

# Is secondary haemorrhage after tonsillectomy in adults an infective condition? Objective measures of infection in a prospective cohort

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Sir,

The authors are to be congratulated on their work, which explores an interesting area of ENT practice.<sup>1</sup> However, we have a number of concerns over both the methodology of the study and the conclusions drawn.

The paper fails to address the question asked in the title and while we still do not know the answer to the question of aetiology of secondary haemorrhage following tonsillectomy, we also feel it is probably not primarily an infective condition. Anecdotally, most of us will have seen antibiotics work in patients with haemorrhage, but equally, have seen patients readmitted with infection and remain free of any bleeding whatsoever. The paper unfortunately does not properly address or set out to test this or any similar hypothesis. As a result, a more appropriate title might have been 'The Lack of evidence of infection in post tonsillectomy haemorrhage'.

The question remains whether infection is the cause of haemorrhage, or merely an additional complication sometimes encountered in patients who return with bleeding.

We felt that the paper failed to discuss current evidence of the useful clinical parameters of infection in general or the appropriate use of inflammatory markers. There was no mention of how surgery or treatment might have affected CRP, white cell counts or temperature. Reference to previous work on use of prophylactic antibiotics, their effect on return to normal activity and influence on temperature<sup>2</sup> (one of the main parameters considered here) is sadly lacking. There was no discussion of evidence of benefits of alternatives such as peroxide gargles<sup>3</sup> although these were later recommended as an alternative management.

Use of any of the recognised models for sepsis and infection would have improved the paper's methodology. Of particular importance in this cohort was the lack of application of any appropriate exclusion criteria whatsoever. We feel that the magnitude of this shortcoming in combination with the incorrect use of parameters of inflammation almost totally negate the results and conclusions. All of the following important variables should ideally have been taken into account: prophylactic antibiotic administration, steroid administration, anti-pyretic administration, surgical method and indication for tonsillectomy. These are of

course all powerful factors which may significantly influence haemorrhage and postoperative recovery.

We suspect that localised tissue necrosis rather than primary infection offers the most plausible explanation for the majority of secondary haemorrhages.<sup>4</sup> While it is established that hot tonsillectomy techniques give rise to increased rates of secondary haemorrhage,<sup>5</sup> hot tonsillectomy also seems to result in a much increased profile of secondary haemorrhage in terms of both its severity and timing after surgery (unpublished NPTA data). Even if tonsillectomy haemorrhage is related to infection rather than tissue necrosis, we feel that the parameters investigated and the limited data collection is unlikely to reliably indicate the presence of localised infection in the tonsillar fossae which represents a unique surgical field.

It is well known that CRP is most useful in monitoring response to treatment rather than presence of infection. Serial measurements are also mandatory to assess change from an individual's initial baseline value. Unfortunately this, as well as temperature was only measured on one occasion in the study. CRP is also affected by a multitude of factors including age, medical conditions, heart and vascular disease, smoking and the patient's weight.

Ten of the patients in the study should probably have been excluded as they were already on antibiotics. There is also confusion explaining how many patients had CRP measurements with numbers varying between 32 and 34 quoted in different parts of the paper. The authors have stated that none of the patients with raised CRP had a raised WCC. This statement is not supported by table 1<sup>1</sup> which shows 5 patients had raised CRP and WCC. Again we do not know if the 15 patients deemed 'appropriate' for antibiotics were already treated with these before admission.

We would therefore suggest that the methodological flaws and errors in the study leave the reader no closer to the answer on the aetiology of secondary haemorrhage following tonsillectomy. More importantly, it neither proves nor disproves an infective aetiology for secondary haemorrhage. It remains likely that this question will only be adequately answered by a combination of well designed prospective clinical research—probably on a multi-centre basis to maximise power— as well as well focussed tissue-based laboratory studies.

1 **Conflict of Interest**

2  
3 None to declare.

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