1/10-1/1,000. Such reactions are abolished if the serum is preheated for 1½ hours at 56°C prior to injection.

These findings are in contrast to those of Dr. Bryant and his colleagues in that these workers were unable to show attachment of IgE at two and four hours after sensitization. However, they do agree with the findings of other workers who have been able to sensitize chopped human and monkey lung and human skin passively in vitro by exposure to IgE antibody for a short time.

Our results indicate that human IgE attaches rapidly to macaque monkey skin and elicits a P.C.A. reaction at two, four, and 24 hours; these reactions being abolished if the serum is heated for 1½ hours at 56°C. Most importantly, they emphasize the importance of using "clean" experimental monkeys with a known history.—We are, etc.,

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Consultant Posts in Baghdad

SIR,—Several advertisements have appeared in the *B.M.J.* inviting applications from consultants in certain specialties to apply for service for periods of three months to one year in a special unit attached to the Medical City Teaching Hospital, Baghdad. As one who has been working for over two years as a visiting professor to the medical college and in the teaching hospital I can assure any consultants who may be interested that I have found work in Baghdad a most rewarding experience. The clinical conditions encountered are many and reasonable facilities for their investigation and management exist. Educational standards are good; a high general standard of education and knowledge of English is demanded of all entrants to medicine, all teaching being in English; the students are keenly interested and lively; and a teacher is often bombarded with questions.

Apart from medicine Iraq is a wholly fascinating country to anyone interested in history and archaeology—the centre of an ancient and sophisticated civilization, of which substantial remains going back 5,000 years still exist, and also there is much to see of the early Muslim world. Finally, may I say that ancient Arab courtesy and welcome have to be experienced to be understood. Without them I doubt if I, who had originally intended to serve here for only one year, would now be entering my third year of service.—I am, etc.,

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Diet and Colonic Cancer

SIR,—In your leading article, "Diet and Colonic Cancer" (2 March, p. 339), you credited Mr. Denis P. Burkitt for the thesis "that diet and faecal constituents are important in the aetiology of cancer of the

large bowel," which he attributes to "the low-residue diet of the West, high in refined carbohydrates and low in fibre." This has obviously happened because Burkitt in his article¹ did not even refer to my work,² published much earlier than his, let alone give me due credit for this thesis. Allow me, therefore, to quote from my article² not only to establish priority but, even more important, to draw attention to the explanation why diets rich in cellulose and vegetable fibres are protective. Perhaps this may lead to additional information which could go a long way to prevent colonic cancer.

In my article I stated that cancer of the colon was far less common in north Indians as compared with south Indians, and that one likely explanation might be that while north Indian diets are rich in roughage, cellulose, and vegetable fibres these are almost completely lacking in the south Indian diets. The article continued: "We have investigated this point further by examination of faeces in these disparate groups, which showed that while vegetable fibres were abundant in the stools of north Indians these were completely absent in those of the south Indians. This may have an important implication because of the recent evidence³ that fermentation of cellulose and vegetable fibres in the intestine gives rise to large quantities of acetic acid which may thus act in a protective manner on the intracellular mucus of the mucous membrane of the colon by virtue of its acid milieu in north Indians while giving no such protection to the south Indians."

An acid milieu precipitates the mucus and prevents it from escaping from the mucus cells, thus protecting the mucus cells from the adverse inflammatory and proliferative changes referred to above. In an alkaline milieu, on the other hand, the intracellular mucus of the mucous membranes is rendered fluid and escapes from the cell, thus producing inflammatory and proliferative changes in the mucosa with a marked increase in mitotic activity. Such hyperplasia of long duration is often a prelude to neoplasia.^{4,5}—I am, etc.,

S. L. MALHOTRA

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India

¹ Burkitt, D. P., *Cancer*, 1971, 28, 3.

² Malhotra, S. L., *Gut*, 1967, 8, 361.

³ Peblack, G., French, T. H., and Folley, S. H., *Biochemical Journal*, 1951, 48, 411.

⁴ Poel, W. E., *Progress in Experimental Tumor Research*, 1964, 5, 53.

⁵ Malhotra, S. L., *Journal of the Indian Medical Association*, 1970, 55, 265.

Geriatric Policies

SIR,—May I take sides in the dialogue between Professor G. F. Adams (28 September, p. 789) and Dr. H. M. Hodgkinson (2 November, p. 290) and state that the easiest solution of the geriatric problem is permanent hospital admission. This is the line of least resistance, and by taking it everybody is left alone in peace. A time comes, of course, when all the available geriatric beds are taken, but this again is a perfectly calm and peaceful stage of the long waiting list.

As a geriatrician with a high turnover and no long-stay problem I must admit that I am a "spiv." Yes, "somebody must carry the can" for me. They are my own long-suffering consultant colleagues in other specialties who just have to learn to deal

with their own geriatric problem instead of getting their "chronic cases" transferred to the geriatric unit. Some of them do it in good humour, some of them with grinding teeth. But they all do it. There is just no alternative.—I am, etc.,

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Disco Deafness

SIR,—Early deafness in young people as a result of exposure to excessive noise in "discos" must now be assuming epidemic proportions. The importance of this problem has been brought especially to my mind because an 18-year-old medical secretary, who has worked for me, has now been found to be suffering from this condition. If every general practitioner in the country had one such new case a year there would be 20,000 new cases in the country annually.

We can only guess at the degree of disability in later years when the normal hardness of hearing in old age would be replaced by disabling deafness in many people. This is a preventable cause of deafness and the medical profession should press for controls to regulate sound at "discos." No doubt controls will eventually be applied, but in view of the enormity of the problem, the sooner the better.—I am, etc.,

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Variation in Intravenous Infusion Rates

SIR,—We share the views of Drs. F. C. Flack and T. D. Whyte (17 August, p. 439) that most hospital staff are probably unaware of the variability of intravenous infusion rates under normal ward conditions. In a recent study at this hospital we measured the time over which 1,898 infusions were administered to 141 medical and surgical inpatients. The times at which the infusions were set up and discontinued were recorded by the nursing staff and later compared with the rate of administration prescribed by the physician or surgeon.

The results, which are shown in tables I and II, illustrate the fact that less than half the infusions were administered over the

TABLE I—Proportion of 1,898 Infusion Bottles Which Deviated from the Prescribed Rate

Deviation (%)	Percentage of bottles	
	Fast	Slow
<20	8.58	16.58
20-40	6.64	8.06
40-60	2.00	4.43
60-80	0.68	1.74
80-100	0.53	0.95
>100	0.05	1.31

No deviation in 48.42%.

TABLE II—Comparison of Deviations Found with Solutions Containing Additives and Those without Additives

Infusion Rate	Bottles with Additives	Bottles without Additives
Too fast	18.0%	18.9%
Too slow	27.9%	37.7%
Correct	54.1%	43.4%

correct period. The skewness of the distribution suggests a bias towards slower rather than more rapid rates. Surprisingly perhaps this bias seems less evident when the solutions contained additives, though in this subgroup the proportion of correctly administered solutions was higher.

It seems likely that the methods used in our survey will underestimate the true extent and size of variations in rate. Because we measured only the mean infusion rate short-term decreases or increases would be missed. Attempts to correct these variations are commonly encountered in the ward as efforts to "get the bottle through in time" or to "slow it down a bit until we have someone free to change it." We look forward to hearing about the servo-controlled regulating clamp mentioned by Drs. Flack and Whyte, which will take a little of the uncertainty out of intravenous infusion therapy.—We are, etc.,

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Department of Nursing,

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Kidneys for Transplantation

SIR,—I would certainly endorse Mrs. Elizabeth J. Ward's plea for doctors to help the supply of viable cadaver kidneys to transplant surgical teams. I cannot, however, agree that patients undergoing regular haemodialysis are relegated to "uselessness and wretchedness." Many patients are able to continue their work or continue to do normal household chores.—I am, etc.,

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Tapeworms and Isolation

SIR,—I am pleased to see the warning by Dr. F. J. Wright (14 September, p. 685) on the danger presented by carriers of *Taenia solium*. The eggs passed by such patients are a danger not only to themselves but also to all around them. In many parts of the world cysticercosis is a common cause of adult-onset epilepsy^{1,2} and of other obscure nervous conditions, and if serology³ is any criterion is far more prevalent than presentation of the adult worm.

However, it would be manifestly impossible to isolate all cases of taeniasis and one would therefore make a plea for adequate species identification of tapeworms encountered. The number of branches to the gravid uterus is not a reliable criterion, the mature bisexual segments giving more information.⁴ These should be submitted to an adequately informed laboratory.—I am, etc.,

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¹ Powell, S. J., et al., *Annals of Tropical Medicine and Parasitology*, 1966, **60**, 152.

² Powell, S. J., et al., *Annals of Tropical Medicine and Parasitology*, 1966, **60**, 159.
³ Proctor, E. M., Powell, S. J., and Elsdon-Dew, R., *Annals of Tropical Medicine and Parasitology*, 1966, **60**, 146.
⁴ Proctor, E. M., *South African Medical Journal*, 1972, **46**, 234.

Variability in Response to Drugs

SIR,—Professor M. D. Rawlins's survey, "Variability in Response to Drugs" (12 October, p. 91) contained much useful information. However, we wish to comment on the section on nortriptyline. This states that "the therapeutic effect of nortriptyline in the treatment of endogenous depression occurs only at plasma concentrations of 50 to 140 ng/ml." This conclusion is one reached by Åsberg *et al.*¹ They studied the relation between plasma concentration of nortriptyline and therapeutic effect after two weeks' treatment in 29 depressed patients. A period of two weeks is not adequate to assess the therapeutic effects of tricyclic antidepressants: four weeks is a minimal period. In addition, the patients in their study received hypnotics including pentobarbitone. Barbiturates have been shown to lower plasma tricyclic levels significantly.^{2,3} Our contention is that the quoted study does not prove what Professor Rawlins states to be factual. Studies on the relation between clinical response and plasma tricyclic levels have produced conflicting results,^{4,5} so that dogmatic statements in this field are inappropriate.

Several years of study in this field have shown us that studies of groups of depressed patients using a fixed oral dosage of tricyclics show different results to studies of individual patients receiving changing amounts of daily oral medication.⁶ Studies of individual depressed patients have shown some patients who respond only to high oral dosage and high plasma levels (that is, 200-300 ng/ml) of nortriptyline.⁷ We recognize, as do most clinical psychiatrists, that some depressed patients do not respond to tricyclics whatever the oral dosage or plasma levels of the drug.—We are, etc.,

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¹ Åsberg, M., et al., *British Medical Journal*, 1971, **3**, 331.
² Sjöqvist, et al., *Proceedings of the European Society for the Study of Drug Toxicity*. Vol. 9, Excerpta Medica, International Congress Series, No. 145, p. 246, 1967.
³ Burrows, G. D., and Davies, B., *British Medical Journal*, 1971, **4**, 113.
⁴ Braithwaite, R. A., et al., *Lancet*, 1972, **1**, 1297.
⁵ Burrows, G. D., Davies, B., and Scoggins, B. A., *Lancet*, 1972, **2**, 619.
⁶ Burrows, G. D., et al., *Clinical Pharmacology and Therapeutics*, 1974, **16**, 639.
⁷ Burrows, G. D., M.D. Thesis, 1972. University of Melbourne.

Comparison between Free Thyroxine Index and Effective Thyroxine Ratio

SIR,—In order to assess the value of a new in vitro thyroid function test, the effective thyroxine ratio (E.T.R.), Dr. E. G. M. D'Haene and others (21 September, p. 708) have compared this test with its present alternative, the free thyroxine index (F.T.I.). Comparisons were made with both Pearson's and Spearman's correlation analysis and the

resulting coefficients were remarkably different (Pearson's $r_P = +0.84$ and Spearman's $r_S = +0.68$).

I agree with the authors when they criticize previous similar studies in which r_P was reported on data from inhomogeneous populations comprising hypo-, eu-, and hyperthyroid subjects. Under these circumstances a Gaussian distribution is very unlikely and a distribution-free method should be used like the rank correlation devised by Spearman. However, when agreement to the kind of correlation analysis has been reached, there remains another item making comparisons between different studies difficult—that is, differences in relative sizes of the subpopulations.

In a similar study Wellby *et al.*¹ found highly significant correlation coefficients in myxedematous ($r = +0.71$, $n = 21$) and thyrotoxic ($r = +0.90$, $n = 27$) patients, but a non-significant correlation in normals ($r = +0.19$, $n = 28$) yielding a "pooled" coefficient of $+0.95$ ($n = 76$). The kind of correlation analysis used is not presented in their paper, but is assumed to be that of Pearson.

We have measured the E.T.R. and calculated an F.T.I. (product of a non-commercial total thyroxine method and triiodothyronine (T3) resin uptake by the Triosorb method) in consecutive, non-selected blood donors and non-treated thyroid inpatients. Correlation analysis with both methods mentioned gave the following results: 79 blood donors, $r_P = +0.16$ and $r_S = +0.36$; 43 thyroid patients, $r_P = +0.92$ and $r_S = +0.85$; and all samples pooled $r_P = +0.71$ and $r_S = +0.62$. These figures confirm the poor correlation between E.T.R. and F.T.I. in normal individuals reported by Wellby *et al.* Whatever statistical method is used they also support the statement that the relatively high proportion of euthyroid subjects in the series of patients studied by Dr. D'Haene and his colleagues may explain a considerable part of the observed difference between the "pooled" r values in the two investigations.

On the other hand, I disagree with this approach to the important problem of selecting the most appropriate thyroid function test. Correlation coefficients merely express the degree of relative correspondence. The article by Dr. D'Haene and his colleagues leaves the impression that the F.T.I. is considered the better parameter because of the imperfect correlation between E.T.R. and F.T.I. This conclusion is subject to criticism as the F.T.I. is not the perfect thyroid function test. The possibility exists that the dissimilarity discovered by the correlation analysis is in favour of E.T.R. in terms of helping the clinician to establish the thyroid status of his patients.—I am, etc.,

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¹ Wellby, M. L., O'Halloran, M. W., and Marshall, J., *Clinica Chimica Acta*, 1973, **45**, 255.

Thrombolytic Therapy in Haemolytic-uraemic Syndrome

SIR,—Some of the statements made by Dr. J. Stuart and others in their article on thrombolytic therapy in the haemolytic-uraemic syndrome, (27 July, p. 217) require