CORRESPONDENCE

AWARENESS DURING ENDOTRACHEAL INTUBATION

Sir,—Dr Dunnett (1977) suggests that awareness during intubation is less likely with his fast injection procedure than with the conventional titration method of McKenna and Wilton (1973). He may be right, although many would argue the other way. Theoretical arguments are worthless compared with direct testing, so it is a pity that Dr Dunnett has not done this. He reports zero incidence of awareness in 38 patients. However, no statistics are needed to see that this is to be expected even if his method gives exactly the same result as that of McKenna and Wilton, who found three cases in 160, that is about 1 : 50.

The $\chi^2$ test is clearly explained by Sykes and Vickers (1970). From this, Dr Dunnett has to show zero incidence in at least 355 patients to be significantly better than McKenna and Wilton. Comparing two proportions is the commonest evaluation needed in everyday medicine, and the programmable calculator makes it instantly available to everyone. Statisticians differ as to the best test with small frequencies, but $\chi^2$ has the merit of being a simple and acceptable approximation.

Meanwhile, there are no grounds for discarding the accepted view that crash induction has roughly 2% incidence of awareness. This is a small price to pay for the safety it gives in life-threatening situations. To make the patient unconscious is sometimes said to be the prime duty of the anaesthetist. It is not. Since the time of Hippocrates the prime duty of all doctors has been to do no harm, above all not to kill the patient. Despite this, patients occasionally die from anaesthesia; therefore, safety has to remain the paramount consideration.

Dr Dunnett is right to look for a method which abolishes awareness without compromising patient safety, and more research is needed on this. An alternative approach has been presented from Northwick Park Hospital (Cormack, Milledge and Hanning, 1977).

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Sir,—Professor Downing and his colleagues (1977) in their paper on the effects of doxapram on postoperative pulmonary complications claim that in the 34 subjects of their series who did not receive antibiotics, the incidence of pulmonary complications was significantly less following doxapram. This conclusion is based on a fallacious application of statistical methods. Tables V and VI of their paper employ $\chi^2$ tests based upon percentages of subjects. The $\chi^2$ test is critically dependent upon the real values of observed data, and cannot be applied to proportions in the manner of this paper. As an example, if all the values in a $2 \times 2$ contingency table are multiplied by a factor of $\eta$—changing their values but not their proportions—the calculated $\chi^2$ will be $\eta$ times the original value if Yates correction is not used, and rather more than $\eta$ times the true value if discontinuity is corrected. By my calculations, if the real values are substituted the tests of table V give a slightly higher value of $\chi^2$ which still does not approach significance. Table VI, concerning patients who did not have antibiotics, states a $\chi^2$ value of 23.1 ($P<10^{-5}$ at 1 d.f.). This reduces to $\chi^2 = 2.48$ ($P>0.1$) if true values are used. In practice, as the calculated expected incidence of complications is so low a $\chi^2$ test should not have been applied to these data.

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REFERENCE

Sir,—We are obliged to Professor Conway for pointing out the error in our statistical ways. However, the purpose of including table VI was to stimulate interest in the problem of the patient undergoing lower abdominal surgery without antibiotic cover. Application of the Fisher exact probability test to the results shown in table VI (Group I+II), gives a $P$ value of 0.054 (just not significant). A larger series might have revealed a statistically significant difference.

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REFERENCES