latter. Practical retraining is an accepted form of behaviour therapy, used for many years, notably by Gwynne Jones; it falls within the definition of behaviour therapy adopted in a recent book on the subject, and the method is included as such in that book (*Experiments in Behaviour Therapy*, edited by H. J. Eysenck, 1964, pages 1 and 57).

Behaviour therapy can be expected to help only in certain disorders. The task ahead is to identify these, while realizing that behaviour therapy is but one part of general psychiatric treatment, which it cannot be expected to replace.

I. M. Marks.
M. G. Gelder.

Institute of Psychiatry,
The Maudsley Hospital,
London, S.E.5.

THE BI-ACROMIAL DIAMETER OF WOMEN

Dear Sir,

Dr. F. D. Kelsey (*Journal*, December 1965, page 1162) draws attention to the variation in the bi-acromial diameter of women, as documented by different observers. The Board of Trade figures (1957) were lowest—35·0, while in his own series the mean was 35·3, S.D. 1·65, the patients being older on average than those measured by Coppen and Rey (1959), whose means were higher still—35·9, S.D. 1·2. He postulates a slight decline with age, and warns of sources of error in measurement due to the degree of tension or relaxation of the subject. Having recently measured 100 controls taken from the general population, it may be of interest to communicate my own figures, which to my surprise were higher than any of the above. The women, aged 18 to 45, had a mean B.A.D. of 36·8, S.D. 2·9. Two measurements were made in each subject by means of a Harpenden anthropometer. It is not plausible that regional factors are entirely responsible for the markedly broader shoulder girths of my Mancunian subjects, and the most likely explanation would appear to be individual variability in the method of measuring. The subject's stance, her state of muscle tone and the degree of pressure applied by the operator must all be taken into consideration. It is probably unwise to regard the figures of different authors as comparable, nor should any one series be accepted as the norm. But rather should the present practice continue of each investigator providing his own control sample.

Susan Shafar.

Psychiatric Day Department,
Crumpsall Hospital,
Manchester, 8.

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SLEEP PATTERNS AND REACTIVE AND ENDOGENOUS DEPRESSIONS

Dear Sir,

In reply to the letters of Kay and Garside, I apologize for what appears to have been my misunderstanding of Kiloh's (1963) personal communication. It would seem, then, that the most reasonable conclusions we can reach at the present time are that (1) Objective recordings and observations do not provide evidence supporting the clinical position concerning sleep patterns. (2) The evidence obtained concerning patients' *reports* about their sleep is conflicting. It is of interest to note that the loading of the clinical feature "early awakening" on the bipolar factor reactive *vs.* endogenous depression is only 0·227 in the most recent Newcastle study (Carney, Roth, and Garside, 1965), compared with the loading of 0·692 in the earlier study (Kiloh and Garside, 1963), the correlations of the feature with diagnosis in the two studies being 0·271 and 0·831 respectively. In the more recent Newcastle study neither "early awakening" nor "initial insomnia" are included in the ten clinical features selected as giving the best prediction of diagnosis of type of depression. The findings of Mendels (1965) also give no support to the suggested diagnostic validity of sleep patterns.

I am happy to see that Garside has now quoted us (Costello and Selby, 1965) correctly, and I really do not see how a statement that findings suggest no difference between reactive and endogenous depressions is the same as claiming on the basis of negative findings that the null hypothesis has been confirmed.

C. G. Costello.

University of Alberta,
Calgary.

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[This correspondence is now closed.—Ed.]