A measure of consciousness and memory during isoflurane administration: the coherent frequency

Sir,—We read with interest the study by Munglani and colleagues [1], which investigated the correlation between auditory evoked potentials and measures of cognitive function in subjects given isoflurane. We are not qualified to comment on the psychological aspects of the study, but are concerned by some technical details of the neurophysiological methods.

The authors stated that the output of the headphones was 65 dB greater than the average hearing threshold, at a stimulus rate of 6 Hz. We are not sure if the output was reduced as the stimulus frequency was increased. If the stimulating voltage was kept constant as the stimulating frequency increased, this would increase the delivered energy and produce an increase in the apparent loudness of the clicks. Is it possible that the effect that they are seeing could be caused, in part, by the change in stimulus energy (within the same sedation level) rather than being caused purely by the change in stimulus frequency?

Our second query deals with filtering of the EEG amplifier and the rate of digital sampling. Band pass filtering is defined normally by the 3-dB points; that is where the signal output has fallen to 0.707 of its original value. The Nyquist sampling criteria require that the sampling frequency must be greater than twice the highest frequency component in the signal being analysed [2]. If the authors' values of 1 Hz-2 kHz are the 3-dB points then they should have been sampling the signal at a much higher rate than 2 kHz, instead of at the 1-kHz sampling frequency that they used. An alternative would be to reduce the band width of the signal so that the signal above 500 Hz is negligible. This seems more logical, because mid-latency EP have a band width typically in the range 15–200 Hz. These comments assume greater importance if, as suggested by the text, there is no mechanism for artefact rejection. (The wide band width used in the study might increase the risk of the signal being distorted by EEG and other artefacts.) The undersampling described above could result in aliasing, which produces low frequency distortion.

We were confused initially by the terminology used in the description of data acquisition. We understand a “sample” to be a single point in time and a “sweep” to be a collection of data points which produces low frequency distortion. We interpret their term as being the excitation frequency which examines a wave of specific frequency (within the same sedation level) and not the fundamental and first harmonic. The terminology used in the section describing the derivation of the coherent frequency is unclear. In classical Fourier analysis, n = 1 indicates the fundamental and the first harmonic, that is they are the same [2]. Thus, the expression for a signal containing the three components: the fundamental and the second and third harmonics...

Finally, we are unconvinced by the term “coherent frequency”. We interpret their term as being the excitation frequency which elicits a sine wave response (i.e. the fundamental frequency only) instead of the complex waveform (i.e. the fundamental frequency plus many harmonics). We think that a term along the lines of “sine wave response excitation frequency” would be more descriptive.

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Sir,—Thank you for the opportunity of replying to Drs Tooley and Greenslade.

We agree that as the stimulus frequency increased, the delivered energy would increase, but this would be the same for each level of sedation studied and therefore our comparisons are valid. As far as the amplifier characteristics are concerned, there was a misprint concerning the band width which was actually 200 Hz. The reason for choosing this band width was that the coherent frequency was usually below 50 Hz; we only observed three harmonics, the third harmonic will be less than 150 Hz thus we set the cut-off frequency at 200 Hz. According to the Nyquist criteria, this would require a sampling frequency of preferably greater than 450 Hz. We chose 1 kHz as this was available on our system. We averaged 100 sweeps of EEG and we agree that the term fundamental and first harmonic are the same.

The term coherent frequency was used to differentiate the observed phenomena from resonance. Our analysis would describe resonance of a system if the stimulus elicited a response (usually a single harmonic at the same frequency) and with the greatest power. We found the output power of a fundamental was usually greater at frequencies other than the one we described as the coherent frequency (e.g. the well described 40-Hz steady state response) but these were complex responses containing high power at other harmonics. The coherent frequency was usually a single harmonic response indicating that the system (the CNS) was “hearing” in time with the stimulus (i.e. there was a constant phase relationship). The Oxford Dictionary definition of coherent is waves having a constant phase relationship. We are currently investigating the phase of relationship of this response and hope to publish data in the near future.

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Comparison of thermodilution, thoracic electrical bioimpedance and Doppler ultrasound cardiac output measurement

Sir,—We read the article by Castor and colleagues [1] and there are several points on which we wish to comment.

First, we feel that the statistical analysis was flawed. The Bland and Altman method [1] requires that each data point is independent of all others and the data must have a normal or near normal distribution. It is only relevant if the number of subjects studied is sufficiently large to provide a t value of approximately 2.0 at P = 0.05 (for 10 subjects, t = 2.3). The agreement between two methods depends on the magnitude of the limits of agreement (i.e. the smaller the limits of agreement, the better the agreement) and not on the magnitude of the mean difference of the two methods.

The latter is only to establish that there is no systematic bias between the two methods.

From figures 1–3, it was clear that there were more than 10 cardiac output (CO) measurements under each condition. These measurements were therefore not independent (which might explain why the data tended to cluster). The authors concluded incorrectly that agreement between thoracic electrical bioimpedance (TEB) and thermodilution (TD) was best during IPPV. In fact, as is clear from figures 1–3, agreement was worst during IPPV because the limits of agreement were actually widest. The best agreement was during spontaneous breathing when the limits of agreement were smallest.

Second, regarding TEB CO measurement, the authors stated that TEB underestimated CO compared with TD at smaller values of CO, whereas there was an underestimation at greater CO compared with TD during spontaneous breathing.” However, this was not the case from the figures shown. During spontaneous breathing, figure 3 shows a clear cluster of data at even greater CO (9–11 litre min⁻¹) when TEB was overestimating.

The authors have also ignored estimation of the “volume of electrically participating tissue” in TEB as a possible cause of inaccuracy. The calculation of this volume by TEB is based on assumptions made in healthy subjects [3] and may not apply in patients. Our previous study in critically ill patients has shown that TEB underestimates CO compared with TD [4]. This resulted from better thoracic conductance (equivalent to lower thoracic impedance) caused by tissue oedema in these patients. The improved conductance is equivalent to a larger cross-sectional area (hence volume) of tissue for electrical conduction. The impedance stroke volume formula does not take this into account. It is possible therefore that inaccurate estimation of the volume of...