CORRESPONDENCE


RECOVERY FROM METHOHEXITONE ANAESTHESIA

Sir,—I am grateful to Drs Dubois and Savege for their comments (1975) on the subject of recovery from anaesthesia with methohexitone (Carson, Graham and Dundee, 1975). We agree entirely with their views on the importance of the definition of recovery and the methods employed to establish the end-point. As they state, no one test can demonstrate accurately that the patient is no longer under the influence of anaesthetic agents or sedative medications. Thus a common definition of full recovery is probably impossible.

The phrase “time to full recovery” may be misleading, however. We were careful to define our end-point, and in our conclusion we emphasized that the peg-board method, by itself, was not intended to determine a definite end-point consistent with “street-fitness”.

The peg-board test was a modification of that described by Vickers (1965). It was simplified intentionally to decrease the “learning effect” of repeated attempts. Fully conscious volunteers are unable to improve their performance by more than 1–2 s using the test as described.

We agree that the more complex the test the longer the recovery. In the psychomotor tests employed by Dubois and Geddes (1974), measurements were made five times during a period of 24 h following anaesthesia. Does this imply that their results are accurate to within 4–5 h?

The subjective feelings of the patient were not studied as an index of recovery; they illustrate solely that patients receiving barbiturates may over-estimate their performance. This information was collected at 30, 60 and 120 min after the patients opened their eyes on command, and not following completion of the performance test as suggested by Dubois and Savage.

The final sentence from Dubois and Savage is most misleading, as Dubois and Geddes (1974) reported the effects of thiopentone only. We agree that the time to full recovery and “street-fitness” after thiopentone is longer than after the steroid, but methohexitone has characteristics different from thiopentone.

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REFERENCES


FACTORS IN DIFFICULT DIRECT LARYNGOSCOPY

Sir,—I would like to support the analysis of the anatomical factors in difficult direct laryngoscopy by White and Kander (1975). We were confronted recently by two patients in whom it was possible to see only the superior edge of the epiglottis on laryngoscopy. In both these ladies it was not possible to displace the tongue forward sufficiently to lift the epiglottis from the posterior pharyngeal wall, and an endotracheal tube, or an introducer, could not be passed beyond it. Attempts at endotracheal intubation were terminated in the first patient by prudence, and in the second by the cracking of a much-filled lateral incisor tooth. Anaesthesia in the first patient, for the operation of hemicolectomy, was continued with nitrous oxide, oxygen and ether via a face-mask. The operation proceeded smoothly and the patient made an uneventful recovery. In the second patient an inhalation anaesthetic was satisfactory also and this lady has been advised to warn the anaesthetist before any future operation.

FIG. 1. Lateral radiograph of head and neck.

Measurements of the x-rays of the patients’ jaws confirm the findings of White and Kander (fig. 1). In both, the ratio of measurement 5 to measurement 6 was 2.8, which lies at the most difficult end of their scale; ratio 5 : 7 was 1.7 and 1.8 respectively, and the cervical interspinous measurements, 10 and 12, were 0 and 7 mm, and 2 mm and 4 mm respectively.

In our second patient, the anatomical difficulties were compounded by inability to open the mouth more than about 4 cm, and by splayed upper incisors. Endentulous patients do not present problems of endotracheal intubation, so dental configuration is obviously a major factor, although
bony resorption of the mandible in the endentulous may be significant also.

While it is reassuring to find evidence confirming anatomical disproportions after failing to intubate the trachea, it would be more valuable to make predictive measurements on patients as part of the assessment before operation. For measurements $5$ and $6$, which seem to be the most useful indices, one would require only a pair of calipers and a centimetre rule.

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I am grateful to Mr N. R. Barnes, R.T., Superintendent Radiographer, for assistance with x-rays and measurements.

REFERENCE

NITROPRUSSIDE AND CYANIDE

Sir,—One of the points made by Vesey and Cole (1975) was their conviction that plasma thiocyanate concentrations are a poor indication of exposure to cyanide during nitroprusside therapy. We would like to point out that there are also pitfalls in using cyanide blood concentrations for that purpose.

We have published evidence (Smith and Kruszyna, 1976) to show that nitroprusside penetrates human red blood cells and reacts rapidly with oxy- or deoxy-haemoglobin in the interior. All five equivalents of cyanide are liberated from 1 mole of the product and one haeme group on the haemoglobin tetrameter is oxidized to the ferric form. The latter is able to trap one equivalent of free cyanide in the biologically inactive form of cyanmethaemoglobin. The remaining four equivalents of cyanide are free to diffuse from the red cell and exert their characteristic effect. Although the cyanide of cyanmethaemoglobin is biologically inactive, it may be measured as part of the total blood cyanide. Until a method is devised for measuring blood-free cyanide, as opposed to total cyanide (including that fraction bound in an inactive form, but it is not at all clear what the situation is in cases of prolonged continuous administration. In mice the apparent blood cyanide concentration exceeds the established lethal limit by a factor of more than two, but we have found that these animals do not die of cyanide poisoning, apparently because the cyanide is bound as cyanmethaemoglobin (Smith and Kruszyna, 1976).

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REFERENCES


AN OBSTETRICIAN'S VIEW OF PAIN

Sir,—As an anaesthetist interested in developing a comprehensive obstetric analgesic service, I welcome greatly the compassionate and sensible outlook of Professor O'Driscoll's article (1975). However, I would like to make three comments.

First, it would seem unwise to base fears regarding extradural analgesia in Great Britain on the grounds of foetal blood concentrations of meptivacaine and lignocaine since these agents are rarely used; the neonatal concentrations of bupivacaine are much lower and the drug has much less tendency to cumulate (Reynolds, 1972).

Second, Morrison and colleagues (1973) have detected neonatal depression following administration of pethidine 59 mg i.v. to mothers more than 60 min and 3 h before delivery, so that the use of even small doses of pethidine appear to have immediate deleterious effects; other studies quoted by Dubovitz (1975) suggest that long-term neurological deficits are attributable to the use of pethidine in labour.

Third, I note with some surprise the preference of pudendal block to extradural block on the grounds of maternal safety. Whatever Professor O'Driscoll's reservations concerning the accuracy of reporting of disasters attributable to extradural analgesia, it cannot be denied that one death has been attributed to pudendal block by the confidential inquiry into maternal deaths covering the years 1964 to 1966. (A further death attributable to pudendal block was almost certainly a result of the accidental use of 1:1000 adrenaline with the local anaesthetic.)

Since the introduction of an extradural service at Lewisham Hospital we have seen no evidence of any tendency on the part of our obstetricians to prolong labour in a traumatic manner. Nevertheless, Professor O'Driscoll's warning will be heeded by all. Our unit has welcomed the virtual disappearance of the need for general anaesthesia on the labour ward, largely as a result of the availability of extradural analgesia.

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REFERENCES

