Gynaecological laparoscopy

Sir,—I was interested to read the findings of Drs Kurer and Welch (1984) in their investigations of techniques of anaesthesia for laparoscopy. I would like to make some comments.

Many studies have recommended the use of controlled ventilation, but even where spontaneous respiration was used, most investigators have used endotracheal intubation (Desmond and Gordon, 1970; Hodgson, McClelland and Newton, 1970; Lewis et al., 1972). There can be little doubt that the combination of Trendelenburg position and abdominal distension must predispose to passive gastric regurgitation. Some studies have shown an increased end-tidal carbon dioxide concentration during spontaneous respiration, and this has been associated with cardiac arrhythmias (Desmond and Gordon, 1970; Hodgson, McClelland and Newton, 1970). The potential dangers of aspiration, hypoventilation and cardiac arrhythmias are all decreased by tracheal intubation.

In the group which was under controlled ventilation, I would question the choice of pancuronium. In this series the duration of anaesthesia was, on average, 15 min or less. In these circumstances, the use of pancuronium, which has a medium to long duration of action, would seem undesirable (Martindale, 1977). It would seem more appropriate to consider the use of gallamine, or alcuronium, or better still one of the new non-depolarizing myoneural blockers, vecuronium or atracurium.

During the operation the authors noted that, in the spontaneously breathing group, some 18% had cardiac arrhythmias. It is unfortunate that they did not evaluate whether this was of statistical significance, compared with the intubated and ventilated group. The authors applied statistical analysis to their other findings, and I wonder if such an analysis might not have shown that spontaneous respiration with a mask and breathing halothane was less than satisfactory during laparoscopy. My own analysis of a test of significance of proportions shows that the incidence of tachycardias of more than 120 beat min⁻¹, possibly attributable to the use of pancuronium. The true significance is therefore much higher.

In the recovery period there were four patients who suffered from respiratory problems, and all were in the controlled ventilation group. The clinical descriptions would seem to be entirely consistent with curarization. As no assessment of the degree of neuromuscular blockade was made, it will be impossible to confirm this. However, the dose of pancuronium used (> 65 µg kg⁻¹) and the short duration of anaesthesia makes this at least a possibility.

I note that these women all had an invasive narcotic premedication, which not only caused complaints of dryness of the mouth, but must further have depressed respiration in the patients who breathed spontaneously, in addition to increasing the chance of nausea. I wonder if a more appropriate premedication might not be used to advantage (Beecher, 1955).

It would seem possible that the use of a short acting non-depolarizing relaxant might reveal that tracheal intubation and controlled ventilation is the method of safety and choice, with fewer respiratory problems after operation.

The combination of a narcotic premedication, spontaneous respiration with halothane, and using only a mask, may lead to complications in the hands of anaesthetists less experienced than the authors.

J. M. Lamberty
Portsmouth

REFERENCES


Sir,—Thank you for giving me the opportunity to reply to Dr Lamberty’s comments about our recent article. I acknowledge that his comment “there can be little doubt that the combination of the Trendelenburg position and abdominal distension must predispose to passive regurgitation” is accepted clinical teaching in anaesthesia. I would, however, like to draw his attention to some detail in the references that he cited.

The Trendelenburg tilt used by Hodgson, McClelland and Newton (1970) was 20° and was implemented for a period of at least 20 min. In the study of Lewis and associates (1972), the tilt was 30° maintained throughout the surgical procedure with a mean duration of 14.2 min. Desmond and Gordon (1970) were in an unfortunate situation in that the laparoscopies studied were of 30 min to 1 h duration and they used a 10–15° tilt. In our study we only used a 10° tilt and the surgical procedure lasted for a mean time of less than 5 min. Also, of the 120 patients we investigated, we were unable to demonstrate one case of passive gastric regurgitation. In a paper by Duffy (1979), two of 93 fasting patients did demonstrate this phenomenon in intubated cases, but in this study “the gynaecologist placed the patients in the lithotomy position with steep head-down tilt”. I would, therefore, suggest that when the gynaecologist with expertise minimizes the physiological problems by requiring no more than 10° tilt and performs the procedure in less than 10 min, gastric regurgitation becomes less of a problem. I would, however, accept Dr Lamberty’s sentiments in suggesting that, when the surgeon or anaesthetist, or both, are inexperienced, it is wiser to intubate the patient.

As regards the use of pancuronium, we did not intend to suggest that it is an ideal agent to produce neuromuscular blockade for this procedure, but were investigating two clinical techniques that had been used in the Royal Berkshire Hospital for
many years and had satisfactorily fulfilled the requirements of both surgeons and anaesthetists. This also accounts for the choice of papaveretum and hyoscine premedication. I would agree that, for further clinical comparison of two such techniques, I would choose one of the newer agents as Dr Lamberty suggests, and monitor myoneural blockade with a peripheral nerve stimulator. Incidentally, despite the invasive narcotic premedication, most of patients in our study required major analgesia after operation. Thus, I would be cautious about using benzodiazepine premedication without intraoperative opiates for inpatients.

The comments made about the statistical significance of cardiac arrhythmias are valid, but I do not see that it can be presumed that the tachycardias could just be attributable to the use of pancuronium, as the stress of tracheal intubation readily produces such changes. Harris, Plantevin and Crowther (1984) have demonstrated that, in laparoscopy patients during intubation facilitated by suxamethonium and topical 4% lignocaine to the vocal cords, 16% developed arrhythmias. Furthermore, for the duration of abdominal insufflation there was no statistical difference in the incidence of arrhythmias between the group which were ventilated with alcuronium and halothane and the group of patients who breathed spontaneously on enflurane.

In summary, I agree with the penultimate paragraph of Dr Lamberty’s letter, but if laparoscopy is to be seriously considered as a day-case procedure, the significantly better morbidity at 4 h in our non-intubated patients breathing spontaneously should be further investigated. I await the results of a study that compares the technique rationally advocated by Dr Lamberty with a technique in which patients are not intubated and breathe spontaneously under the conditions as described in our article.

F. KURER
London

REFERENCES

Relative potency of agonist and partial agonist opioids

Sir,—The use of relative potency when comparing agonist with partial agonist drugs may be misleading. Partial agonists have flatter log dose–response relationships than full agonists, and with non-parallel curves a relative potency can only refer to a single level of response, for example point A in figure 1. At any other level, the relative potency must be different. With doses producing greater response (points B), the ratio partial agonist:agonist is inevitably increased.

It is, therefore, inappropriate to apply a relative potency derived in one set of circumstances in a different situation, unless it can be shown that the same level of effect is achieved in both instances. In their paper comparing the respiratory depressant effects of meptazinol and pethidine, Slattery and colleagues (1983) may have concluded incorrectly that, in the doses tested, there is no advantage of meptazinol over pethidine in respect of respiratory depression “since the equipotent dose ratio for analgesia is 2.4:1”. It is impossible to compare levels of response in the widely differing circumstances in which this ratio was determined (using on-demand analgesia) and applied.

In addition, the use of on-demand analgesia to determine relative potency of analgesics (Slattery et al., 1981; Harmer et al., 1983) is full of pitfalls. A retrospective assessment of a variable degree of pain is an unreliable method of assessing level of effect, especially if it is uncontrolled in relation to recent opiate medication; also, the ratio of doses of drugs used over 24 h, which are affected by drug, duration of effect and cumulation, is irrelevant to single dosage.

Where partial agonists are concerned, the method is liable to introduce numerically large errors. When the effects sought are on the linear part of the dose–response curve, the relatively flat relationship means that considerable changes in dosage cause limited changes in effect. Thus small, statistically insignificant differences in mean pain score may reflect large differences in dose. Also, when patients, in an effort to achieve more analgesia, use doses greater than the smallest producing maximum effect (point C, fig. 1), the dose bears no relationship to the analgesia obtained, and the relative potency derived is worthless. This may possibly occur with partial agonists of low maximum efficacy. Finally, the patient’s assumption of satisfactory analgesia, the pain score indicated and the amount of analgesic used may all be affected by side effects produced.

It is not surprising, therefore, that Slattery and colleagues’ potency ratio of 2.4:1 for meptazinol and pethidine differs so widely from Paymaster’s (1977) assessment of equipotency at a dose of 100 mg. Paymaster’s patients obtained adequate analgesia at this dose, but Slattery’s, who had undergone major upper abdominal surgery, may well have sought a greater effect. The same group (Harmer et al., 1983) determined a different potency ratio, of 1.39:1, for the two drugs used in patients after similar surgery, which may reflect a lower level of analgesia achieved by

![Figure 1: Schematic log dose–response relationships for agonist and partial agonist drugs.](https://academic.oup.com/bja/article-abstract/57/7/718/294080)