Finally, it should be recorded that the authors' undocumented quotation of my results in the Baganda tribe must have been derived from information obtained while the work was still in progress, for when a sufficiently large number (758) of Baganda children had been examined a significant difference was recorded.

It is to be emphasized that field studies of gross sickling and *falciparum* rates are of little importance unless they extend over several generations, or express in addition the differential mortality from malaria. The justification for the use of parasite counts, which I consider essential in this work, is that they give a measure of the individual's resistance to malaria, and by implication a measure of what mortality may be expected from malaria in sicklers and non-sicklers respectively.—I am, etc.,

Kampala, Uganda.

ALAN B. RAPER.

REFERENCES

- ¹ *British Medical Journal*, 1954, 1, 290.
- ² *Trans. roy. Soc. trop. Med. Hyg.*, 1954, 48, 312.
- ³ *The Distribution of the Human Blood Groups*, 1954. Blackwell, Oxford.
- ⁴ Lehmann, H. (1954). *Eugen. Rev.*, 46, 101.
- ⁵ Foy, H., et al. (1954). *British Medical Journal*, 1, 294.
- ⁶ *East Afr. med. J.*, 1946, 23, 75.

Deprived Children

SIR,—I think there may be a danger that the data which Dr. Hilda Lewis presents in her book on *Deprived Children* may be so used that it results in misplaced complacency. For instance, your annotation (*Journal*, October 30, p. 1039) in which you write, "Dr. Lewis through her follow-up study is able to show that there may not be full justification for the very pessimistic views of such workers as Bender, Lowrey, Goldfarb, and Bowlby on the effect of separation from the mother in the early years," may lead some of your readers to conclude that those of us who have stressed the grave dangers of separation have been crying "wolf." In fact, Dr. Lewis's data provide yet further evidence that separation (and also other forms of maternal deprivation and rejection) give rise to serious psychiatric disturbance and that separation is especially frequent as an antecedent in the most serious cases. Of the 19 affectionless characters in her series, nine had experienced it (four for between three months and two years, and five for more than two years).

What Dr. Lewis's findings make plain is that, though many children who experience separation suffer damage, many others do not become seriously disturbed. This is of much scientific interest but of no more importance to the practice of preventive health than the recognition that many children who drink tuberculous milk come to no great harm.—I am, etc.,

London, W.1

JOHN BOWLBY.

REFERENCE

- ¹ *Deprived Children*, 1954. London.

Prevention of Colds

SIR,—The paper by Drs. O. M. Lidwell and R. E. O. Williams (*Journal*, October 23, p. 959) on their trial of hexylresorcinol raises a number of points of interest. Their bacteriological findings appear to confirm the chemical estimation of the aerosol concentration in the air, but these do not tally with the estimated content calculated from the "aerovap" output (Table IV). What can be the explanation for the apparent loss of five-sixths of the bactericide? That there was a constant source of depletion is further borne out by the failure to record a decrease of sick-absence rate, whereas Dickson¹ found a definite decrease in the rate and duration of sick-absence over a four-year period. As a result the aerosol installation was extended to cover "every room of the two largest Departments of State" (411 rooms).

Mackay,² whom Drs. Lidwell and Williams do not mention, using 6 to 7 $\mu\text{g.}/\text{cubic ft.}$ (to convert $\mu\text{g.}/\text{cubic ft.}$ to $\mu\text{g.}/\text{cubic m.}$ multiply by 35.3) of air, estimated on the normal "aerovap" output of hexylresorcinol, reduced the bacterial content of artificially infected air from 10^6 organisms/cubic ft. to <10 /cubic ft. in 35 minutes when *S. marcescens* was the infecting agent. The experiments were repeated on a number of occasions in a room two-thirds the size of the

Newcastle offices. In neither Dickson's nor Mackay's tests can there have been any substantial difference between the actual and calculated aerosol concentrations. It would seem that there must be some hitherto unencountered factor which has influenced the Newcastle experiments, since an aerosol increase of 6 to 7 times normal was necessary before a substantial bactericidal effect was recorded, under which circumstances the concentration was reputed to produce throat irritation although Mackay's 6 to 7 $\mu\text{g.}/\text{cubic ft.}$ was reported to produce only "a slight feeling of dryness at the back of the throat" detectable by "only the more sensitive observers." In our own experience an output of ten times normal giving a mist very obvious in a Tyndall beam in a dark room has not given rise to any complaints.

There is an unexplained discrepancy in the results recorded in Table V, wherein hexylresorcinol at 1.3 $\mu\text{g.}/\text{cubic ft.}$ gives higher die-away rates of streptococci than the 4.3 $\mu\text{g.}/\text{cubic ft.}$ concentration. Thus explanations have to be sought for: (1) the enormous loss of aerosol, which presumably was not due to excessive ventilation; (2) if the complaint of irritation due to over-concentration is upheld the failure of such an aerosol to show direct evidence of bactericidal powers; and (3) the failure of the chemical tests to detect that amount of hexylresorcinol. It is to be hoped that Drs. Lidwell and Williams can advance reasons for their results being at variance with the findings of other workers.—I am, etc.,

Goring-on-Thames.

A. HOWARD BAKER.

REFERENCES

- ¹ *Irish J. med. Sci.*, 1953, 6S, 337.
- ² *J. Hyg. (Camb.)*, 1952, 50, 82.

Racial Difference in Blood Constituents

SIR,—The interesting paper concerning haemoglobin levels in European and Asiatic populations by Dr. G. R. Wadsworth (*Journal*, October 16, p. 910), and an earlier paper by Wadsworth and Oliveiro¹ on total plasma protein concentration, prompts me to add data collected during the last war on 201 British and 217 Indian soldiers stationed in North India (Dehra Dun). The British soldiers had been in India at least six months: the Indian soldiers were drawn from various parts of the country. All men were on normal duties. The diet for both groups was adequate in calories and total protein, but in the Indian group only about a quarter of the protein was of animal origin. Blood samples were taken in the morning over the period March to October. Haemoglobin, erythrocyte sedimentation rate, and plasma protein concentration were measured on all samples. Details of the methods are given elsewhere.²

In the case of Hb (g./100 ml.) the results were as follows: British group, mean = 16.61 ± 0.07 , S.D. = ± 1.08 ; Indian group, mean = 15.67 ± 0.13 , S.D. = ± 1.93 . The distribution of the British group was normal, but that of the Indian group was skewed to the left with a pronounced tail between 12.6 and 8.4 g./100 ml. A *t* test showed the means to be significantly different ($P < 0.1\%$). However, the modes of the distributions were very similar (British 16.61; Indian 16.42 g./100 ml.), and when Hb levels below 13.0 g. were eliminated from the Indian sample the distribution became almost identical. It thus appeared that the difference between groups was due mainly to the presence of mild anaemia in one of them, and the significant difference was apparent only. A racial difference was hence unlikely. This would correspond to Dr. Wadsworth's findings and those of other workers.³ Variance analysis showed a significant month to month variation for both groups (British, $P < 0.1\%$; Indian, $P < 1.0\%$) with higher levels in the hotter months of May to July.

The results for the E.S.R. (mm./hour) showed the following: British group, mean = 7.37, S.D. = ± 5.60 ; Indian group, mean = 7.27, S.D. = ± 7.14 . Both distributions were markedly skewed to the left and a *t* test considered not valid. A χ^2 comparison demonstrated that for arbitrary class ranges there was no significant difference in E.S.R. between the groups. Variance analysis showed absence of month to month variation, but other data⁴ suggested that a seasonal variation does exist in some individuals, with higher levels in the hot season. The means and S.D. for both groups are appreciably higher than those accepted for temperate climates and correspond in general with those of Khanna and Sachdev⁴ for Indian males. In the case of the British soldiers there was no evidence for subclinical infection, and one may

perhaps assume that the higher figures are due to climatic factors. Similar suggestions were put forward a number of years ago by Sundstroem⁵ and Frimodt-Møller and Benjamin.⁶ In connexion with possible racial differences in E.S.R. the work of Muzyka and Lille⁷ on the correlation between E.S.R. and the blood groups is of interest. A number of papers have been published on E.S.R. in non-European groups,⁸⁻¹³ but European controls have not always been used.

Plasma specific gravity figures were converted into total plasma protein using the formula of Hoch and Marrack.¹⁴ The main results were as follows: British group, mean = 7.07 ± 0.03 , S.D. = ± 0.44 ; Indian group, mean = 7.50 ± 0.03 , S.D. = ± 0.46 . The means were significantly different ($P < 0.1\%$). Variance analysis demonstrated a significant month to month variation for both groups (both gave $P < 0.1\%$), and for each month the Indian means were higher than the British. Where *t* tests were done on monthly means, significance was found at $P < 0.1\%$. The incidence of subclinical infection (hookworm, etc.) and the intake of vegetable protein was higher in the Indian group, and it is possible that both conditions may increase plasma globulin and total plasma protein concentration. It is hence not valid to assume that the significant difference in plasma protein levels between the groups is in fact of racial origin. The present findings correspond to those of Wills and Bell,¹⁵ but Wadsworth and Oliveira¹ found no significant difference between various Asiatic and European populations. The literature is covered by the latter workers, but further data have been recently published for Africa^{16 17} and the Dutch East Indies.¹⁸ Little correlation was found in the data presented here between Hb, E.S.R., and plasma protein. It may be of interest to add that no significant difference was found in oral temperature between the two groups. This confirms previous work.¹⁹

Where groups of individuals are being compared, it is not sufficiently realized that any particular type of measurement may be affected by a number of factors such as time (diurnal, menstrual, seasonal, growth and ageing), sex, nutrition, state of activity and immunity, altitude, weather and climate, and so forth. Such variations are not always controlled or even specified. The interpretation of a significant difference (derived from crude data) between the characters of racial groups is hence not to be lightly attempted. The magic of the significance test may sometimes be a pitfall for the unwary.—I am, etc.,

Frimley, Surrey.

E. T. RENBOURN.

REFERENCES

- ¹ *British Medical Journal*, 1953, 2, 1138.
- ² Renbourn, E. T. (1947). *J. Hyg. (Lond.)*, 45, 455.
- ³ Gil, J. R., and Terán, D. G. (1948). *Blood*, 3, 660.
- ⁴ *Indian med. Gaz.*, 1946, 81, 296.
- ⁵ *Physiol. Rev.*, 1927, 7, 320.
- ⁶ *Tubercle (Lond.)*, 1927, 9, 57.
- ⁷ *C.R. Soc. Biol. (Paris)*, 1933, 113, 1295.
- ⁸ Roxas, B., and Baens, S. (1931). *J. Philipp. med. Ass.*, 11, 393.
- ⁹ Kitamura, K. (1939). *J. Chosen med. Ass.*, 29, 141.
- ¹⁰ Lassman, P. (1939). *Ann. Soc. belge méd. Trop.*, 19, 557.
- ¹¹ Cea, L. (1940). *Tratado de Haematologia Clinica*, Salvador.
- ¹² Brouwer, D. (1940). *Geneesk. T. Ned.-Ind.*, 80, 2184.
- ¹³ Hollins, C., and Lewis-Fanning, E. (1947). *Trans. roy. Soc. trop. Med. Hyg.*, 41, 217.
- ¹⁴ *British Medical Journal*, 1945, 2, 151.
- ¹⁵ *Lancet*, 1951, 1, 820.
- ¹⁶ Sicé, A., and Bonnet, P. (1936). *Marseille méd.*, 2, 707.
- ¹⁷ Courdurier, J., and Brygoo, E. (1947). *Méd. trop.*, 7, 254.
- ¹⁸ Streef, G. M., and Streef-Spaan, A. M. (1938). *Geneesk. T. Ned.-Ind.*, 78, 2816.
- ¹⁹ Renbourn, E. T. (1946). *British Medical Journal*, 1, 909.

Foetal Defects in Early Pregnancy

SIR,—Dr. Ian G. Wickes's valuable paper on "Foetal Defects Following Insulin Coma in Early Pregnancy" (*Journal*, October 30, p. 1029) throws a great deal of light on the causation of these abnormalities and, I think, supports a theory which I put forward 20 years ago. I suggested in a paper called "Cephalic Dysostosis," which was based on a study of hypertelorism, that the cranial deformities found in oxycephaly, hypertelorism, Crouzon's disease, and acrocephalosyndactyly were the result of some deleterious agent causing temporary cessation of cerebral growth in early intrauterine life. The temporary cessation of cerebral growth allows the bony sutures to unite in some places, and when the brain starts to grow again it forces the skull to expand where the sutures have not united. Thus the skull may be forced upwards and produce oxycephaly, or laterally and produce hypertelorism, and so on. At that time I could not suggest what the deleterious agent might be

and thought that it was probably toxic or infectious. However, Dr. Wickes's work now suggests that it may be some temporary interference with the endocrine mechanism, and no doubt where cranial dysostosis occurs spontaneously the endocrines are disordered by mild infections or even dietetic disturbances. It is to be hoped that Dr. Wickes's work will lead to the observation of similar cases and to the pathological examination of fetuses should the patients miscarry.—I am, etc.,

London, W.1.

CLIFFORD ALLEN.

REFERENCE

- ¹ *J. Neurol. Psychopath.*, 1934, 14, 332.

The M'Naghten Rules

SIR,—It seems to me that when discussing the M'Naghten Rules we should realize that there is possibly far more behind the scenes than may at first meet the eye. These rules represent a milestone in the evolution of justice in Great Britain. When they were formulated it is reasonable to suppose that the necessity was obvious, and we must give credit to the legal profession of that time for being just as anxious to do the right thing as they are to-day.

"Times have changed," of course, is a very popular cry nowadays, but perhaps it would be better to say that human nature has not changed, although values have. I am sure that Dr. Eliot Slater (*Journal*, September 25, p. 713) is not seeking to change human nature, only to reorientate its values by a more exact understanding. I think it would be better to view Dr. Slater's points in general and say that it represents a plea for another milestone. As his reasons for this plea he offers us an explanation which is the end-result of considerable observation and thought. The least we can do is give him credit for his line of approach. As these reasons involve observations on human nature and the patterns of reaction to certain environmental influences they naturally have to take into account free will, public opinion, and the like, which everyone likes to talk about but very few probe.

Dr. Slater represents the approach of a psychologist, and, as such, many like to put them all in a pigeon-hole labelled "Up in the clouds," etc. Surely such a sweeping statement is an example of the very things that one is up against. Is not the suggestion that one is absolved from responsibility by accepting such a doctrine an example of the pot calling the kettle black? What is more mechanistic than suggesting that such a revelation will lead to a chain reaction ending up with everyone going berserk? There seems to be a sore need nowadays for a philosophy that treats the individual as an individual and allows him to understand himself a little better. Naturally there are dangers, but, as always, they lie in the interpretation rather than the revelation. Naturally there will be opposition, but that opposition will come from those who feel, "Thank God I am not as other men are," and not from those who say, "There, but for the grace of God, go I."

I am sure that other general practitioners realize this concept of environment and "nature." Although it is extremely difficult to put into words, it is a good basis for prevention not only of illness but of other abnormal situations—I would even go so far as to say war itself.—I am, etc.,

Lancaster.

J. A. FARRER.

SIR,—Dr. Eliot Slater (*Journal*, September 25, p. 713) need not be greatly perturbed by the spate of adverse criticisms that have appeared in these columns recently. Few psychiatrists able and prepared to think courageously and boldly will disagree with him on his statement regarding determinism. To us such a statement is axiomatic. It is ineluctable. It transcends our present-day stunted concepts of crime, of punishment, of imputability, of responsibility. On the other hand, to attempt to take away the M'Naghten Rules from our judges and barristers would be to deprive them of a great deal. Outmoded and not a little ludicrous though these Rules are, they yet represent the sum