

Letters to the Editor

Shoulder function after radical neck dissection

Dear Sir,

We read with interest the audit into the incidence of handicap after unilateral neck dissection by Shone and Yardley. Certainly the 'shoulder syndrome' (Nahum *et al.*, 1961) is a common and unpleasant complication of this operation when performed as described by Crile (1986) and is largely attributable to motor denervation of the trapezius muscle. More recently a second motor supply to this muscle has been described originating from the third and fourth cervical spinal nerves (Weitz *et al.*, 1983). These nerves may be preserved if the deep cervical fascia is not breached in the posterior triangle of the neck during the performance of a radical neck dissection and the benefits from taking this precaution have already been reported (Jones and Stell, 1985; Manning and Stell, 1989). We therefore recommend this modification to the classical technique of radical neck dissection in an effort to reduce the occurrence of this unpleasant complication.

Yours faithfully,

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References

- Crile, G. W. (1986) Excision of cancer of the head and neck. *Journal of American Medical Association*, **47**, 1780–1786.
- Jones, T. A., Stell, P. M. (1985) The preservation of shoulder function after radical neck dissection. *Clinical Otolaryngology*, **10**, 89–92.
- Manning, M., Stell, P. M. (1989) The shoulder after radical neck dissection. *Clinical Otolaryngology*, **14**, 381–384.
- Nahum, A. M., Mullally, W., Marmor, L. (1961) A syndrome resulting from radical neck dissection. *Archives of Otolaryngology*, **74**: 82–86.
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Reply:

Dear Sir,

The clinical significance of the innervation of Trapezius through the third and fourth cervical spinal nerves has been the subject of several papers. We entirely agree with McCombe and Cook that preservation of these nerves may result in retention of partial Trapezius function, and look forward to seeing some EMG evidence which would confirm or refute this.

Yours faithfully,

G. R. Shone

M. P. J. Yardley,

P.S. The references in McCombe and Cook's letter contain at least two errors—Crile's paper was published in 1806* and Weitz *et al.* in 1982.

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*Try 1906! Ed. (George Washington Crile, 1864–1943).

Mastoidectomy packs: Xeroform or BIPP?

Dear Sir,

Chevretton *et al.* (1991) carried out an interesting study which confirmed the clinically observed superiority of BIPP over Xeroform. They have not provided a possible explanation on why BIPP is so effective, but cast doubt on two possible means I had suggested in an earlier study (Nigam and Allwood, 1990). The evidence for their doubt is not backed up by any scientific evidence in their retrospective study.

In paragraph one they describe Xeroform as a 'non-adherent absorbent gauze' and they then appear to contradict themselves by stating the gauze is impervious to blood. Clinically Xeroform dressings become infected and therefore must be permeable, whereas BIPP dressings remain comparatively fresh. In practice, antibiotic cover is not required with BIPP packing to the mastoid.

Yours faithfully,

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Reference

- Nigam, A., Allwood, M. C. (1990) BIPP—How does it work? *Clinical Otolaryngology*, **15**: 173–175.

Reply:

Dear Sir,

We thank Mr Nigam for the interest shown in our paper. The aim of our study was to compare the clinical efficacy of Xeroform and BIPP as a mastoidectomy dressing. It was not a study of the mechanism of each pack's efficacy.

There was evidence, however, to reject both hypotheses of Nigam and Allwood (1990). Their first hypothesis was that the meticulous debridement of the mastoid cavity by the surgeon prevented subsequent infection. In our study the same meticulous debridement took place in both the Xeroform and BIPP cavities by the authors and yet only the Xeroform packs became infected. The only variable at operation was the choice of pack.