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 recording of a single harmonic at a single stimulating frequency. If the authors mean that they sampled from the evoked response. Does the statement "… and averaged 100 samples of EEG at each stimulating frequency..." really mean 100 sweeps of EEG?

The terminology used in the section describing the derivation of the coherent frequency is unclear. In classical Fourier analysis, n indicates the fundamental and the first harmonic, that is they are the same [2]. Thus, the fundamental and the second harmonic..."

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 "beating" 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. The limits of agreement between two measurement methods (mean difference ± 2 SD) is only relevant if the number of subjects studied is sufficiently large to provide a 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°C 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 in patients. The improved conducitance 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
electrically participating tissue contributed to the inaccuracy of TEB CO.

The magnitude of TEB CO change as a result of alterations in electrode placement quoted by Castor and colleagues was not referenced. We feel that incorrect placement of electrodes is unlikely to cause major errors in the TEB method in practice. A previous study has shown that alteration of inter-electrode distance by 5 cm did not produce a significant difference in stroke volume estimation [5]. In practice, random error in electrode placement between operators is unlikely to exceed 5 cm.

Third, regarding Doppler CO measurement, we were intrigued that Castor and colleagues were able to obtain satisfactory Doppler signals from the Quantascope (Vital Science) during IPPV. In our experience, interference from the ventilator during IPPV prevents satisfactory Doppler signals from being obtained. The duration during apnoea was also too short, so that only one or two waveforms can be obtained in each respiratory cycle. We concluded that the Quantascope was technically unsuitable for CO measurement during IPPV. A previous study using the same equipment has also experienced similar technical difficulties [6]. This is in contrast with the experience of Castor and colleagues when they can average Doppler CO over a 12-s interval.

We disagree with the suggestion that the inaccuracy of the Doppler method was caused by error in estimating the aortic diameter. It is important to note that the purpose of the Quantascope omitting two concentric Doppler beams (wide and narrow) is to compensate for error caused by tissue attenuation and to eliminate the dual beam-to-beam interference and the angle of insonation of the beams during CO measurement [7, 8]. We feel that the most likely reason for the inaccuracy of the Quantascope was the technical difficulty in obtaining optimum power return of the Doppler beams from the ascending aorta. This may be related to suboptimal setting of the sampling depth or inability of the wide beam to insonate the whole aortic lumen. We also feel that the phenomenon of aliasing suggested by Castor and colleagues is unlikely to be an important cause for the inaccuracy of the Quantascope. The Quantascope uses a transducer of relatively low frequency (2 MHz) and aliasing would not have occurred unless either the sampling depth was exceedingly deep or the blood velocity was exceedingly high.

Studies such as that by Castor and colleagues are important in validating non-invasive CO measurement methods. However, it is important that data are analysed correctly and results interpreted appropriately.

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Retrobulbar block fails to prevent an increase in serum cortisol concentration on emergence from anaesthesia after cataract surgery

Sir,—Barker and colleagues [1] title their paper with a bold statement, suggesting that a retrobulbar block is superfluous if a general anaesthetic is given for cataract surgery.

Cataract surgery per se does not involve considerable tissue trauma and is not likely itself to evoke much of a stress response if appropriate anaesthesia is given. Their description is of a stress response occurring in all patients given a general anaesthetic whether or not retrobulbar block is incorporated. It would seem inconceivable that a retrobulbar block given in association with a general anaesthetic would not provide appropriate analgesia and another conclusion must be drawn.

The general anaesthetics were given to premedicated patients who first had a central venous catheter inserted via the antecubital fossa. General anaesthesia was induced with "a sleep dose" of thiopentone and tracheal intubation followed the use of vecuronium. No mention is made of the use of nitrous oxide, volatile agent or opioid and it is my experience that if this indeed was the general anaesthetic given, then a considerable increase in arterial pressure would occur. Intubation itself is a major stress, as is tracheal extubation.

Hett and co-workers [2] showed that patients who had pethidine premedication with nitrous oxide and a volatile agent still had an increase in arterial pressure on induction and this is a considerably more complete anaesthetic than that given by Barker and colleagues.

In conclusion, the inference that a retrobulbar block fails to prevent a stress response cannot be drawn from the published data. Almost certainly the increase in serum cortisol concentration in their patients was secondary to inadequate general anaesthesia. Local anaesthetic blocks for the eye are useful and do work well. They may be useful adjuncts to general anaesthesia, but this paper does not advance our knowledge.

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Sir,—Thank you for the opportunity to answer the letter from Dr Seddon commenting on our paper. We are sorry that he wrote without carefully reading the paper; he has completely missed the point.

We showed both in this paper and in previous work which was cited [1], that a metabolic and hormonal "stress" response occurs with cataract surgery conducted under general anaesthesia, even though, as Dr Seddon states, there is little tissue damage. We showed in this paper that the addition of a retrobulbar block prevented this response while surgery was taking place. Dr Seddon's comments show that he has not read as far as the methods section where we stated specifically the anaesthetic given, and another conclusion must be drawn.

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