Response to Lowe *et al.*

20 April 2007

Sir,

Thank you for the opportunity to respond to the comments made by David Lowe *et al.* We are encouraged that they too are of the impression that secondary haemorrhage post-tonsillectomy is unlikely to be infective in origin.

We would respond to their various points as follows:

Our title raises the key question and describes what we did.

A detailed literature search highlighted current standards for objectively measuring systemic infection while also accepting that there is no guaranteed gold standard in this area. Our search indicated Temperature, White cell count (WCC) and C-reactive protein (CRP) as reliable measures. We feel that we provided sufficient discussion concerning CRP and those other factors which influence its level.

We did not enter into a detailed discussion regarding the role of antibiotics or other measures as this was not part of our aim in the paper. We did not 'recommend' the use of hydrogen peroxide we merely suggested that it's use 'may be sufficient'.

We would be interested in the evidence to support their own statement that: 'prophylactic antibiotic administration, steroid administration, anti-pyretic administration, surgical method and indication for tonsillectomy…..are of course all powerful factors which may significantly influence haemorrhage'.

Regarding elevated WCC and CRP, our text states that 26 patients had an elevated CRP. In 17, this was not accompanied by an elevated WCC while in 9 it was. We apologise if this was not made sufficiently clear by our choice of wording.

We stand by our methodology, although not perfect, as a cross-sectional analysis of consecutive patients presenting with secondary post-tonsillectomy haemorrhage in whom a range of measures at the time of admission failed to support a diagnosis of systemic infection sufficient to warrant systemic antibiotic therapy.

Conflict of Interest

None to declare.

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Is secondary haemorrhage after tonsillectomy in adults an infective condition?

28 March 2007

Sir,

The use of antibiotics in patients with secondary bleeding after tonsillectomy is an almost universal practice in the UK – supported, like a significant part of our practice by limited evidence. The observational study by Ahsan *et al.* is an interesting attempt to fill this gap.[1] However, we have to disagree with some of their findings and statistical calculations – and consequently, most of their conclusions:

1. The authors used WBC counts, Neutrophil count and CRP as haematological/biochemical (and not clinical, as stated) indicators of infection. Their use is understandable, as they are objective and readily measurable indicators. The problem however with their rationale is that, although a rising CRP – WBC – neutrophil count is suggestive of systemic infection, a normal value does not rule it out. That is why clinical indicators of infection are so helpful and include increasing odynophagia, dysphagia, lymphadenopathy, foetor etc while microbiological indicators of infection include cultures and direct microscopy from tonsil fossa swabs. As none of these appear to have been measured, one cannot say that infection was ruled out. Indeed, in our prospective study[2] we found a highly significant correlation between levels of pathogens and subsequent risk of bleeding after tonsillectomy: we found that a preoperative swab which

showed normal flora or no bacterial growth was associated with an 8.8 per cent chance of bleeding *versus* a 26.9 per cent chance of bleeding with pathogen growth, and this finding was statistically significant (odds ratio: 3.8, 95% Confidence Interval 1.1 to 12.1)

2. fCRP levels were available for 34 patients (page 25, 'results – outcome measures – right column') or is it 32 patients (page 25, results-outcome measure – left column)?

3. The data on elevation of CRP levels is confused. We quote from page25 – results '…15 (of 47) (31.9%) patients were observed to have an elevated WCC, 13 (86.6%) of these patients had an elevated neutrophil count and CRP levels were available for only nine patients. Seven had elevated levels while two had normal level…' This is also reflected in table 1 – so, we understand that seven patients had both elevated CRP and elevated WCC – right? Wrong! In page 25, results we read '…An elevated CRP was found in 53% of patients (17/32). Interestingly, none of these patients had an elevated WCC or neutrophil count…' At the discussion, we read 'Fifteen patients in this study were identified with a raised WCC count and neutrophil count with majority of them having elevated CRP levels…' Just how many patients had (a) elevated WCC; (b) elevated CRP; and (c) elevated WCC and CRP?

4. Out of a total of 494 patients, 47 returned with post-tonsillectomy bleeding. '… Fifteen of the 47 patients (32%) had a return to theatre to achieve haemostasis. This is an overall rate of 0.68% for all tonsillectomies performed…' – No, not really: It is (15/494 =) 3% - almost five times higher.

5. In different parts of the paper the authors state '… An elevated CRP was found in 53% of patients (17/32)…' and then 'In terms of the CRP, this study found 26 patients with a CRP above 10 mg/L' – it is rather unclear what proportion of patients had indeed elevated CRP levels.

We agree with the authors that there is not enough evidence to support the use of antibiotics in secondary post-tonsillectomy bleeding. Though an absence of evidence does not equate to an absence of benefit, we cannot agree that their study produces evidence to the contrary – and in the light of our study – we feel that the current UK practice of prescribing antibiotics for secondary tonsillectomy bleeding ('Wisdom–Based Medicine' not opposed but complementary to 'Evidence–Based Medicine') is justified.

Conflict of Interest

None to declare.

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References

1 Ahsan F., Rashid H., Eng C., *et al.* (2007) Is secondary haemorrhage after tonsillectomy in adults an infective condition? Objective measures of infection in a prospective cohort. *Clin. Otolaryngol.* **32,** 24–27

2 Stephens J.C., Georgalas C., Kyi M., *et al.* (2007) Is bacterial colonisation of the tonsillar fossa a factor in post-tonsillectomy haemorrhage? *J. Laryngol. Otol.* [E-pub ahead of print]

Is secondary haemorrhage after tonsillectomy in adults an infective condition?

4 April 2007

Sir,

The paper by Ahsan *et al.*[1] on the aetiology of secondary haemorrhage post-tonsillectomy raises some interesting points on the most frequent cause of procedure related morbidity amongst Otolaryngology patients. An unpublished audit of our department's figures shows that of 542 tonsillectomies 24 were readmitted due to secondary haemorrhage over the period of 1 year. This accounts for 26% of all documented morbidity in the same year for the whole department.

We concur with the authors that there is a general assumption that secondary haemorrhage following tonsillectomy is of infective aetiology despite sufficient evidence to support this. We also agree that in general, as