

claims that the article was wrong in stating that there is no published case of day-stay adult tonsillectomy in Britain. He quotes his own article published in 1996. However, if he had read my article properly he would see that it was accepted for publication prior to his own article.

Secondly, Mr Mitchell questions the study design because of possible bias caused by using the in-patient population as a day surgery one. Again, if he had read my article properly he would have seen that this point was conceded in the discussion, and I quote:

This study has an important source of bias. The sample studied was from an in-patient population who were suitable for day surgery and not actually a day surgery population.

Finally, he states that I assume that day surgery results in substantial cost savings. Again, if he had read my article properly he would have seen that no such assumption is made. It is merely mentioned that the economic benefits were a possibility.

I hope that this reply corrects the inadequacies in Mr Mitchell's and Mr Kenyon's letter. I do feel that they could have been avoided if they had read my original article properly.

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#### Gynaecomastia: have Webster's lessons been ignored?

A. J. PARK and B. G. H. LAMBERTY 1998; 43: 89-92

Sir: I congratulate the authors for doing an excellent review of the various surgical approaches in excision of gynaecomastia, even though they reported five cases with ugly hypertrophic scars that were carried out elsewhere. The timely reminder of Webster's peri-areolar incision is most appropriate for surgeons who wish to carry out such operations, which ideally should result in aesthetic results.

The authors omitted to comment on the trans-nipple areolar approach used by some surgeons. I have found that such an approach will also have higher risks of symptomatic scars at the nipple, and occasionally a synechia may occur at the nipple-areolar junction. I now use a predominantly periareolar approach. In fact, I now classify periareolar incisions into three types: (a) incision with the areola; (b) incision at the areolar-skin junction; and (c) incision outside the areola.

The decision as to which approach to use is dependent on a physical examination of existing scars on the individual. Those who heal with a faint whitish scar will benefit from a type c approach, as leaving a whitish line to contrast with the pigmented areola will make the incision visible.

The type a approach is for those who are prone to keloids or hypertrophic scars, as evidenced by the presence of a BCG scar in the deltoid region or other thickened scars on the knees, elbows or elsewhere on the body. The majority will have the type b approach, which is just at the junction. I have observed that the other two approaches give better cosmetic end results in selected patients, and wonder of the authors or your readers have a similar observation.

C. K. REXON

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#### Authors reply

Sir: Thank you for the comments on our paper. I have no experience of the other incisions that Rixon uses but feel that an incision at the areolar-skin junction gives a good cosmetic result in Caucasians. Rixon comments that this incision can leave a whitish line to contrast with the pigmented areola. This may be the case in darker skinned patients but I have not found it a problem with white skinned individuals. I have also not been faced with the problem of correcting gynaecomastia in men prone to keloid formation so cannot comment on the intra-areolar incision.

However, I am grateful for Rixon's comments and will consider them in future.

A. J. PARK

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#### Stapler failure in pharyngeal diverticulectomy: a suggested modification in surgical technique

H. KUBBA and S. S. MUSHEER HUSSAIN 1998; 43: 31-32

Sir: We read with interest the above article on stapler failure in pharyngeal diverticulectomy. With the advent and popularity of endoscopic stapling diverticulectomy the hazards of stapler failure are even greater.

We recently experienced stapler failure during endoscopic diverticulectomy. After the Ethicon Endopath stapler was placed across the party wall between the oesophagus and pouch and fired in the usual manner we noticed a distal defect of 1.5 cm with no staples visible. As this was recognized early we instituted proper conservative management and the defect healed in 5 days.

The problems of stapler failure in endoscopic operations are twofold. Firstly, the defect may go unrecognized and, since the usual procedure is to commence oral feeding soon after the procedure, mediastinitis may be the first sign of the perforation. Secondly, some of these elderly patients may not tolerate 'open' operations to close these defects after stapler failures.

We advocate close inspection of the stapled party wall so that defects can be noticed and managed before complications ensue.

B. N. KUMAR AND A. P. JOHNSON

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#### A combined fascia and mesh closure of large incisional hernias

M. S. WHITELEY, S. B. RAY-CHAUDHURI and R. B. GALLAND 1998; 43: 29-30

Sir: Kindly allow us to comment on the above article.<sup>1</sup> Whiteley *et al.* described a combined fascia and mesh closure of large



incisional hernias. This is identical to the technique described by Browse and Hurst.<sup>2</sup>

The only new point made is that the technique is also suitable for right subcostal incisional hernias. However, anatomically there is no anterior rectus sheath at the lateral aspect of a subcostal incision, and the recommendation to turn a fascial flap downward for a first layer cannot apply. The technique therefore seems to add nothing to Browse's operation and does not appear to be anatomically applicable to the 'large incisional hernia' in a subcostal incision. A simple extraperitoneal mesh repair would seem to be easier and more applicable to the latter situation.

## REFERENCES

- 1 Whiteley MS, Ray-Chaudhuri SB, Galland RB. Combined fascia and mesh closure of large incisional hernias. *J R Coll Surg Edinb* 1998; **43**: 29-30.
- 2 Browse NL, Hurst P. Repair of long, large midline incisional hernias using reflected flap of anterior rectus sheath reinforced with Marlex mesh. *Am J Surg* 1979; **138**: 738-9.

V. NARAYNSINGH, R. SINGH-RAMPAUL and  
D. MAHARAJ  
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## Authors' reply

Sir: We thank Naraynsingh, Singh-Rampaul and Maharaj for their interest in our paper and suggest that the answers to their comments are already in the paper if it is read carefully enough.

In the second paragraph we point out the existence of the method that Naraynsingh, Singh-Rampaul and Maharaj quote, and in the penultimate paragraph we both quote it and discuss it. We have made it quite clear that our paper describes our experience of this recognised technique in situations not previously suggested.

At no point in our paper do we say that the fascia used for the first layer of repair need be the rectus sheath. We can assure Naraynsingh, Singh-Rampaul and Maharaj that we have always found a fascial layer superficial to the abdominal wall musculature that can easily be mobilised as described in our paper. This may well be a combination of external oblique and scar tissue in the lateral portion of the subcostal incision, but it functions as well as the anterior rectus sheath more medially.

The reasons why there are advantages over a simple extraperitoneal mesh repair are also noted in the paper. With the technique described dissection of the sac is not required, reducing operating time and unnecessary risk of breaching the peritoneum or damaging the sac contents. The formation of the fascial layer means that there is a second structural layer to the repair, giving additional strength over that provided with mesh alone. Furthermore, the suturing of mesh into a defect without abdominal wall support leaves a repair with some tension; and with only peritoneum separating bowel from mesh, this leaves a risk of bowel adherence and subsequent damage.

We thank Naraynsingh, Singh-Rampaul and Maharaj for their interest, and hope that, with careful re-reading of our paper, they will feel able to try the technique for themselves.

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## Screening times with image intensifier in orthopaedic trauma surgery

A. G. SUTHERLAND and D. F. FINLAYSON 1998; **43**: 265-66

Sir: I read this article with interest having only just recently published on very much the same topic.<sup>1</sup> I was interested to note that the various procedures were not broken down by any form of classification that would indicate their severity or complexity. There may be differences in screening times for particular procedures, depending on the type and severity of the case mix.

I was also interested in the statement that there was no apparent difference between radiographers involved, although no figures were presented to support this conclusion.

In our own study, which focused on extracapsular neck of femur fractures, we found highly significant relationships between the seniority of the radiographers and the screening time/radiation dose. There were also differences depending upon the complexity and severity of the fracture being treated.

## REFERENCES

- 1 Gainnoudis, PV, McGuigan, J, Shaw, DL. Ionising radiation during internal fixation of extracapsular neck of femur fractures. *Injury* 29 **6**: 469-72.

## Author's reply

Sir: We are grateful for Mr Shaw's comments. His paper was published in *Injury* after we submitted our work, and deals with some similar issues.

We agree that the complexity of operations will have an effect on screening times, and indeed suggested this as a possible reason for some longer screening times for consultant-led procedures. With small numbers it was not possible to place our patients with proximal femoral fractures into meaningful groups for analysis in the way that Mr Shaw has done, but we believe that a larger study may well reach similar conclusions.

In Inverness, a much smaller centre than Bradford, radiographers were from a smaller pool of more equal experience, which may explain the fact that there were no real differences in screening times between radiographers.

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