Fainting during General Anaesthesia in Supine Dental Patients

SIR,—I have repeatedly suggested¹⁻³ that though an emotional faint can occur in the horizontal position fainting would not be a hazard to life if dental patients were anaesthetized lying down. There are, however, disturbing reports suggesting that even in this position should fainting happen to coincide with the administration of an intravenous agent such as methohexitone or diazepam (Valium) there may be a collapse so severe as to threaten life. In one reported case,⁴ for example, a boy aged 11 given only 20 mg of methohexitone instantly collapsed, was pulseless, and stopped breathing. And a similar collapse was reported⁵ in a youth given 7.5 mg of diazepam. His life was feared for at first and it was 15 minutes before consciousness returned. I have received reports of other dental cases in which there was a similar and most alarming collapse in the horizontal position with intravenous agents.

In the cases so far reported the collapse has been treated with the patient remaining horizontal. Sharpey-Schafer suggested⁶ that an emotional faint resulted from the pooling of blood in the splanchnic region. If this is so the logical treatment would be to place the entire patient in a steepish head-down tilt, which should improve the return of blood to the heart and so restore the blood pressure.—I am, etc.,

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- ¹ Bourne, J. G., *Lancet*, 1966, 1, 879.
- ² Bourne, J. G., *Anaesthesia*, 1970, 25, 473.
- ³ Bourne, J. G., *Lancet*, 1973, 1, 35.
- ⁴ Shafto, C. E., *British Journal of Anaesthesia*, 1969, 41, 407.
- ⁵ Verrill, P. J., *British Medical Journal*, 1970, 4, 348.
- ⁶ Sharpey-Schafer, E. P., *British Medical Journal*, 1956, 1, 506.

Who is the Dental Anaesthetist of the Future?

SIR,—No one can deny that a fully anaesthetized patient needs the services of a separate person specially devoted to safeguarding his welfare, but does this per se make anaesthesia safer? Deaths have occurred with an operator/anaesthetist in action and also with a separate anaesthetist of all three groups—dentist, general medical practitioner, and specialist anaesthetist. A statistical analysis to prove whether one group has more (or fewer) deaths than the others would serve no purpose other than to arouse emotional reaction. So long as man remains imperfect deaths under anaesthesia will continue to occur.

Surely the rational approach towards prevention of mishaps is to give more training both at undergraduate and postgraduate levels. The constructive plan of Professor P. A. Bramley and his colleagues (4 May, p. 270) seems to have fallen on stony ground. I would have expected a massive response in your columns from teaching institutions throughout the country, but a great silence so far is the only outcome.—I am, etc.,

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Biofeedback

SIR,—While I must commend the scientific caution with which your leading article on biofeedback (17 August, p. 427) approaches its subject, I would suggest not only that you failed to give proper recognition to the range and extent of the present literature on the subject in both experimental and clinical psychology but also that you did not inform your readers of the rigorous and careful experimental design which characterizes studies in this new and important field of behaviour science.

As you rightly state, it is highly dangerous to extrapolate from experimental work with curarized rats to a human clinical population. Psychologists have long experience of these inherent dangers and have already shown that it may not be possible to generalize from one particular strain of rats to another¹ let alone from one species to another. Yet generalization has to be attempted if experience gained in the psychological laboratory is to be of benefit to human populations. It can reasonably be argued that undue caution on the part of traditional elements in psychology and in medicine delayed the application of the laboratory-based principles of learning theory to psychiatric disorders in the form now recognized as behaviour therapy.

The question whether control of autonomic functions by conditioning is direct or is mediated is one which experimental psychologists such as Miller have obviously been concerned with from the very onset of biofeedback research, hence his use of curarization.² The evidence to date indicates that control is almost certainly direct rather than mediated, and indeed some studies go further by providing evidence of the probable physiological mechanisms underlying such control.³

When using any laboratory-based techniques with a human population it must always be considered to what extent the results obtained are attributable to the technique and to what extent they are due to placebo effects, anticipatory attitudes, expectation, and instruction.⁴ All too little attention has often been paid to these important human factors in medical research and, in particular, in drug studies. Considerable attention has, however, usually been paid to them in psychological research, including biofeedback.⁵⁻⁷ As with any clinical method, such factors are likely to play a part but there is certainly no indication that the results of biofeedback can largely be explained in terms of them. In my own research I was able to show a mean heart rate increase of 23 b.p.m. in five experimental subjects while five control subjects showed no change over baseline heart rates. Each group received exactly the same instruction and the same amount of reinforcement. The only difference between the two groups was that the control group received visual pseudobiofeedback (that is, feedback was derived from the subject's yoked control rather than from his own physiological responses).

The first paragraph of your leading article seems to imply a dichotomy between clinicians and psychologists. In reality, of course, there are psychologists who are primarily experimentalists and others who are primarily clinicians. The latter have been interested in the clinical use of biofeedback

for a number of years already.⁸ While scientific caution is always commendable, I would suggest that biofeedback is now established and founded on adequate experimental evidence from both animal and human research to the point that caution should not be permitted to hold back the benefits to patients that can be obtained from its clinical use now.—I am, etc.,

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- ¹ Broadhurst, P. L., and Levine, S., *British Journal of Psychology*, 1963, 54, 121.
- ² Miller, N. E., and Dicara, L., *Journal of Comparative and Physiological Psychology*, 1967, 63, 12.
- ³ Weiss, T., and Engel, B. T., in *Biofeedback. Behavioural Medicine*, ed. L. Birk. New York and London, Grune and Stratton, 1973.
- ⁴ Sternbach, R. A., *Psychophysiology*, 1964, 1, 67.
- ⁵ Wolf, S., *Journal of Clinical Investigation*, 1950, 29, 100.
- ⁶ Lasanga, L., et al., *American Journal of Medicine*, 1954, 16, 770.
- ⁷ Stroebel, C. F., and Clueck, B. C., in *Biofeedback. Behavioural Medicine*, ed. L. Birk. New York and London, Grune and Stratton, 1973.
- ⁸ MacPherson, E. L. R., *Behaviour Research and Therapy*, 1967, 5, 143.

Care for the Mentally Handicapped

SIR,—The addresses of Professors A. D. B. Clarke and J. Tizard at the B.M.A.'s Annual Scientific Meeting (27 July, p. 240) again highlight the profession's own confusion on the subject of its place in this field. Were the professors concerned with the efforts of psychology, sociology, education, or medicine, or did they assume, as so many do, that all have much the same contribution to make? Unfortunately this confusion has tarnished our image.

The fact that intelligence (however defined), and consequently its relative lack, is a sociological phenomenon and that a subnormal population (however defined) is homogeneous only in terms of intelligence renders such information almost totally irrelevant in terms of physical or psychological treatment needs. Subnormality of itself is not and never has been a medical problem. The involvement of medicine generally and psychiatry particularly in the field of subnormality is precisely that of the paediatrician who is not primarily concerned with the phenomenon of childhood as such but with the varieties of disease which accompany it. The psychiatrist (and the paediatrician) is involved in subnormality only to the extent which disease or pathology accompany it. Intelligence, like childhood, is important only in so far as it effects communication and the expression of disease and as it affects the choice of or the response to treatment, or both. Lack of intelligence of itself is untreatable in medicopsychiatric terms.

Dependence attributable to intellectual deficit alone is the concern of the sociopolitical agencies but not of medicine. Dependence attributable to primary or intercurrent physical or psychiatric pathology is the concern of the medical profession, since much of it is treatable. Once it is realized that most pathology occurs in the context of subnormality and is not causally related to it the medical profession can do much to lighten the burden of dependence.

The recognition of disease in the subnormal is complicated by the dual difficulties of communication and expression. As the intellectual deficit increases words become less valuable as communicators and are replaced by a variety of non-verbal motor behaviours, which may function as communicators but often merely as expressors highlighting the problems of failure of communication