Sir,—We appreciate the letter of Campriubi and Sabate which relates to our recent article on mucosal pH (pH₉) during orthotopic liver transplantation (OLT). Their preliminary data suggest that the use of venovenous bypass (VVB) during the anhepatic stage of OLT may indeed be beneficial for mucosal oxygenation: in contrast with our results, gastric pH, was preserved with VVB, but decreased transiently without VVB. As pointed out in our article, we were unable to strictly test the hypothesis that VVB would maintain mucosal blood flow as VVB was used routinely at our institution. Therefore, we cannot exclude the possibility that pH₉ values might have decreased to even lower values without VVB. The main purpose of our study was to assess the ability of tonometry to detect intraprocedural mucosal hypoxia, to measure gastric and sigmoid pH, and to relate the observed changes in pH, to the occurrence of endotoxemia and primary graft function.

Although in the preliminary report of Campriubi and colleagues, important information on patient status and determinants of tissue oxygenation is lacking, the fact that cardiac index, oxygen extraction and lactate concentration are comparable with the values measured in our study suggests that the different results for gastric pH, during the anhepatic stage may be attributed to the more severe chronic impairment of intestinal perfusion in our patients with end-stage cirrhosis. This is supported by the lower pH₉ values measured early during hepatectomy in our study (7.28 ± 0.18) compared with those of Barak et al. (pH, of 7.30 ± 0.09) (p < 0.01). In fact, low pH₉ values (pH < 7.20) have been reported in patients with chronic intestinal ischaemia and portal vein obstruction. In agreement with our data, the pH₉ values of Campriubi and colleagues’ patients without VVB who had returned to baseline after reperfusion. However, in our patients with end-stage hepatic cirrhosis, pH, did not reach normal values (pH > 7.32) before the second postoperative day, an observation that further supports the presence of chronic impairment of mucosal microcirculatory perfusion. Hence, pre-existing chronic mucosal hypoxia might explain why pH, decreased during the anhepatic stage, although overall portal blood flow was maintained by the use of VVB.

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Sir,—Clinical interaction studies evaluating the influence of different anaesthetic techniques on the action of neuromuscular blocking agents often compare two or more groups of patients treated separately but concurrently as part of the same study. These studies are termed “parallel” to emphasize their difference from other clinical studies in which patients are their own controls. Parallel comparisons include most of the usual forms of interaction studies investigating the abilities of different inhalation anaesthetics to potentiate the neuromuscular effects of neuromuscular blocking agents. However, the large between-patient variability in dose-response and dose-duration studies with neuromuscular blocking agents may preclude demonstration of subtle differences in susceptibility to these agents. Paired crossover studies can minimize the effect of inter-individual variability. However, these paired crossover studies are difficult to perform with surgical patients. Healthy ASA I or II patients that are anaesthetized two or more times, receiving on each occasion a different anaesthetic technique, are rare. Therefore, these paired crossover studies have to be performed with volunteers.

Our clinical study was designed as a parallel comparison of different anaesthetic techniques, that is opioid-nitrous oxide–oxygen, opioid–nitrous oxide–oxygen–isoflurane and opioid–nitrous oxide–oxygen–sevoflurane, in their ability to potentiate the neuromuscular effects of the most commonly used non-depolarizing neuromuscular blocking agents in surgical patients. Using an Kruskal–Wallis analysis of variance (ANOVA) and subsequently the Ryan–Einot–Gabriel–Welsch (REGW) multiple range test to identify eventual sources of difference, there was a significant difference in magnitude and duration of neuromuscular action between the control group, receiving no volatile agent, and the groups receiving inhalation anaesthetics. We were unable to demonstrate any significant differences in neuromuscular block produced by vecuronium, pancuronium, and atracurium. Using one-way analysis of variance (ANOVA) and subsequently the Bonferroni adjustment, we demonstrated any significant differences in neuromuscular block produced by vecuronium, pancuronium, and atracurium.
in the potentiating abilities of isoflurane and sevoflurane on the effects of neuromuscular blocking agents, that is a significant difference in both depth and duration, sample sizes of several hundreds of patients per neuromuscular blocking group would have been required. Such sample sizes are beyond practicable limits. We selected our population carefully, using rigid inclusion criteria, as described in the text, to reduce patient variability. This inevitably introduced constraints such as the availability of large numbers of eligible patients within a reasonable period of time. Any investigator is limited by both resources and time. Additionally, our study was intended as a phase II trial to evaluate the interaction between sevoflurane and neuromuscular blocking agents within a limited number of surgical patients.

Both the investigator and clinician should be concerned about the ability to detect an important clinical difference. Different investigators disagree on what is a clinically significant difference. They also disagree on the risk they are willing to take of missing a meaningful effect caused by drug interaction. The choice of a 20% difference in ED₅₀ as a clinically significant value is arbitrary. Such a difference is small compared with the large inter-individual differences in the response to neuromuscular blocking agents. With the degree of variability and sample sizes used in our study, a 30% reduction in the ED₅₀ of pancuronium and atracurium and a 45% reduction in the ED₅₀ of vecuronium had an 80% chance of being significant at the 0.05 level.

Finally, we still feel that further interaction studies on the influence of higher concentrations of volatile anaesthetics on larger doses of neuromuscular blocking agents are warranted. To elucidate small but significant differences in the ability of isoflurane and sevoflurane to potentiate the action of neuromuscular blocking agents, these interaction studies need to be performed in volunteers who are studied twice, receiving isoflurane on one occasion and sevoflurane on the other.

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Cusum: a statistical method to evaluate competence in practical procedures
Sir,—It was interesting to read the commentary by Kestin describing the use of cusum analysis to measure the competence of anaesthetic trainees at practical procedures. The cusum is a useful graphical tool for discerning trends from a series of observations. The derivation of boundary lines for sequential tests allows comparison of the observed proportions of success or failure against predetermined standard criteria. This could be developed into a continuous performance monitor in anaesthesia.

In his article, Kestin discussed the problems of keeping paper records and plotting fractions on the graphs. In response to this we set out to develop a computerized personal log book system for practical procedures to run in parallel with the electronic anaesthetic log book on the Psion 3a. However, in setting up the algorithms we have encountered a number of problems.

The values for s, h₀, and h₁ calculated using the formulae in the appendix do not agree with the values in table 1 of the article. On reviewing the original articles on the application of the cusum, the value of Q should read $Q = \ln \left( \frac{1-p_0}{1-p_1} \right)$. The values in the table are indeed correct if this calculation is used for Q.

The null hypothesis in the article is stated as “the true failure rate is NOT different from the acceptable failure rate” and if the cusum exceeds $h₁$ then it is rejected. This does not imply that the true failure rate exceeds the unacceptable failure rate which is the performance indicator which interests us. Similarly the alternative hypothesis is stated as “the true failure rate is equal to or exceeds the unacceptable failure rate” and if the cusum decreases to less than $h₀$ then it is accepted. Surely this would imply that the falling cusum of registrar B in figure 1 has a true failure rate that is equal to or exceeds the unacceptable failure rate.

$p₀$ corresponds to the failure rate under the null hypothesis which should surely read “the true failure rate is not different from the unacceptable failure rate”. If the cusum exceeds $h₁$ then this hypothesis is accepted and the trainee’s performance is unacceptable with reference to the agreed unacceptable failure rate. Similarly $p₁$ corresponds to the failure rate under the alternative hypothesis which should surely read “the true failure rate is equal to or exceeds the acceptable failure rate”. If the cusum decreases to less than $h₀$ then this hypothesis is rejected and the trainee’s performance is no worse than the accepted failure rate.

Furthermore, there appear to be unclear criteria in the definition of success or failure for a particular procedure. This is confusing and it is difficult to see why type 1 and 2 errors of 10% (0.1) were chosen.

For reasons of convenience for the plotting of the graph, the values of $s$, $h₀$, and $h₁$ where multiplied by 10. It is not clear if, in the event of a success (1-0) or failure (0-1), the criteria are stated as the incremented values.

The statistical method described by Kestin appears to be a very powerful analytical tool and may have a wide range of applications in the assessment of trainees. However, the errors and inconsistencies in the appendix are a source of confusion. It would be useful if the derivation of the definitions, variables, hypotheses and theory behind the calculations were explained more clearly.

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A. K. MCDONNELL
Royal Devon and Exeter NHS Trust

Sir,—I would like to thank Drs Hammond and McIndoe for their interest, and in particular for correcting the error in the formula for Q in the appendix–$p₀$ and $p₁$ have been transposed.

In answer to their other comments, the advantages of using $α$ and $β$ of equal magnitude is explained in the methods section; the choice of 0.1 was a compromise between the common values for $α$ of 0.05 and for $β$ of 0.1 or 0.2 used in clinical studies. The software used on the Psion 3a can be used to record and display the cusum; in this case, it is not as helpful to have $α$ and $β$ equal than if graphical methods are used. The graphs used by our trainees were plotted using the nearest integers to 10$α$ and 10$β$ as the increments.

The original article in the British Medical Journal referred to by Drs Hammond and McIndoe was the article that stimulated my interest in this topic. However, I found it difficult to understand the basic statistical concepts of cusum analysis from this article. I found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever found it even more difficult to write an explanation myself, and this article is the only one of mine which the reviewers have ever

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Although in the preliminary report of Camprubi and colleagues, important information on patient physical status and determinants of tissue oxygenation is lacking, the fact that cardiac index, oxygen extraction and lactate concentration are comparable with the values measured in our study suggests that the different results for gastric pHi, during the anhepatic stage may be attributed to the more severe chronic impairment of intestinal perfusion in our patients with end-stage cirrhosis. This is supported by the lower pHi values measured early during hepatectomy in our study (7.28 ± 0.08). In fact, low pHi values (>7.32) before the second postoperative day, an observation that further supports the presence of chronic impairment of mucosal microcirculatory perfusion. Hence, pre-existing chronic mucosal hypoxia might explain why pHi decreased during the anhepatic stage, although overall portal flow was maintained by the use of VVB.

It also cannot be excluded that the apparent difference in baseline pHi values between our study and that of Camprubi and colleagues’ patients without VVB had returned to baseline after reperfusion. However, in our patients with end-stage hepatic cirrhosis, pHi did not reach normal values (>7.32) before the second postoperative day, an observation that further supports the presence of chronic impairment of mucosal microcirculatory perfusion. Hence, pre-existing chronic mucosal hypoxia might explain why pHi decreased during the anhepatic stage, although overall portal flow was maintained by the use of VVB.

Volatile anaesthetics and neuromuscular block

Chronic gastric ischemia—A cause of abdominal pain or bleeding identified from the presence of gastric mucosal acidosis. Journal of Cardiovascular Surgery 1989; 30: 852-859.


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P. M. C. WRIGHT
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University of Newcastle upon Tyne


Correspondence

E. Saline PCO2 is an important source of error in the assessment of gastric intramucosal pH.
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