

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

KLAUS HEYMANN.

21 Ladbroke Grove,
London, W.11.

REFERENCES

- HEYMANN, K. (1965). "Some thoughts on the fundamental nature of depression." *Practitioner*, 194, 668-672.
 — (1967). "Depression and the physiological clock." *Practitioner*, 199, 224-227.
 LANGE, J. (1928). In Bumke's *Handbuch der Geisteskrankheiten*, Vol. 6, p. 215. Berlin.

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*. . . .

RUDOLPH PAYNE.

Little Plumstead Hospital,
nr. Norwich,
Norfolk.

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