anesthesia. Finally, in the same unit a study of 140 elective Caesarean section patients who received spinal anaesthesia with hyperbaric 0.5% bupivacaine 1.5-2.5 ml gave an average umbilical vein pH of 7.31 (0.04) and an umbilical artery pH of 7.26 (±0.04). In common with the study in question, all the patients received a preload of Hartmann’s solution 1 litre followed by an infusion of ephedrine 60 mg in Hartmann’s solution 1 litre to prevent hypotension. However, as suggested by Kang [2], the only difference was that these patients received a rapid infusion of ephedrine 10 mg in the first 2 min. We agree with the findings of Kang that the rapid infusion of ephedrine prevents hypotension and subsequent neonatal acidosis. Umbilical artery and vein pH in the 20 patients studied by Kang were 7.26 (±0.03) and 7.34 (±0.04), respectively.

In conclusion, we recommend the use of spinal anaesthesia using a combined spinal and extradural technique for both emergency and elective procedures in obstetrics. It provides both rapidity and density of spinal block, combined with the ability to extend the block and provide postoperative analgesia with the extradural catheter. Hypotension and subsequent neonatal acidosis can be prevented by rapid infusion of ephedrine and, as always, by ensuring adequate precautions have been taken to prevent aorto-caval compression.

Sir,—Thank you for sending me this correspondence. While we accept that mean cardiac output (CO) did not change in the spinal group, there were some patients who exhibited a profound decrease. This was the reason why we also reported the percentages changes in CO. We accept that aorto-caval compression could have led to a reduction in cardiac output in some patients, although every effort was made to prevent this. However, as the subjects were allocated randomly to the groups and the same preventative measures were undertaken in each group, we suggest the difference in pH is more likely to be secondary to the haemodynamic effects of spinal anaesthesia.

The umbilical artery data from the author’s large series are reassuring. It would be interesting to know the number of babies with an umbilical artery pH ≤ 7.15. We agree that one possible reason for the differences between our results and those of Kang, Abouleish and Caritis [1] could have been the different ephedrine regimens used. This was pointed out in the discussion and is a topic we are presently investigating.

S. C. ROBSON
London


S. C. ROBSON
London


PREDICTION OF THE SPREAD OF REPEATED SPINAL ANAESTHESIA WITH BUPIVACAINE

Sir,—The paper by Tuominen and colleagues [1] is based on an interesting series of patients who underwent repeated spinal anaesthesia. On the first occasion, all the patients received plain bupivacaine, but subsequently a modified technique was used, with the patients being allocated to three groups. The first group had blocks to T10 or below on the first occasion and the second group had blocks above that level. During the second anaesthetic, the technique was changed in an attempt to increase the level of block in the first group and decrease it in the second. The changes in technique produced significant alterations in the level of block in the groups as a whole, although it has to be noted that there were individual exceptions in whom the result was the opposite to that “expected”. These results represent a nice demonstration both of the way in which the anaesthetist may influence events in spinal anaesthesia, and of the way in which it can frustrate his best intentions!

The third group of patients developed a very wide range of blocks on the first occasion. During the second anaesthetic, an identical technique was used but hyperbaric, rather than plain, bupivacaine was injected. There were no differences between the two blocks in the group as a whole, although various levels of injection, patient position and needle size were used. The result was used to justify the conclusion that density of solution is relatively unimportant in determining intrathecal drug spread. As the vast body of literature on spinal anaesthesia takes the opposite view, I cannot accept this conclusion. Any technique, even when applied meticulously by a single individual, would produce a range of blocks. When such basic factors are not kept constant, they may obscure the influence of other factors, even one as fundamental as baricity. It is essential in studies of spinal anaesthesia that the only difference between two groups should be the one under investigation, so that its effect is clear.

J. A. W. WILDSMITH
Edinburgh


Sir,—We thank you for the opportunity to reply to Dr Wildsmith’s letter.

Because of limited space in this section, we refer directly to groups 1, 2 and 3. The only difference between the first and second spinal anaesthetic was the baricity of bupivacaine, otherwise the technique was similar in each patient: the level of injection, patient position and needle size, and the volume of the local anaesthetic solution or speed of injection. In the two other groups, the technique for the first and second spinal anaesthetics was different because of the different aims: to produce a higher or lower block. In this study each patient served as his own control, and this should serve as a reliable method in a clinical study. We compared the changes within the groups, but not between groups 1, 2 and 3. The only way to overcome the problem of a wide range of blocks would have been to discard results from some patients, but that did not seem reasonable.

Our conclusion was not “density of solution is relatively unimportant”. We stated: “Individual anatomical properties may play a more important role than expected in the subarachnoid spread of local anaesthetic”... and, “The extent of block is affected also by the baricity of the solution in relation with the position of the patient.”

The conclusion stems also from the studies by Stienstra and van Poorten [1], Mitchell and colleagues [2] and Alston [3] in which no differences between the spinal blocks with plain and hyperbaric 0.5% bupivacaine were found. Those studies demonstrated nicely that there are always patients who have a low block and others with unnecessarily high spread of analgesia, irrespective of the density of the local anaesthetic solution used.

We drew our conclusion on the basis of the results in all three groups of this study, our earlier study of repeated spinal anaesthetics and the findings by Mitchell and colleagues [2] and Stienstra and van Poorten [1].

We do appreciate the vast literature on regional anaesthesia in addition to the concepts drawn from recent studies. However, we are also prepared to accept new opinions on the prediction of spread of spinal anaesthesia. We feel that what is needed most at this stage is greater knowledge of the anatomical-physiological determinants of the individual patient.

M. TUOMINEN
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Helsinki

CORRESPONDENCE


POSTDURAL PUNCTURE HEADACHE

Sirs,—We read with interest the article by Lynch and colleagues [1] comparing the incidence of postdural puncture headaches (PDPH) associated with 22-gauge and 25-gauge Whitacre needles. As mentioned in the accompanying editorial, a significant factor affecting the incidence of PDPH is age. Lynch’s group examined patients over a wide age range (15–81 yr). We feel that to include within the same group patients with such a well recognized difference in the incidence of PDPH, is inappropriate.

Lynch and colleagues attempted to subdivide their patient groups further, but considering the previously demonstrated incidence of PDPH of less than 4% (as stated by the authors) in younger patients [2,3], the number needed to demonstrate a significant difference (P < 0.05) in PDPH, even in this susceptible group, would have to be much greater. We feel that it is important to stress that the stated result that there was “no significant difference between groups” does not mean that the 22-gauge and 25-gauge Whitacre needles have clinically equivalent incidences of PDPH. Knowing the number of patients studied and the expected incidence of PDPH, there is a greater than 50% chance of finding no difference between the two needles if, in fact, this is not the true situation.

On examination of the published data, we were unable to reconcile the stated incidence of 0.9% for PDPH in females older than 45 yr with the detailed patient characteristics shown in table III. Clarification would be helpful.

It is important and clinically relevant to discover if a 25-gauge Whitacre needle has significant advantages over a 22-gauge needle and we hope that Dr Lynch and his colleagues are continuing their study so that this question may be answered.

C. M. COONEY
J. TARPEY
Dublin


Sirs,—Thank you for the opportunity to respond to the letter from Drs Cooney and Tarpey. As the authors stated, the age factor in the aetiology of postdural puncture headache (PDPH) is indeed important, but as our study was designed also to look at the handling characteristics of the 25-gauge Whitacre needle, we feel that the inclusion of all age groups in the study was justified. Younger patients were still well represented in the study, however, as some 66% of the patients in both groups were younger than 40 yr (M: F = 2:1).

We agree with Drs Cooney and Tarpey on the importance of enrolling sufficient numbers of patients to assess statistical significance. As the incidence of PDPH becomes less (1–2%), only the prospective evaluation of PDPH in a large series of patients of matched sex, age and medical background, using matched techniques for matched surgical procedures will resolve these questions [1,2]. The design of large multicentre studies with appropriate patient numbers, although beset with difficulties for a variety of logistical and historical reasons, may be one possible solution to this problem. We are exploring these possibilities at present, while still continuing our studies with Whitacre needles.

We agree also that the lack of a statistically significant difference in the incidence of PDPH between the groups does not necessarily mean that they will have a clinically equivalent incidence of PDPH. Preliminary reports from other authors using a Whitacre 25-gauge needle in large groups of patients have confirmed their ability significantly to reduce the incidence of PDPH without compromising the ease of administration of spinal anaesthesia [3]. The incidence of 0.9% refers to the incidence of PDPH in young males and not older female patients as the authors mistakenly assume.

J. LYNCH
Seattle


Adequacy of preoperative safety checks of the Bain breathing system

Sirs,—Recently, we discovered a large crack in the fresh gas supply to a Bain breathing system—a well known, but infrequently encountered, complication. The leak was discovered by a simple occlusion test of the inner (fresh gas flow) tube—a test which should be known to, and used by, all anaesthetists.

Following a visual check, the circuit was pressurized, the bag inspected for leaks and the expiratory valve opened and checked. The oxygen flush was activated; the reservoir bag failed to deflate. A 6-litre min−1 fresh gas flow was commenced and the inner tube occluded using a finger (a 2-ml syringe plunger may be used). The flowmeter bobbin failed to descend, indicating a leak from this part of the circuit. Close visual inspection revealed no obvious fault, but when the system was dismantled, the inner mount of the co-axial manifold was found to be sheared.

Reliance on the Venturi test does not necessarily detect leaks that occur in the distal part of the circuit. The only reliable test is to occlude the inner tube and to observe if the transmitted back pressure causes the flowmeter bobbin to descend. Reliance on pulse oximetry, or on end-tidal carbon dioxide monitoring, would provide a late indication of impending disaster.

We surveyed all the 30 full-time anaesthetists working at our group of hospitals. This included 16 consultants, two senior registrars, six registrars, and six senior house officers. Staff grades and clinical assistants were excluded from the survey. Of the 30 anaesthetists surveyed, only 14 (47%) knew of the correct test and, of these, only nine (30%) used it. The reason for this was that they relied on sophisticated monitoring to detect apparatus failure.

The results of this survey suggest the teaching of safety checks of the Bain system may be deficient, and that anaesthetists are lulled into a false sense of security by sophisticated monitoring techniques. Furthermore, this deficiency was not confined to junior grades.

Although this small survey is not representative of the profession as a whole, it is representative of anaesthetists at our group of hospitals, and may well apply to other institutions.

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