The rhomboid flap: a simple technique to cover the skin defect produced by excision of a mucous cyst of a digit

Sir,

I noted the article by Imran, Koukkai and Bainbridge1 in the August 2003 issue entitled ‘The rhomboid flap: a simple technique to cover the skin defect produced by excision of a mucous cyst of a digit’.

To see this ancient form of treatment propagated in a respected educational medical journal in this day is of grave concern to me. It has been clearly shown by multiple studies, including a recent published study by myself, that mucous cysts of the digits arise from the proximal interphalangeal joint of the digit. This is from a fluid leak around the distal interphalangeal joints of the digits with subcutaneous migration producing an often more distally based ‘cystic lesion’. Treatment with debridement of the dorsal aspect of the joint and irrigation or curettage of the thin cystic walls is all that is necessary. Under no circumstances is it appropriate or necessary to perform complex skin cover. These procedures, which have been popularised in the plastic surgery literature and inter-active education conferences, are unnecessary. The lesions will always epithelialise within a shorter period of time than three weeks.

I plead that the concept of rotational or any flaps in the treatment of the mucoid cyst of digits be abandoned.

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Author’s reply

Sir,

We would like to thank Dr Beckenbaugh for drawing our attention to his work on the subject of mucous cysts. We agree that the majority of mucous cysts can be adequately treated with simple excision and that it is important to remove the underlying spike of bone arising from around the distal interphalangeal joint.

The vast majority of mucous cysts can be closed directly if care is taken to separate the skin from the underlying cyst wall. In this connection we find that loupe magnification is often very helpful.

However, in a small proportion of patients the skin is so atrophic, or the underlying cyst has been infected so many times, that a significant defect persists. In this situation the rhomboid flap that we have described is a simple, safe and effective means of closure that can be planned on a mathematical basis and requires little in the way of technical expertise in its elevation.

We accept that the fundamentals of surgery for the mucous cyst are: the vast majority can be excised and closed directly; the underlying joint is always abnormal and the osteophyte should be sought and removed and a small proportion of cases will require flap closure.

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Radiographic measurement of joint space height in non-osteoarthritic tibiofemoral joints

Sir,

We read with interest the paper in the September 2003 issue by Deep et al1 entitled ‘Radiographic measurement of joint space height in non-osteoarthritic tibiofemoral joints: a comparison of weight-bearing extension and 30˚ flexion views’. They found that there may be a difference of up to 2 mm between normal tibiofemoral joints when assessed with weight-bearing posteroanterior radiographs at full extension and 30˚ flexion. This observation is important when assessing patients with these radiographic views.

However, we would be grateful if the authors could explain their rationale for randomising patients for inclusion in their study, and why they have used the term “double blind” in the methods? Randomisation is used in clinical trials as a method of reducing the possibility of systematic bias between treatment groups. Double blinding reduces bias that may be caused by either the patient or observer knowing which treatment group has been allocated. This paper is an observational study and we see no advantage in selecting patients at random over sequential selection. It is logical that the observers in this study are blinded. However, all patients in this study receive the same management and we are not sure how ‘double blinding’ can apply?

Prospective, randomised, double blind clinical trials are very important in giving high quality evidence in clinical practice.2 The number of such trials in the orthopaedic literature is increasing,3 although many are underpowered.4 We feel that in describ-
ing a “double blind, randomised study” the authors may be attempting to increase the impact of their paper which may equally have been described as an assessor blinded, observational study.

We read with interest the article by Grossman et al1 in the November 2003 issue entitled ‘Outcome after later combined brachial plexus and shoulder surgery after birth trauma’. We agree that the end-to-side repair technique remains controversial. In our practice it has proved extremely successful in a variety of situations. Our technique uses a careful but deep longitudinal neurotomy performed under high magnification with a diamond knife. This differs from the operative technique attempted before recommending late nerve repair for obstetric brachial plexus palsy and in particular the use of the end-to-side technique.

We would suggest that a better controlled study should be undertaken before recommending late nerve repair for obstetric brachial plexus palsy and in particular the use of the end-to-side technique.

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Outcome after later combined brachial plexus and shoulder surgery after birth trauma

Sir,

We read with interest the article by Grossman et al1 in the November 2003 issue entitled ‘Outcome after later combined brachial plexus and shoulder surgery after birth trauma’. The possibility of carrying out late nerve reconstruction for obstetric brachial plexus palsy certainly deserves exploring. We are particularly interested in the use of botulinum toxin. The results look encouraging but we are not convinced that they really provide evidence that surgery on the brachial plexus and shoulder produces benefit over the surgery for internal rotation contracture of the shoulder alone.

The use of end-to-side of nerve repair is controversial. The work carried out by Viterbo et al2,3 demonstrated that suturing the distal end of a divided nerve to the side of another nerve allowed re-innervation to occur in a rat sciatic nerve model. However, experiments in sheep, a larger animal model more likely to be representative of the human situation, have shown the technique to be unreliable.4

The indications for operation are stated to have included persistent paralysis of biceps. However, there are no results included in respect of elbow flexion.

Table III lists the modified Gilbert shoulder evaluation results. There are four cases who had an internal rotation contracture at follow-up which would appear to be incompatible with a grade 5 result.

We would suggest that a better controlled study should be undertaken before recommending late nerve repair for obstetric brachial plexus palsy and in particular the use of the end-to-side technique.

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Author’s reply:

Sir,

We appreciate Messrs Hems and Sherlock interest in our work.

Actually, as noted in Table I, in 12 of the cases a posterior capsulolasty was required along with the release of the medial rotation contracture. While certainly there are cases with a medial rotation contracture which require only a release, the intra-operative neurophysiological studies in our patients suggested that a contracture release alone would have undoubtedly failed in the majority, if not all of the cases.

We agree that the end-to-side repair technique remains controversial. In our practice it has proved extremely successful in a variety of situations. Our technique uses a careful but deep longitudinal neurotomy performed under high magnification with a diamond knife. This differs from the operative technique described in some experimental reports.

In all cases, the children had M4 or better biceps recovery post-operatively.

A careful look at our modified Gilbert score (Table II) and the outcome data (Table III) shows that the presence of the post-operative limitation of lateral rotation (active or passive) is still compatible with the grade assigned.
The idea of a controlled study is always attractive. Some paediatric neurologists still propose this for all children with a brachial plexus birth injury, regardless of age. The results in our practice suggest that such a study would be inappropriate treatment for many of those selected for operation. However, if such a study was undertaken, it is imperative that all surgical procedures be done by the same experienced surgeon on a carefully defined set of patients.

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Errata

A. Arora, A. Agarwal. Dhaga syndrome: a previously undescribed entity


It is regretted that Figure 3 was reproduced as Figure 4. The correct Figure 3 is shown right:

Fig. 3a
Radiographic findings in case 2 showing the 'constriction sign', which appears as an indentation or constriction in the middle of the longitudinal periosteal reaction on the distal ulna.

Fig. 3b