1 Both nitrogen excretion and nitrous oxide uptake fitted well to the form $\dot{V}_{N_2O} = A \cdot t^k$ up to 100 mins into anaesthesia although a large range of values of the constants (A and k) between individual patients was found.

2 After 70 mins the results from a conventional compartmentalised model failed to produce an adequate fit to the data. Discrepancies between the predictions of the model and our data could be accounted for by hypothesising that measurable quantities of gas were lost or gained at the surgical incision. Comparison of the predictions of the model and our results with those of other authors including Virtue *et al.* indicated that the departure was not present for results obtained with intact volunteers.

3 The good agreement between our data and the model obtained from 4 to 70 mins into anaesthesia lead us to conclude that after wash out of the FRC uptake by the body as measured by a closed circuit is a true reflection of alveolar uptake. No "blood/gas" barrier need be hypothesised to account for the results.

We feel that the method we have employed updates the closed circuit method of Severinghaus⁵ in much the same way as the breath-by-breath methods of Swanson *et al.* update those of previous authors. Additionally we find that our results are in agreement with their conclusions. We would go further than Swanson *et al.*, however, in maintaining that Lin's continued insistence on the idea of a steady state of nitrous oxide uptake irrespective of washin to lung is both confusing and potentially hazardous. In some patients if it was used as a strategy for control of a closed circuit an unacceptable dilution of nitrous oxide by excreted nitrogen could result. The methods he has used to "establish" this idea do not bear close examination and his assumptions are questionable.

It would seem from our study that the power law relationship, $\dot{V}_{N_2O} = 1000 t^{-1/2}$, is a safe starting point for flow of gas in to a closed circuit. However, the wide variation in the constants between patients and the unpredictability of leaks in circuits in normal clinical use make oxygen monitoring essential when attempting a minimal flow technique in a closed circuit.

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Patient-controlled analgesia

To the Editor:

The paper by Sprigge and Otton on patient-controlled nalbuphine administration for postoperative analgesia¹ raises two interesting issues. Despite the demonstration that nalbuphine has a ceiling respiratory depressant effect² the authors consider the "possible risk of respiratory depression due to overdose" and suggest that the method be confined to a high dependency unit thereby negating the advantage of nalbuphine.

Our study of nalbuphine given on-demand³ allowed the patient to administer very much larger doses than those available to Sprigge and Otton's patients; a 20 mg bolus to start, 5 mg each demand, with a compulsory delay of three minutes between demands. Despite the fact that some patients re-

ceived very large doses indeed (up to 180 mg in 1 hour), no clinically significant respiratory depression was observed. The maximum FETCO_2 recorded was 5.1 per cent, and the maximum rise in any patient was 4.8 per cent.

The use of these very large doses raises the second issue. Does nalbuphine have a ceiling analgesic effect? Our experience indicates that it does. Nine of our 20 patients studied requested another method of analgesia, seven because they could not obtain satisfactory pain relief despite a near-maximum rate of self-administration. On the other hand all these patients were very sleepy, and amnesic; the hypnotic effect of nalbuphine appears to continue to increase with increasing dosage after peak respiratory depression is obtained.

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Patient-controlled analgesia

To the Editor:

Sprigge and Otton's article¹ was most timely. They correctly point out that the post-surgical patient is more often than not undertreated for pain. I would agree once more that the cause of this problem is the wide variance in drug requirements from one patient to the next coupled with the quite legitimate effort on behalf of physicians and nurses to provide the maximum safety for patients under their care

even if this involves less than ideal pain relief in some cases. One can only applaud the use of demand analgesia (DA) for establishing the relative potency of a relatively new analgesic drug compared to older and better known agents. It is one of the virtues of this technique that "real" patients can be studied and the use of a placebo is made unnecessary.

In my opinion, DA has two main virtues compared to other techniques of pain relief. One is that drug overdosage as well as underdosage can be avoided, the other is that swings in drug levels during every dosing cycle can be minimized. To best achieve this, small increments of a long-acting drug should be made available at frequent intervals in such a fashion that the limits set by the therapist are only infrequently tested. This was not the case in these authors' work: it seems very likely from their Figure 2 that particularly during the first hour postoperatively some patients would have self-administered more than four injections, had the machine been programmed to provide a shorter lock-out period than 15 minutes. Obviously, if the machine settings impinge on the true drug requirements of patients, conclusions about the relative potency of two drugs might be distorted.

In our own investigations, morphine 10 mg·100 kg⁻¹ or its equivalent is made available to our patients. This is however divided into ten increments instead of the four used by the authors. This type of medication is adequate in most instances save that we find it necessary from time-to-time to add a loading dose to this regimen as the patient arrives in the recovery room. This is hand-administered by a physician who remains in attendance for the subsequent five to ten minutes to evaluate its effect.

I was particularly disturbed by the fact that patients were drowsy during the entire period under study and that they required a great deal of assistance for activating the machine. This is contrary to all our experience with well over 1,000 cases and should not – in my opinion – be charged against the technique itself but rather against the premedication or the equipment used. It should be remembered that the Palliator was originally designed to be used in labour and it is quite possible that two-push triggering is too complex a task for the just awakened subject who nevertheless might experience considerable discomfort. I must emphatically