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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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