SALIVARY LITHIUM

DEAR SIR,

Several recent articles have emphasized both the usefulness and the possible pitfalls of use of salivary Li levels as a substitute for plasma Li levels in the monitoring of Li therapy (Ravenscroft *et al*, 1978; Verghese *et al*, 1977). One important possible problem with salivary Li monitoring is the question of whether anticholinergic drugs added to Li therapy might affect salivary secretion and thereby change the apparent saliva Li/plasma Li ratio. We studied saliva and plasma Li concentrations in 28 patients receiving Li therapy alone on 41 different samplings, and in 11 patients receiving Li plus a drug with anticholinergic properties (trihexyphenidyl, at least 5 mg, chlorpromazine or thioridazine, at least 100 mg, or tricyclic antidepressants, at least 100 mg) on 15 different occasions. The mean ratio was slightly but not significantly higher in patients on Li therapy alone ($2.2 \pm .5$, $x \pm SD$) versus patients receiving Li plus an anticholinergic ($1.9 \pm .5$, $x \pm SD$). Thus anticholinergic supplementation is probably of only small concern to the clinician following patient saliva Li levels.

The ultimate question, however, regarding saliva Li levels is not whether they accurately predict plasma levels. The goal is prediction of clinical response and prediction of toxicity. Just as some studies have claimed that intra-erythrocyte levels reflect brain Li concentration better than do plasma levels, salivary Li may represent post-membrane Li concentration and may conceivably *better* guide the clinician than plasma levels. We really need a controlled study of Li therapy with one group adjusted on the basis of saliva Li and the other group on the basis of plasma Li. The groups should be compared for clinical outcome and incidence of toxicity.

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GHQ AND PSYCHIATRIC CASE

DEAR SIR,

I apologise for my delay in commenting on the recent controversy in your columns about the validity of the General Health Questionnaire (*Journal*, August 1978, **132,** 191), and hope that my present temporary address will be accepted in extenuation. Corser and Philip's original article (*Journal*, February 1978, **132,** 172) came to some non-controversial conclusions about the GHQ: namely that a high score does not necessarily indicate psychiatric illness or *vice versa*, and that being in a state of illness at one point in time does not mean that an individual will continue in that state.

There are two separate uses of a GHQ score, and the authors do not appear to distinguish between them. If it is required to discover information about an individual patient in a consulting setting, then the use of a screening test will be to alert the clinician to the possible existence of a psychiatric illness in that patient. Establishing that the patient is a case and attaching a diagnostic label are left to the clinician. The individuals described as 'true positives' in the various validity studies of the GHQ have *diagnosable* psychiatric disorders.

On the other hand, the GHQ scores of a group of respondents may be used to study the covariation between disturbance and other variables within that group, or to compare the amount of disturbance between two groups of patients. How well the screening test works in a given setting will depend upon the prevalence of illness in that population. In Dr Corser's study, 20 (16 per cent) of the 119 new arrivals to his practice had high scores, which means that one would predict a probable prevalence of psychiatric illness in that population of 16.7 per cent. At this prevalence, the proportion of those with high scores who are likely to be found to be cases at subsequent interview—usually called the 'hits-positive rate' of the test—will be 75 per cent. That is to say, there are likely to be 1 false positive for every 3 true positives. (The formulas for these conversions, and a fuller discussion of the validity of the GHQ, may be found in the *Manual of the General Health Questionnaire*, NFER, London, 1979).

In their subsequent letter the authors make the novel suggestion that many of the items of the GHQ are personality traits. Like Dr Philip himself, I was influenced by the work of Graham Foulds, and designed an instrument that measured deviations

from usual functions rather than long-standing traits. In Dr Foulds' terms, true positives with the GHQ are 'personally ill'. In my terms, they typically have mixed affective disorders, often with somatic symptoms, when seen in a primary care setting. Acute psychotic patients almost invariably have high scores, but some chronic psychotic states and manic patients may be missed.

Finally, in their original paper (p. 175) the authors wish to refute the suggestion that a psychiatric illness is an illness that should be referred to a psychiatrist. No psychiatrist would make such a suggestion. We could no more cope with all the psychiatric illnesses than dermatologists could cope with all the rashes, or paediatricians with all the sick children, in the community. The adjective 'psychiatric' connotes the area in which a patient experiences his symptoms, not the nature of the specialist to whom he should be referred.

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ANDROGENS IN SEXUAL DYSFUNCTIONS: A PLEA FOR CAUTION

DEAR SIR,

We consider that a strong caution should be issued against the possible temptations of widespread and perhaps indiscriminate use of androgens in sexual dysfunction clinics which may follow upon the publication by Carney *et al* (*Journal*, October 1978, **133**, 339–46). They state that no virilizing side effects were observed nor reported from their treatment with testosterone, but we consider there is insufficient information about the effect of repeated or prolonged courses of androgens upon women.

A further important issue is the possible effect of androgens upon the foetus. We note that the authors requested that adequate contraceptive measures should continue throughout the trial but gave no reasons for this request. We therefore assume that they did not discuss with the couples the possible effects of exogenous testosterone upon a foetus were the woman to become pregnant. The dosage of testosterone administered is unlikely to give rise to physical intersex states, but, in their review, Goy and Goldfoot (1976) do not rule out the possibility that the administration of androgens at certain critical periods of human intrauterine development may modify future sexual behaviour of the foetus.

Although we occasionally prescribe short courses of testosterone to women with persistently low sexual arousal, we (1) take great care to explain that the effect of androgens on the foetus is not yet established and (2) strongly advise effective contraception throughout the course. Were conception to occur during treatment we would consider recommending termination of pregnancy, although this situation has not yet arisen in our practice. Until more facts are available we hope other therapists will prescribe androgens to women of childbearing age with extreme caution.

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Reference

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MALIGNANCIES IN SCHIZOPHRENIC PATIENTS

DEAR SIR,

The clinical observation on lung carcinoma in schizophrenia made by Dr David Rice (*Journal*, January 1979, **134**, 128) has prompted us to write about some of the relevant preliminary findings of a study on mortality in psychiatric patients which has just been concluded and is at present being analysed.

The data for the study were collected from the records of patients who died in Prestwich Hospital during the 30 year period 1947–76. All the relevant information was collected from the Death Register and the case notes at the hospital. The cause of death was ascertained and in the case of deaths due to neoplasms this was confirmed in most cases either on the operating table or after autopsy. There was thus little chance of misdiagnosis. The psychiatric diagnosis was made by assessing the symptomatology as recorded in the case notes. Since some cases of neoplasms were admitted because of symptoms resulting from cerebral secondaries, only those patients who had been in-patients for a duration greater than 12 months are included in this report.

The table following gives the incidence of various types of malignancies seen in schizophrenic patients. The preliminary results showed no significant difference in the overall incidence of neoplasms in schizophrenia. However, a detailed analysis suggested that there was a significant excess of all types of gastro-intestinal tract neoplasms and a much lower incidence of lung carcinoma in schizophrenics