

meaningless unless the anticipated improvement is measured by audit of performance before and after implementation of the proposals. Indeed the recommendations themselves imply the need for "output" audit in "promotion of examination by the Royal College of General Practitioners" (though the appropriateness of audit by formal examination is questionable).

No system (medical, military, clerical, or legal) can progress rationally or improve efficiently without planning its new "inputs" on the basis of previous, adequately measured, "outputs": the basic tenet of cybernetics.

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THE FUTURE OF PSYCHIATRY

SIR,—In your editorial (April 26, p. 963) on my pamphlet, *The Future of Psychiatry*, there appear some very curious phrases which are not at all in line with reality. It is said, for instance, that "no candidate is likely to pass a higher qualification in psychiatry without a passing knowledge of the therapeutic applications of learning theory". Having run the psychology course for the D.P.M. and other higher qualifications in psychiatry at the Maudsley for some 30 years, and having acted as examiner for the latter for almost the same length of time, I feel I can speak with some authority in stating that this is just wishful thinking—unless "passing knowledge" is interpreted as synonymous with "having a nodding acquaintance with". Candidates certainly have only a very elementary knowledge of learning theory; indeed, as this is a very complex and difficult field, it is hard to see how the position could be different. I also doubt if "the statement that psychiatrists have no adequate knowledge of behaviour therapy is open to dispute". In making the statement I was obviously referring to psychiatrists having received the usual psychiatric training; there are some psychiatrists who have studied psychology before taking up medicine, and there are others (including those cited in your editorial, whose work in behaviour therapy is praised) who explicitly studied learning theory in an academic psychology department for a lengthy period. These people have learned psychology and learning theory properly, and not as part of their usual psychiatric training; they are irrelevant to the argument.

The piece ends by stating that "psychiatrists, despite Eysenck's disparaging remarks about their general level of ability, are quite capable of using behavioural techniques wisely and appropriately". Having never made disparaging remarks (or any other kind of remark) about psychiatrists' general level of ability, I am at a loss to understand the meaning of this phrase. My point was that the lengthy medical training of psychiatrists was irrelevant to the use of behavioural techniques, and that the proper use of these techniques required a background training which psychiatrists in the nature of the case could not possess (except in isolated and very unusual cases, as described above). Provided that they receive the appropriate training, which would add perhaps another year or two to their already overlong period of study, I have no doubt that many of them could advance to the stage of using these methods "wisely and appropriately". That they can do so without the requisite knowledge and training is equivalent to saying that a psychologist, having received a 30-hour course of introduction into medical treatment, could apply these methods wisely and appropriately. I beg to disagree.

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HYPONATRÆMIA AND DIURETICS

SIR,—Hyponatremia is, as Dr Roe (May 17, p. 1146) reported, not uncommon in "ill" patients, particularly if they are elderly, and regardless of whether they have been taking diuretics. Perhaps I might comment that his statement "If the 'sick cell' hypothesis is correct then it might be expected that many of the observed electrolyte disturbances would be corrected simply by treating the underlying disease" could with equally impeccable logic (and arguably more truth) be rephrased "If inappropriate secretion of A.D.H. were a common concomitant of any severe illness . . ."

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MECHANICAL FACTORS IN ATHEROSCLEROSIS

SIR,—It is certainly healthy that medical dogma be examined periodically, and to the extent that there may exist in some circles a dogmatic view that lipids are the sole aetiological factor in atherosclerosis, then Dr Stehbens' article¹ may have performed a useful service. However Dr Stehbens seems rather to be beating a straw man. Nobody has suggested that the lipid hypothesis explains everything, so it is gratuitous on Dr Stehbens' part to attribute such a narrow posture to his colleagues. Among a wide acquaintanceship with scientists who have worked on lipids in relation to atherosclerosis, I do not know a single one who is oblivious to the highly probable role of mechanical factors. I doubt that I have ever been to an atherosclerosis meeting which omitted acknowledgment of mechanical factors, and I have never heard or read a denial that they exist and are relevant. In referring at random to one published "symposium" on atherosclerosis, I find not only that it includes a paper on hypertension and atherosclerosis,² but also that two research groups who have devoted most of their own effort to studies of lipids make specific reference to haemodynamic factors and arterial injury.^{3,4}

It is certainly true that there has been more lipid research than haemodynamic research in regard to atherogenesis—with the very important exception of a large body of research on hypertension as an aetiological factor. The emphasis on lipids can scarcely be the result of a conspiracy within funding agencies, for the phenomenon seems to be international. More likely these agencies are receiving relatively few proposals for research on haemodynamic factors, or at least relatively few high-calibre proposals. Possibly the time is not yet technologically ripe for further exploration of these problems. Perhaps the time is ripe but the field has not attracted enough scientists with appropriate backgrounds. In any event Dr Stehbens seems to have misstated the issue in alleging there is a "long-neglected tissue factor influencing, if not governing, the accumulation of lipid in the blood-vessel wall" (italics mine).

While agreeing in a sense with Dr Stehbens' remark that "large-scale dietary intervention trials for the prevention of coronary heart-disease have been disappointing", I would emphasise that the disappointment is not so much in the results of these trials, which have been quite encouraging indeed, but rather in the very small number and limited size of well-controlled primary prevention studies.^{5,6}

1. Stehbens, W. E. *Lancet*, March 29, 1975, p. 724.
2. Freis, E. D. *Am. J. Med.* 1969, 735.
3. Getz, G. S. *ibid.* p. 657.
4. Frantz, I. D. *ibid.* p. 684.
5. Dayton, S., Pearce, M. L., Hashimoto, S., Dixon, W. J., Tomiyasu, U. American Heart Association Monograph no. 25. 1969.
6. Miettinen, M., Karvonen, M. J., Turpeinen, O., Elosuo, R., Paavilainen, E. *Lancet*, 1972, ii, 835.