

Effect of ECT on insulin

SIR: we are pleased that Williams *et al* (*Journal*, July 1992, 161, 94–98) have confirmed our finding (Berrios *et al*, 1986) that ECT causes a temporary elevation of plasma insulin, the more so for recent animal work (in which Dr Nutt also participated) seemed to show that this was not the case (Thiagarajan *et al*, 1988). However, contrary to what Dr Williams *et al* have reported, we found that 'the higher the ordinal position of ECT (in a given treatment course) the higher the peak of insulin at ten minutes' (p. 223). This discrepancy can only be resolved by further research.

At the time of our original work we had also: (a) compared subjects with and without anticholinergic premedication to ascertain whether vagal stimulation was involved, and ascertained that insulin was *not* elevated in subjects so premedicated, (b) found a similar elevation of insulin in patients with DSM-III major depression. (However, following the advice of the Editor that these findings might confuse the issue, we deleted them from the final version of the paper.)

The fact that a post-ECT elevation of insulin is found both in schizophrenia and in depression suggests that the response is unlikely to be specifically related to the neurobiology of depression.

BERRIOS, G. E., BULBENA, A., MARTINEZ, A. R., *et al* (1986) Significant rise in plasma insulin after electroconvulsive therapy. *Acta Psychiatrica Scandinavica*, 74, 222–224.

THIAGARAJAN, A. B., GLEITER, C. H. & NUTT, D. J. (1988) Electroconvulsive shock does not increase plasma insulin in rats. *Convulsive Therapy*, 4, 292–296.

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Reliability of DISCUS rating in individuals with learning disabilities

SIR: Jones (*Journal*, September 1991, 159, 441–442) drew attention to the lack of reliability of the DISCUS rating (Sprague *et al*, 1989) in the assessment of motor disorders, particularly of tardive dyskinesia in individuals with learning disabilities. We, therefore, undertook to examine the inter-rater reliability of DISCUS ratings in 29 in-patients with learning disabilities. Each individual was independently assessed by two raters. Inter-rater reliability was assessed using the method of Brennan & Silman (1992).

Pearson's product-moment correlation between scores was high ($t = +0.70$, $P < 0.001$). However,

there was a difference in mean scores obtained by each rater of +3.24 (s.d. 2.68, $t = 2.05$). This results in a 95% range for agreement between observers of –2.25 to 8.73. The 'true' value of the mean difference was considered as a measure of bias between observers and was obtained by calculating 95% confidence intervals. From the samples studied, the 95% confidence interval for the mean difference was found to be 1.36 to 5.12. As zero lay outside this interval it was concluded that significant bias existed between observers.

Studies purporting to demonstrate a relationship between tardive dyskinesia, medication, and other indices in this group of individuals must take into account difficulties in objective assessment and significant bias between observers.

BRENNAN, P. & SILMAN, A. (1992) Statistical methods of assessing observer variability in clinical measures. *British Medical Journal*, 304, 1491–1494.

SPRAGUE, R. L., KALACHNIK, J. E. & SHAW, K. W. (1989) Psychometric properties of the dyskinesia identification system: condensed user scale (DISCUS). *Mental Retardation*, 27, 141–148.

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Evaluation in mental health care

SIR: I am sure Freeman (*Journal*, July 1992, 161, 1–3) is not suggesting that evaluation is an impossibility in mental-health care. Everyone will accept that there are extensive difficulties in identifying appropriate methods for evaluation in psychiatry because of the nature of the problems presented by patients and the effect of the environment in which the services are provided. Surely, however, these problems are not unique to mental health as the same arguments could be put forward for the services to elderly people and children. What is needed, therefore, is an understanding of the complex problems associated with evaluation of mental-health services before pronouncements are made as it appears to be the case in Dr King's (1991) report.

Evaluation has become an essential part of the new National Health Service reforms and there is an increased expectation that the effectiveness and efficiency of health-care services is undertaken by professionals. Hence so much emphasis on medical audit, clinical audit, and research. Exciting opportunities are created as the result of the implementation of National Health Service/Community Care

Act 1990, and these should be utilised to undertake to examine the future provision of the mental health services. I believe that the clinicians who have an intimate and close understanding of the services are the most appropriate professionals to do this.

KING, D. (1991) *Moving on from Mental Hospitals to Community Care. A Case Study of Change in Exeter*. London: Nuffield Provincial Hospital Trust.

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Reporting predictable negative results

SIR: Korczyn *et al* (*Journal*, July 1992, 161, 132) report the negative results of their search for a mutation in the PRNP gene in six patients with bipolar affective illness. If I have understood their letter correctly, their justification for this search was based upon the following logical steps: mutations in this gene have been linked with a familial form of Creutzfeldt-Jakob disease; this has been demonstrated in Libyan Jews; some patients with Creutzfeldt-Jakob disease have affective symptoms; therefore, Libyan Jews with familial affective disorder should have the PRNP gene mutation.

Studies with negative results are of course scientifically as interesting as those with positive results and sometimes more so. This, however, is more likely when negative results are not as glaringly expected as in this case. The authors do not seem to have been discouraged by their findings and recommend that studies for mutant DNA sequences be performed in sporadic and familial forms of psychosis. I earnestly hope that they will not be testing for all known DNA mutations at random in psychotic patients. If they do, goodness knows how much more journal space will be filled with their negative results!

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Hypnotising lobsters, etc.

SIR: I was very surprised that the idea of hypnotising lobsters was thought to be evidence of gullibility requiring further photographic proof (Brooks, *Journal*, July 1992, 161, 134).

As a young child in rural Ireland I was taught to 'hypnotise' various animals by my mother. My particular expertise was in hypnotising turkeys and geese, for which I gained immense kudos as most of my peers were afraid of them. The technique involved stroking them firmly on the back of the neck, until the head rested on the ground at which point a white line was drawn in front of their heads. I often had dozens of them all over the yard, immobile until either they were moved or a loud noise disturbed them.

One recognised technique for hypnotising young children involves gentle, firm massage as this produces the relaxation and narrowing of attention required for induction.

My interest in hypnosis has continued although I confine my practice to people and my cat, Martha, when she requires calming at the vet's.

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Stress-induced hallucinations

SIR: Spivak *et al* (*Journal*, March 1992, 160, 412-414) describe stress-induced hallucinations, apparently experienced in clear consciousness, in army recruits who exhibited no other gross psychopathology. Cohen (*Journal*, July 1992, 161, 130) proposes that these experiences are most likely hypnagogic, a common and familiar source of isolated hallucinations. But I think that it may be a mistake to dismiss these unusual stress-induced experiences in this way.

In a recently completed series of retrospective interviews with 200 healthy subjects, 3% ($n=6$) described a single isolated experience of a brief second-person auditory hallucination, in which they heard a voice speaking to them, in clear consciousness. (No subjects in my series described stress-induced *visual* hallucinations, except in circumstances suggesting they were in a state of clouded consciousness.) These auditory hallucinations had occurred when the subjects had been faced with stress they regarded as 'severe', for example, life-threatening illness in their child. The words heard related to the concurrent stress. Most subjects described hearing comforting words, for example, "it's going to be all right", but some experienced threatening words such as "you're going to die". None of the subjects had experienced any associated psychopathology except distress in proportion to the perceived stress. None had consulted a psychiatrist around the time of the experience. The 'voices' were variously attributed to communication from God, to ghosts, or to