

## Letters to the Editors

### Antibiotic prophylaxis in clean neck dissections

*J Laryngol Otol* 2004;**118**:213–16

Dear Sirs

We read with interest the article entitled 'Antibiotic prophylaxis in clean neck dissections' by Seven *et al.* in the March 2004 issue of *The Journal of Laryngology & Otolology*.<sup>1</sup>

The study suggested that the use of peri-operative antibiotics for 24 hours in patients undergoing clean neck dissections results in a significant reduction in the incidence of post-operative wound infection. Detailed analysis of the available information allows alternative interpretation of the data; therefore we believe that the methodology and results collected do not justify the conclusions drawn.

The study was based on a historical retrospective review of 51 patients who underwent clean neck dissections in the authors' institution; seven of these patients developed post-operative wound infections (13.3 per cent). This was compared with a prospectively analysed group of 57 patients in whom, following the use of peri-operative ampicillin for 24 hours, a carefully defined wound infection was noted in one patient (1.7 per cent).

The difference in wound infection rate was noted to be statistically significant ( $p = 0.02$ ), which is not really surprising as the historical infection rate was about three times that noted in some series.<sup>2,3</sup> Clean wounds are defined as uninfected operative wounds in which no inflammation is encountered, and the respiratory, alimentary or genitourinary tracts are not entered.<sup>4</sup> The incidence of clean surgery wound infection is widely quoted at 1–2 per cent.<sup>5</sup> (Perhaps if the control group is taken as having a high post-operative infection rate, the reduction of this rate in the post-intervention group might have been for additional reasons to the one suggested in the paper.)

In addition, no attempt was made to clarify the definition of wound infection used for the historical group. One may assume that it could have been even higher if the strict definition of wound infection quoted by Johnson *et al.*<sup>6</sup> was used in both groups.

We would also disagree with the assertion made by the authors that, following this study, a double-blind, placebo-controlled trial would be unethical. In fact, our assessment of this article would lead us to take a converse view. After an initial retrospective assessment of a high post-operative infection rate, an audit loop showed significant reduction in post-operative infection when adhering to protocol. A double-blind trial of a prophylactic antibiotic regimen against a prospective control group is the ideal study to clarify the fact in question: 'Does antibiotic prophylaxis significantly reduce an acceptable incidence of post-operative infection in clean neck dissection?'

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### References

- 1 Seven H, Sayin I, Turgut S. Antibiotic prophylaxis in clean neck dissections. *J Laryngol Otol* 2004;**118**:213–16
- 2 Johnson JT, Wagner RL. Infection following uncontaminated head and neck surgery. *Arch Otolaryngol Head Neck Surg* 1987;**113**:368–9
- 3 Herranz J, Sarandeses A, Fernandez MF, Barro CV, Vidal JM, Gavilan J. Complications after total laryngectomy in nonradiated laryngeal and hypopharyngeal carcinomas. *Otolaryngol Head Neck Surg* 2000;**122**:892–8
- 4 National Academy of Sciences, National Research Council Divisional Medical Sciences, Ad Hoc Committee of the Committee on Trauma. Post-operative wound infections. *Ann Surg* 1964;**160**(suppl):1–192
- 5 Leaper DJ. Use of antibiotic prophylaxis in clean non-implant wounds. *J Antimicrob Chemother* 1998;**41**:501–4
- 6 Johnson JT, Myers EN, Thearle PB, Sigler BA, Schramm VL Jr. Antimicrobial prophylaxis for contaminated head and neck surgery. *Laryngoscope* 1984;**94**:46–51

### Author's reply

Dear Sirs

Thank you for permitting me to reply to Repanos *et al.* and their comments on our paper. We welcome their comments but believe such criticisms arise from misinterpretation of our paper.

As we stated, a detailed analysis of our historical control group has previously been published<sup>1</sup> and wound infection for both groups was defined according to the criteria developed by Johnson *et al.*<sup>2</sup> These groups were similar with respect to the potential risk factors for wound infection development such as previous radiotherapy, systemic disease and stage of disease.

We did not assert that 'a double-blind, placebo-controlled trial would be unethical' but, in fact, stated that there was a possibility that 'a double-blind, placebo-controlled trial might be unethical because of a relatively high rate of wound infection in patients who had undergone neck dissection with no antibiotic use'.

Mr Repanos and his colleagues propose a double-blind trial of a prophylactic antibiotic regimen against a control group. We are aware that the double-blind, randomized, controlled trial is the best method we have against potential bias. However, randomized controlled trials are some way off in our clinic because, as mentioned in the paper, the relative infrequency of this operation makes such data collection difficult and prolonged.

We are disappointed that Mr Repanos and his colleagues did not support their criticisms with citations and that they did not share with readers insights into their own practice. A retrospective review has obvious inherent biases and our prospective series was designed to compare outcome not only with our historical control group but also with data from the relevant literature. Unfortunately, there is no prospective study in the literature which has shown the rate of wound infection in patients who have undergone clean neck dissection without antibiotic use. Therefore, we could not compare our results with those obtained from others. Nevertheless, when compared with published retrospective data,<sup>3,4</sup> our wound infection rate is significantly lower.

The factors affecting wound infection development in clean neck dissection (CND) are not entirely clear, but we

believe that the clinical data obtained from our study indicate that prophylactic antibiotic use in CND is likely to be the important factor for minimizing the risk of post-operative wound infection development. However, like others, we would welcome a future double-blind, placebo-controlled trial.

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#### References

- 1 Seven H, Cakir B, Yavuz E, Senvar A. Is antimicrobial prophylaxis necessary for neck dissection? *Kulak Burun Bogaz Ihtis Derg* 1998;**3**:182–4
- 2 Johnson JT, Myers EN, Thearle PB, Sigler BA, Schramm VL. Antimicrobial prophylaxis for contaminated head and neck surgery. *Laryngoscope* 1984;**94**:46–51
- 3 Coskun H, Erisen L, Basut O. Factors affecting wound infection rates in head and neck surgery. *Otolaryngol Head Neck Surg* 2000;**123**:328–33
- 4 Carrou RL, Byzakis J, Wagner RL, Johnson JT. Role of prophylactic antibiotics in uncontaminated neck dissections. *Otolaryngol Head Neck Surg* 1991;**117**:194–5

#### Importance of X-ray in chronic stridor

Dear Sirs

We report the case of a 30-year-old woman who presented with a 5-week history of progressively increasing stridor, dysphagia and occasional vomiting. Two years previously, she had undergone a colonic interposition graft to bypass a corrosive induced oesophageal stricture. After the operation she had been able to eat and drink normally without any problem. Six weeks before presentation she swallowed a kitchen knife which was removed by means of an operation through a

cervical incision. Ten days later, she was transferred to a psychiatric hospital for rehabilitation and spent 3–4 weeks there. During this time she developed minimal stridor and dysphagia but was able to eat and drink. Thus the stridor was ignored by the doctors.

Three days after her discharge from the psychiatric hospital, she attended an accident and emergency department with total dysphagia and vomiting. She denied any foreign body ingestion. She was referred to an ENT team, without X-ray, because of her stridor and previous history. On examination, whilst on air, she had biphasic stridor, spitting of saliva and no cyanosis. She appeared to be comfortable with a SaO<sub>2</sub> of 96 per cent and a temperature of 37.4°C. Chest examination revealed transmitted stridor and slightly diminished air entry which was bilaterally symmetrical with no wheeze.

X-rays of her chest and neck (Figures 1 and 2) showed no obstruction in the trachea, visible air in the oesophagus and the rubber tip of a syringe plunger, visible at the level of T2 spine.

The patient admitted having swallowed a syringe plunger when she was in the psychiatric hospital but had not told the doctors. She told the psychiatric doctors about her mild dysphagia and difficulty in breathing which were ignored because of normal vital parameters. Once the plunger was discovered, she underwent pharyngoscopy and oesophagoscopy and the plunger in the oesophagus was removed. Post-operatively she underwent barium swallow, which revealed sluggish peristalsis and no obstruction in the colonic interposition graft. She was therefore discharged.

Foreign body ingestion is a common occurrence in children and specific high-risk groups of adults such as those with underlying oesophageal disease, prisoners, the mentally retarded, and those with psychiatric illnesses.<sup>1</sup> The most common foreign bodies are fish bones in adults

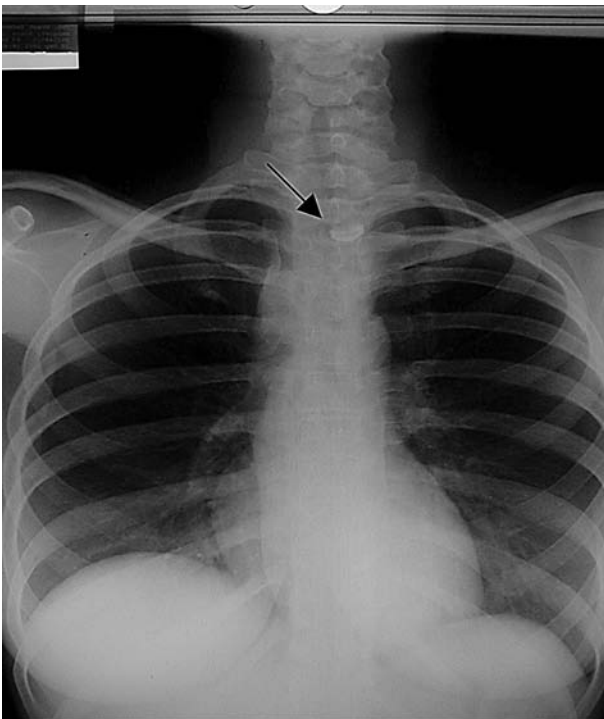


FIG.1

X-ray of chest showing the tip of a syringe plunger just above the medial end of the left clavicle.



FIG.2

X-ray of neck showing air in the trachea and oesophagus. Foreign body is not obvious.