Correspondence

Experience with parents' presence during anaesthesia induction in children

To the Editor:

We were interested to read the paper by Hannallah and Roasales (Can Anaesth Soc J 1983, 30: 286-9) and wish to make further observations on the importance of the parents' presence during anaesthesia induction.

For many years it has been our regular practice to interview the child together with the parents pre-operatively. Every effort is made to gain their confidence and to give adequate explanations about the prospective anaesthetic.¹

The reluctance of some anaesthetists (to allow parents to be present) is overcome by using a "simpler" method of induction. Our personal preference is i.m. Ketamine and scopolamine administered whilst the child is held in a parent's arms in the waiting area.²

Similarly we also encourage the early admission of a parent into the recovery room - thus leaving the overall impression with the child that the parent has been constantly present and also relieving pressure on the recovery room nurses.

We suggest that the two weeks' follow up after surgery is unlikely to be sufficient to reveal the long-term beneficial effects of the intervention for the experimental group or the detrimental effects for the control group.

A most interesting finding in the study, and one that the authors did not discuss, is displayed in Table VII: It reveals a high percentage of children in both groups with emotional difficulties after surgery, indicating the need for a baseline of behaviour prior to surgery for the experimental and control groups.

A need exists for additional intervention such as information, anticipatory guidance, support of care-

givers etc. for the parents and their children, in addition to the presence of parents during induction of anaesthesia.

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- 2 Birkhahn HJ, Shamash R, Gutman D. Ketamine-dissociative anaesthesia in pediatric oral surgery. J Oral Surg 1971; 29: 853-7.

Rebreathing and the Bain circuit

To the Editor:

Once again Spoerel (Can Anaesth Soc J 1983; 30: 148-54), in attempting to justify the unjustifiable ($100 \text{ ml} \cdot \text{kg}^{-1} \cdot \text{min}^{-1}$ flow in spontaneous breathing patients) has contributed to the misunderstanding of the mechanics of the Mapleson D breathing system.

While finally agreeing with Mapleson¹ that this system requires a $\dot{V}_E/\dot{V}_F$ ratio of at least 0.5, he also states that FiCO_2 is "passive" and does not contribute to CO_2 homeostasis. I do not think that Spoerel has clearly supported this dangerous concept, which, if carried to its logical conclusion, may be interpreted as inferring that rebreathing is not a significant contributor to high PaCO_2 under anaesthesia!

Really though, it's about time that this spontaneous versus controlled respiration concept was finally discarded.

This and other papers^{2,3} on similar aspects of