the bone marrow aplasia or to the immune deficiency. Only systematic studies with the M.L.R. test in these two states will answer the question.—We are, etc.,

M. SASPORTES
A. BERNARD
J. DAUSSET

Institut de Recherches sur les Maladies du Sang,
Hôpital Saint-Louis,
Paris

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Adrenal Failure in Bronchial Asthma

SIR,—In view of recent publications¹⁻³ and correspondence,^{4,5} including the report of a death,⁶ concerning the substitution of beclomethasone dipropionate by inhalation for systemic corticosteroid therapy in bronchial asthma, we wish to record the following case to emphasize that, even when the maintenance dose of prednisolone is reduced slowly, clinical manifestations of adrenocortical hypofunction may appear.

A man aged 43 started regular treatment with oral prednisolone for chronic asthma in 1958 and was stabilized on a maintenance dose of 7.5 mg daily. In December 1972 beclomethasone dipropionate 100 µg four times daily by inhalation was added to this treatment, and after two weeks the dose of prednisolone was reduced to 6 mg daily. The dose had to be temporarily increased to 20 mg daily a few weeks later because of a recurrence of asthma associated with an influenza illness. From March 1973 further reductions in dose of 1 mg per day were made at monthly intervals without any subsequent deterioration in the forced expiratory volume in one second (FEV₁), which remained in the range 1.3-1.6 l. In July, after the dose of prednisolone was reduced from 2 mg to 1 mg daily, he started to complain of malaise, weakness, lethargy, and pains in the back and lower limbs. When prednisolone was withdrawn a month later he felt much worse and was noted to have lost 5 kg in weight over a period of three months. His FEV₁ was unchanged at 1.6 l, the blood pressure was 120/80 mm Hg, and the serum electrolytes were normal. The morning basal plasma cortisol was 4 µg/100 ml and rose to only 12 µg/100 ml 30 minutes after 0.25 mg of tetracosactrin by intramuscular injection. Treatment was started with 7.5 mg of prednisolone daily along with 40 units of corticotrophin gel twice daily, and within 24 hours he was free of symptoms.

We regard as highly undesirable any technique for the substitution of beclomethasone dipropionate by inhalation for prednisolone by mouth which aims at a complete withdrawal of prednisolone within four days, as advocated by Brown and his colleagues.¹ These authors observed symptoms of adrenocortical insufficiency in 17 of their 37 steroid-dependent asthmatics, but in this unit, though over 100 asthmatics have been converted from systemic corticosteroid therapy to beclomethasone dipropionate by inhalation, the patient described above is the only example of clinically apparent adrenocortical insufficiency so far encountered. It is our practice to rely on a very gradual reduction in the dose of prednisolone (1 mg per day

each month) to allow time for restoration of normal hypothalamo-pituitary-adrenal function, in an effort to prevent the unpleasant and potentially avoidable symptoms of adrenocortical insufficiency, and its more serious consequences.

We do not perform tetracosactrin tests as a routine on patients receiving long-term corticosteroid therapy before starting conversion to beclomethasone dipropionate, as advocated by Dr. A. O. Robson.⁴ In a pilot survey of 20 patients on an average maintenance dose of 10 mg of prednisolone daily⁷ we found almost complete loss of plasma cortisol response to tetracosactrin in every case, and it thus seemed improbable that this test would help to discriminate between patients who were liable to develop adrenocortical insufficiency and those who were not. Furthermore, we doubt Dr. Robson's premise that a test of adrenocortical function is a useful means of assessing the response to stress of the whole hypothalamo-pituitary-adrenal axis.—We are, etc.,

N. J. COOKE
S. J. CAMERON
G. K. CROMPTON
I. W. B. GRANT

Respiratory Diseases Unit,
Northern General Hospital,
Edinburgh

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Problem Oriented Medical Record

SIR,—The comments by Dr. P. H. M. Carson (23 June, p. 713) concerning the problem oriented method of clinical recording should be answered, particularly since they come from someone who has worked as a colleague of Dr. L. Weed.

I accept the criticism that the problem oriented style of recording is not necessarily superior to the traditional format for most of the purposes which Dr. Carson mentions, but I would not dismiss quite as quickly as he does the possibility that clinicians and their patients might benefit from the automated handling of clinical data—and for this purpose a well structured record is highly desirable.

The main point on which I would take issue with your correspondent, however, if I understand both him and Dr. Weed correctly, is that what was intended by its designer to be the main advantage of the problem oriented record is seen by Dr. Carson as its major drawback. The potential value of the problem oriented method lies in the fact that those who use it can display their understanding of the cases which they are treating by the manner in which they structure their records. If data allocated to a problem are inconsistent or irrelevant the recorder can seek guidance if he recognizes the anomaly himself, or can be offered teaching if it is detected by a more experienced clinician.

Whether or not the problem oriented style of recording is an improvement on

traditional methods depends to a very great extent on whether or not advantage is taken of the opportunities which it offers for educational dialogue. If the method is looked on merely as a new format for clinical data, Dr. Carson is right—adoption of the method accelerates the tendency for recording to become a ritual rather than activity with clinical significance, and attention is directed away from the patient. If, on the other hand, the method is regarded as a way of exploring and improving the understanding of a patient's condition which is displayed by his closest medical attendant, the problem oriented style draws the medical record into the centre of clinical activities as a working document with considerable potential for improvement in patient care. In this regard, the method developed at the Walter and Eliza Hall Institute seems to have some advantages over the style proposed by Weed.¹

Proper use of the problem oriented method therefore necessitates the recognition of medical records as educational instruments, and this requires a revision of philosophies concerning clinical recording and of role perceptions by all concerned. A proportion of clinicians, particularly the more recently qualified, appear to find this re-evaluation relatively easy, but that the majority find it a punitive and possibly pointless process is attested by all those who have written of the difficulties of implementing problem oriented systems.

It seems naively optimistic to hope that the medical profession will feel thoroughly comfortable with the problem oriented record before 20 years or more have passed, and until that time we shall need men like Dr. Weed to sustain our efforts. It will also be useful to have men like Dr. Carson to make us examine our objectives.—I am, etc.,

B. SHERWOOD MATHER

Royal Children's Hospital,
Melbourne, Australia

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Short Boys

SIR,—It is curious that the subject dealt with in your leading article (4 August, p. 245) should have appeared under this title, and have received so little attention. Surely, the vast majority of these boys are normal late developers?

You do state that "slow growth of their child is a common reason for parents to seek medical advice." It has been said that common things commonly happen, but, without mentioning the common basis, fringe cases are offered treatment which for most would be alarming and unnecessary. Fortunately you quote the wise advice "wisdom suggests that the paediatrician stay his hand in the 'usual' case." This still holds good.

It has been shown¹ that severe emotional disturbance can retard growth and intellectual development, but perhaps the situation is put into better perspective by considering the matter as one of the normal development curve—late development as opposed to early development.

In my experience over 20 years with more than 9,000 boys and 5,000 girls between the ages of 15 and 18, rather over 10% of boys were physically late developers and about the same number early developers who were physically nearer adults. Among girls there

were fewer late and more early developers because at the chronological age of 15 girls are on the whole biologically more mature than boys. By late developers I mean that at 15 the child would pass for 14 or 13 or even 12 chronologically. The medical neglect of this problem is evident in one of the fallacies of the educational system, where raising the school leaving age regards only the chronological age and not the immensely important biological age, which has such a vital influence on attitudes at the watershed of puberty. Detention in unsuitable schools is particularly hard on the early developer, and leads to asocial behaviour.

There appears to be a strong genetic link in the stages of development. In the great majority of cases inquiry confirms that the early developing boy has a father who developed early, and the late developer likewise father and brother who developed late.

It is an axiom of paediatrics that if you have a disturbed child you treat the parents. If parents of "short boys" were more enlightened about their own development and the problems involved there would be even fewer emotional difficulties, thus further reducing the need for any medication of the unfortunate child. The problem of the early developer who then stands still and is overtaken by boys he could previously master at games would be similarly helped. The watershed period of adolescence is often particularly vulnerable, and requires much more careful study.—I am, etc.,

M. E. M. HERFORD

Farnham Royal,
Bucks

1 Widdowson. F. M., *Lancet*, 1951, 1, 1316.

Diseases in the Tropics

SIR,—The suggestion by Dr. W. Z. Conco (11 August, p. 331) that the rarity of appearance of ischaemic heart disease (I.H.D.) among Africans is a consequence of high pain threshold and failure of communication demands comment. In the interests of epidemiological accuracy I must submit that these factors cannot explain the huge discrepancy between the incidence of I.H.D. in Africans and those of European or Asian stock. The 1,600-bedded general hospital from which I write is fortunate in being able to treat, and compare the incidence of, disease in Indians and Africans (both rural and urban). Electrocardiograms and chest x-rays are done in all cardiac cases, at no cost to the patients. Necropsies are performed in all cases of unexplained death when consent is available. Indian and African patients are managed by the same group of physicians. It was in this setting that a survey of consecutive cardiac patients was made.¹ One of the conclusions reached was that "myocardial infarction was at least 15 times more common in the Indian than in the Zulu." An extract of relevant statistics gleaned from some 51,000 general admissions during 1971 and 1972 yields a corrected ratio of about 40 Indians to 1 African in cases classified as "arteriosclerotic heart disease including coronary disease."

I do not wish to minimize the well-known fallacies of hospital admission statistics, but I am convinced, from some 15 years' full-time experience in this field, that these figures indicate a real discrepancy and that the fac-

tors suggested by Dr. Conco contribute little to it. While Africans may have a higher pain threshold than Indians or whites I cannot believe that this is an important reason for the failure to diagnose I.H.D. If they reach hospital in large numbers with other heart disease (hypertensive, rheumatic pulmonary, and myopathic) and feel and describe the pains of pleurisy, pneumonia, pericarditis, rheumatic fever, meningitis, and acute abdominal conditions, why should they not do so with I.H.D.? A proportion of patients might die from I.H.D. before reaching hospital, but what of the 70 or 80% who should survive the first attack? Why should they, unlike other cardiac cases, selectively avoid hospital? Why should I.H.D. consistently escape detection in African post-mortem material? I.H.D. accounts for morbidity among white and Indian hospital staff, yet is not observed among the African staff.

This rarity of I.H.D. among Africans is universal and not confined to the rural people who make up a large proportion of the patients of this hospital.¹ It may be that with increasing urbanization the incidence of I.H.D. will increase² but this trend is not evident in this population yet. If Dr. Conco's postulates are valid, we should find more I.H.D. among the urbanized, sophisticated, English-speaking African population. This is not yet significantly apparent here.

Regarding communication, the language of a community will develop according to the needs it serves. If they have no words to express cardiac pain, it is reasonable to infer that such pain is a rare experience in that community. Lack of terminology does not seriously impede the diagnosis of countless other painful conditions. I agree that scientific terminology is lacking in Zulu, but this is not relevant to symptoms. The fact that a patient's language has no words for meningococcal meningitis does not prevent him from complaining of headache. Even in sophisticated European practice it would be most unusual for patients to proffer the complaints listed in Dr. Conco's table IV, the significance of which escapes me.

To my mind the evidence of true rarity of I.H.D. in the African is overwhelming. My concern is that epidemiological enthusiasm will be dampened by Dr. Conco's specious explanation and the opportunity for investigation of this important phenomenon will be reduced. Also, if it can be shown that his African practice produces a significant number of cases of I.H.D., then this community should be an important field for research, for that is an unusual pattern in Africans at present.—We are, etc.,

J. E. COSNETT

Edendale Hospital,
Pietermaritzburg,
Natal, South Africa

1 Cosnett, J. E., *British Heart Journal*, 1962, 24, 76.

2 Seftel, H. C., Kew, M. C., and Bersohn, I., *South African Medical Journal*, 1970, 44, 8.

Coronary Heart Disease in Africa

SIR,—I would like to make some comments on Dr. W. Z. Conco's paper "Diseases in the Tropics; a Confrontation in African Rural Practice" (11 August, p. 331), having recently returned from the Makerere Medical School in Uganda.

If the diagnosis of coronary heart disease

is made on clinical grounds alone it does indeed critically depend on a correct interpretation of the symptoms, and the doctor in a developing country may have very real problems. But in Dr. Conco's own words any assessment of the prevalence of coronary heart disease can only be based on "informed conjectures" if there is no E.C.G. evidence to back up the clinical impression, and if no necropsy material is available. At the Makerere Medical School there is now a very great deal of E.C.G. and necropsy material, and this material provides incontrovertible evidence for the infrequency of coronary heart disease. It is not claimed that it is totally absent; the claim is that it is much rarer than in European and Asian communities in the same countries. Moreover, in the indigenous peoples the ones at risk are those who have tended to adopt our mode of life and especially those who are members of our own profession.

Knowledge about the prevalence of coronary heart disease in any country in the world can only be obtained by analysing objective data. No doubt there are some sudden deaths in Africa due to coronary heart disease, just as there are in Britain. But the knowledge of the prevalence of coronary heart disease in Britain is based on sound objective criteria, and exactly the same criteria must be applied in the African continent. When such criteria are applied, the very striking differences in the prevalence of the disease are clearly seen and objectively acceptable, and do not depend on conjectures, nor do they depend on any more selection than is unavoidable.—I am, etc.,

J. R. BILLINGHURST

Romford Group of Hospitals,
Essex

Social Workers in Hospitals

SIR,—Dr. P. E. Baldry (15 September, p. 589) is a lucky man to possess the intuitive skills necessary to appreciate and handle the unspoken problems of hospital inpatients, but for the less fortunate may we support Mrs. Carole Smith's plea (25 August, p. 443) for more effective deployment of hospital social workers? Though basic personality is undoubtedly an essential prerequisite, we would suggest that the skills necessary to understand and help anxious or disturbed patients who may have added difficulties of communication are learned and not innate (psychiatrists are made and not born). A graduate degree in sociology or psychology may not equip a social worker entirely for such a task, but we believe it to be of greater value in this respect than the average medical student's education, which considers the patient's persona to be of negligible importance compared with his lump or his diabetes. Add to this the content of a hospital doctor's work which is—necessarily—unlikely either to leave him time or to make it a priority to stop and listen for half an hour to a querulous patient. If the social worker can provide such a service, then the medical profession should be grateful, not jealous, for not only does it free them to treat the illness (which is why most of them are in the business) but would encourage quicker rehabilitation of the patient.

Unless or until the medical school curriculum includes within its terms of