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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.

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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”.

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

FIG. 1. Schematic log dose–response relationships for agonist and partial agonist drugs.
Dr Kay probably correct in saying that equipotency ratios for analgesia should not be compared with the effects of a single dose on respiration. However, most patients receive multiple doses of analgesics, so that the practical question is whether a range of different prescribed doses could have effects on ventilation akin to the agonists pethidine and morphine. This seems at least possible.

Dr Kay alleges that comparing potency by a patient demand technique is generally unreliable. However, determinations of equivalent potency for buprenorphine and pentazocine (Slattery et al., 1983) pethidine and meptazinol (Slattery et al., 1981) and now morphine, pethidine and nalbuphine (Bahar, Rosen and Vickers, 1984), fall within generally accepted ranges. The consistency shown does not support the view that estimates based on self-administered regimens are “full of pit-falls”. It is possible that meptazinol is the exception and some of the problems may lie in its lack of efficacy. In a small study of 10 mothers in labour, using self-administration by the i.m. route, meptazinol was 1.5 times less potent than pethidine, and three of five mothers receiving meptazinol asked for an extradural block in future: none of the five receiving pethidine did so.

Sir,—Dr Kay schematically illustrates an aspect of theoretical analgesia receptor pharmacology. His thesis would have been more convincing with real data, but these may be hard to find. Our experience with meptazinol for severe postoperative pain by self-administration showed it to be significantly less potent than pethidine. We agree that it is probably also less efficacious than pethidine (in the way Dr Kay’s diagram shows) because, although mean analogue scores for pain were not significantly different, some patients who received meptazinol withdrew from the trial, while none of those receiving pethidine did so.

It is not clear whether Dr Kay is quoting from unpublished data, giving an opinion, or merely extrapolating from the theoretical dose–response curves when he goes on to assert that meptazinol may be, per contra, more potent than pethidine at doses equivalent to less than 100 mg of pethidine. In any event, not many clinicians are interested in the relative potency of barely therapeutic doses. Furthermore, Dr Kay does us less than justice by selective quotation. We did not say that meptazinol “is less potent than pethidine” but that “in these terms . . .” (rate of consumption) “it is less potent than pethidine, although it seems relatively more effective when given intramuscularly . . .”

Dr Kay maintains that relative potency should be related to a single dose or level of response. That was so in our study. The response was obtained when the patient titrated himself to optimum analgesia with the study drug, balancing pain relief against side effects. Each patient was at liberty to keep pain relief optimal all the time.

SIR,—The case of bronchospasm and hypotension following cardiopulmonary bypass reported by Drs Durant and Jouckien (1984) raises several interesting points.

The authors describe the rarity of this phenomenon, with a reported incidence of four cases worldwide. In the recent experience of this hospital there have been three cases of sudden massive increase of airway pressure (>40 cm H2O) in a series of 90 bypass procedures. None of these patients had a history of allergy or asthma. This incidence of 1 in 30 would imply that perhaps the condition is not all that rare.

The description of this increased airway pressure as bronchospasm would imply an underlying bronchomotor abnormality. This issue is clouded by the use of drugs such as aminophylline and adrenaline, which have profound cardiovascular and bronchomotor effects. So many factors are changing at the end of cardiopulmonary bypass that a simple cause may be hard to find. This was particularly true of our second patient, in whom no cause was found, the “bronchospasm” improving gradually with aminophylline and adrenaline. However, in our other two patients a specific cause was found or implied. In the first, the

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