report of the effectiveness of lithium in a case of hypersomnia accompanied with polyphagia occurring in an adolescent female (2, 3), I would expect the drug to be effective in typical Kleine-Levin syndrome, but this is yet to be confirmed.

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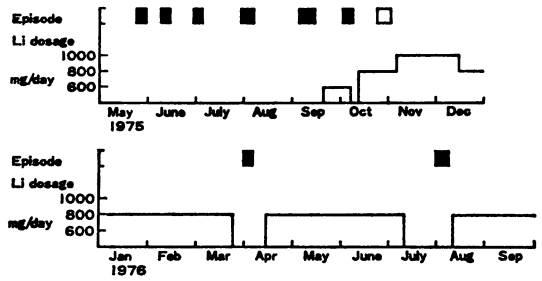

Fig 1—Hypersomniac episodes (shaded boxes) and lithium dosage. The unshaded box at the end of October 1975 refers to an episode of depersonalization.

PRENATAL PROGESTERONE AND EDUCATIONAL ATTAINMENTS

DEAR SIR,

The paper of Katherina Dalton (*Journal*, Nov. 1976, 129, 438–42) has received so much advance publicity, and presents such unexpected findings, that it deserves the closest scrutiny.

It essentially makes two claims: one that the effects of toxaemia in the mother and the intelligence of the child can be reversed by progesterone; the second, even more remarkable, that progesterone will increase the intelligence of the subsequent child above normal. Her results, however, do not warrant these conclusions.

The first claim would require that the mothers treated for toxaemia of pregnancy by progesterone were compared with an identical group treated with placebo injections. These conditions appear to have been satisfied with the first study reported in the *Journal* in 1968 ('Antenatal progesterone and intelligence', 114, 1377–82), but the only statistically significant result ($P = <\cdot05$) was that the progesterone children were more frequently walking at six months. The current study, however, gives insufficient information to support the first claim, let alone the second, largely because insufficient information is given about the control group. Progesterone mothers are likely to have been an unusual group to have opted for progesterone injections in the 1950s, 11 of them within the first trimester. It is not altogether fanciful to assume that they were both more open-minded and more concerned about the future health of their children than the controls, who were picked at random from obstetric wards or the General Practice Register. Not only is no evidence cited for the equivalent intensity of toxaemia for the toxaemic controls, but no comparison is made about any of the controls and the progesterone mothers, except to say that they belonged to classes 3–5.

Having, however, selected what one must hope are comparable controls, Dr Dalton uses the chi-squared test which distinguishes the groups qualitatively rather than quantitatively, but she does not quote the cut-off point used to divide the groups. More importantly, she assesses the three groups together, thus allowing the generally greater difference between the toxaemic controls and the progesterone group to obscure the significance, or lack of it, between the normal controls and the progesterone mothers. (To be fair, the only statistics where figures are provided 'Entrance to university'—actually distinguishes the progesterone group from the normals more significantly, $\chi^2 = 9\cdot53$ and $P = <\cdot01$). Under these circumstances, the presence of two more controls than can be accounted for by the double matching of four is merely a quibble.

It is all too easy to destroy exciting findings by over-zealous criticism. Nevertheless, it is sad that such an interesting paper should have been published in its present form, and we hope Dr Dalton will furnish us with sufficient details to confirm her remarkable claims.

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DEAR SIR,

In her article, Dr Katharina Dalton states that 'progesterone given to the mother (antenatally) not