlled trial of lincomycin in patients with *Str. pyogenes* infection of burns; no streptococci resistant to lincomycin