

tionship is recognized between such phenomena of habituation and habit extinction, but this cannot be considered to be absolute, and the difference may prove to be allied to the components of the orientation reaction (Sokolov, 8). There is also the shifting baseline to contend with, as Seager and Brown note, and while this can be adjusted during the session or removed instrumentally, Wilder's (9) work on the Law of Initial Values indicates that different degrees of anxiety might be involved. Since, for a constant stimulus, the response will be larger if the subject is close to his resting level than if he is in a state of anxious arousal, the sensitivity of the indicator is lowered when the patient is not relaxed. This obviously reinforces Seager and Brown's statement on the importance of the patient being fully relaxed for efficient monitoring and therapy. We do not seek to denigrate the use of skin resistance in monitoring, but to counteract the impression that it is a straightforward and reliable method of measuring anxiety, including the fluctuations produced in reciprocal inhibition. We have separately tried many indices and have found that in cases where the GSR was unfruitful the finger pulse volume (in two cases—DAB) and muscle tension (in seven cases—NK—measured by EEG electrodes on scalp and forearm) have proved helpful. Muscle tension is also, of course, a direct monitor of the therapeutic method, i.e. muscle relaxation. There appears also to be some promise in the waveform of respiration, though we are not aware of a satisfactory method of quantifying this effect.

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### DEPRESSION: A PHYLOGENETIC VIEW

DEAR SIR,

May I comment on the letter by Dr. Price (*Journal*, January, 1968, p. 119), in which he promotes a phylogenetic hypothesis of depression?

In principle I support him heartily in such an approach. However, I feel unhappy when observations based on animal behaviour are directly, or even indirectly, applied to man, unless, as in the case of Lange's hibernation hypothesis (1928), measurable variables can be compared.

Again, Dr. Price bases his argument on a fundamental dichotomy within the depression complex, while remaining quite ready to admit to a lack of unanimity on such a clinical division.

My own experience makes me support those who see reactive and endogenous depression as part of the same disease spectrum. Therefore, I incline to favour the more unitarian proposition (1965) in which clinical depression is equated phylogenetically with a "basic emotional state", postulated to be the normal mood-level of early man. In contemporary man, by contrast, normal mood could then be regarded as being one of relative elation, this having been superimposed on the primitive mood-control system by "evolution".

Clinical depression in such a framework can then be considered to be a reversion to the basic or primitive state.

Such a reaction might be triggered, as suggested elsewhere (1967), by stress on man's central physiological clock. This structure may respond by reverting to its inherent (? 25 hour per day) circadian rhythm, while at the same time releasing basic mood.

Like Dr. Price's, this hypothesis is open to corroboration by testing. In some respects the latter lends itself more easily to experimentation. Professor Jenner (*Journal*, December, 1967, p. 1447-1448) has already intimated that he might undertake the appropriate isolation experiment if a suitable subject became available.

I should add that this latter hypothesis does imply that depression has a detrimental connotation phylo-

genetically. This contrasts with the view propounded by Dr. Price.

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#### MENTAL SUBNORMALITY IN SCOTLAND

DEAR SIR,

Drs. Innes, Kidd and Ross have found in the North-East Region of Scotland a case rate for mental subnormality of 6.02 per 1,000 (*Journal*, January, 1968, p. 35).

Perhaps this low figure can be explained by their assumption that a great majority of the subnormal patients are known to the hospital services or Local Authorities.

This is true, but there is a considerable difference between a "great majority" and "all" cases when undertaking epidemiological inquiries.

The unusually high predominance of male patients is suggestive that perhaps special social factors may have to be taken into consideration.

It would, therefore, be of considerable interest if they could continue their useful study by contacting all the family doctors in their Region and inquiring from them how many subnormals they know of or look after.

Furthermore, as the accurate diagnosis in borderline cases during the first few years of life is notoriously difficult, it would be interesting to know what proportion of the "normal" children in the North-East Region of Scotland have been adequately screened by chromatographic and cytogenetic investigations.

Last, but not least, in this study the criteria of mental subnormality was "where it was registered by the appropriate care authority that the intelligence quotient of a patient has been ascertained to have been less than 70 on full intellectual development". I will not go into the implications of this definition because this subject is now a hardy perennial in the *Journal*, rather like the first cuckoo used to be in the correspondence columns of *The Times*. . . .

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#### INFANTILE PSYCHOSIS

DEAR SIR,

Professor Jacobides, in his letter (*Journal*, February, 1968, p. 244) concerning the study of infantile psychosis reported in this *Journal* (November, 1967, p. 1169) by Miss Lockyer and myself, has raised some important points which require an answer.

He suggested that there may have been non-measurable *qualitative* differences between the psychotic children and the control children which did not show up in the symptom comparisons we reported. Certainly, it is essential to search for qualitative as well as quantitative differences, although we would not agree that these are non-measurable. They may be difficult to measure, but if the qualitative elements can be communicated from one psychiatrist to another they should be susceptible to definition and to measurement. The inclusion of the characteristic of "autism" in addition to "abnormal relationships" as more generally defined was an attempt to get at possible qualitative differences, and indeed only 8 control children showed this characteristic. In 4 of the 8 children the "autism" was rated as "slight", and in only one child was autism present at the time of first attendance. Our failure to obtain a more complete differentiation of the groups on this item may have been due, at least in part, to unreliability in the psychiatric judgments. It should also be said that qualitative differences might have been apparent if we had chosen other items on which to compare the groups. Nevertheless it is difficult not to be impressed by the extent of the overlap between the groups when it is remembered that all children reported to have "psychotic traits" or "some psychotic features" had been excluded from the control group.

In this context, the role of brain damage in the control group may be important. We noted in our original paper that brain damage was a feature of many of the control cases (as would be expected from their low IQ), and in response to his request further details are now provided in Table I. Compared with the psychotic group, about the same proportion of the controls (25/63) showed *no* evidence of brain damage, but in a higher proportion of the controls (24/63 compared with 12/63 psychotics) there was a "strong likelihood" of brain damage.

As Professor Jacobides suggested, nearly all the control children with "autism" had evidence of brain damage ("strong likelihood" in 6 out of the 8 cases and "possible" in 1 case). Two of these 8 children were thought at follow-up to be probably psychotic although not so diagnosed when first seen. None of the 8 children had been thought to be "psychotic-defectives" (a diagnosis suggested by Professor