

Tongue necrosis might have been avoided in our case by super-selective embolization of the lingual artery branch. However, super-selective embolization was not possible in this case because of arteriosclerosis.

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Increased post-operative haemorrhage seen in adult coblation tonsillectomy *JLO* 2003;**117**:704–6

Dear Sirs

It was with dismay and disappointment that we read the above paper. Both the conduct and conclusions of this paper are unfortunate to say the least.

Over the last three years coblation tonsillectomy has been developed and used in East Lancashire. It has been taught to surgeons from all over the UK, including Bolton, and the world. Training videos are also available from the manufacturers.

We have published two papers on the reduced post-operative pain levels found with this procedure. In these papers we pointed out that at that stage no conclusions could be drawn on secondary haemorrhage, and the same should be said about a sample of 36.

Our post-operative haemorrhage rates were presented at BACO this July and the American Academy meeting in Orlando in September 2003 based on a group of 1030 coblation tonsillectomies, a somewhat larger sample than the one quoted in this paper. Our results show a significantly reduced rate of secondary haemorrhage in both paediatric and adult groups.

Our peer-reviewed results will appear shortly as a paper in *The Laryngoscope* and we urge all surgeons genuinely interested in the future of tonsillectomy to read and digest it . . .

In the tiny study by Noon and Hargreaves, the surgeon has made his own departures from the recommended method for coblation tonsillectomy. He has used the CoVac wand, a single lumen device which is no longer available, rather than the Evac 70 wand which provides simultaneous suction and irrigation, transforming the nature of the operation. The placing of a ligature at the lower pole has never been recommended and its use is a mystifying departure and quite unnecessary.

All surgeons in our department use the operating microscope except when requested by those unwilling to try this invaluable accessory to demonstrate the use of the equipment with the naked eye. The microscope makes the surgery faster and haemostasis more meticulous. Clearly, for some surgeons this is a fact that has to be seen to be believed.

Noon and Hargreaves question the coagulative abilities of the coblation equipment but used correctly this is more than adequate, as demonstrated by our own results.

The only reasonable conclusion to be drawn from this paper is not that coblation tonsillectomy has an increased risk of secondary haemorrhage in adults but that the surgeon involved is still on the learning curve and it is perhaps better that this method is not used by those who feel unable to adopt the best practices in its use. The figures in this paper do not exclude the presence of a learning curve as was stated. It is simply that we all have different learning curves.

To this end the BAO-HNS and NICE has commissioned training guidance which is in preparation and will be available soon. Hopefully this will help avoid an elegant and promising technique being discredited.

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Dear Sirs

Noon and Hargreave's paper reporting a secondary haemorrhage rate of over 22 per cent in 36 patients represents an unacceptable level of significant morbidity, regardless of the technique employed.

The coblation technique is recently introduced, and my own experience of a similar number of adults and children undergoing tonsillectomy by this technique, is that it has produced a significant reduction in duration and severity of post-operative pain, and a secondary haemorrhage rate lower than that of other methods.

As with all new technology, there is a learning curve and appreciation of the tissue response to this type of plasma field dissection: for example, inadvertently using ablation after the tonsil has been removed, will result in a deep hole appearing in the muscle bed, with possibly difficult to control bleeding from deeply placed vessels. (I recommend that the ablation setting is reduced to the minimum after the tonsil has been removed and before any final coagulation to the tonsil bed is performed).

The company advises surgeons to perform the first dozen or so coblation tonsillectomies on paediatric patients as the operation is technically less demanding. The authors do not make it clear if this advice was followed, before embarking on more challenging adult surgery. Comparing the skill of an operator using a well-practised technique and a new technology must surely introduce a bias in favour of the established technique.

The current guidance from the National Institute for Clinical Excellence (NICE)¹ concludes that the coblation technique offers advantages in terms of reduced post-operative pain, and has similar risks of haemorrhage to other tonsillectomy techniques.

While the authors dismiss the learning curve effect as the likely cause for their high secondary haemorrhage rate, my interpretation and personal experience, is that it will prove to be the factor accounting for the observed and unexpectedly high complication rate.

To address this potential learning curve problem, NICE has requested the BAO-HNS to produce training standards for this procedure.

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Author's reply

Dear Sirs

I think it is quite clear that my paper is neither an attack on the procedure of coblation tonsillectomy or on any specific individuals who have in the past extolled the virtues of this technique. More it is a simple honestly based critical appraisal of a relatively small series of patients performed in the hospital here. From the data presented, which have obviously been critically reviewed and accepted for publication, I find it difficult to see how either conduct or conclusions could be regarded as unfortunate. I felt very strongly that coblation tonsillectomy was to be a very useful technique and I embraced it from the outset, and undertook the time and trouble to be formally trained in this technique. The conclusion as to whether post-operative haemorrhage rates from a sample of 36 can be decreed significant or not, really depends on how many post-operative haemorrhages there are.

I make note of the Blackburn group's impressive series of coblation tonsillectomies, and my congratulations go out to them for my work. My paper obviously shows I do have an interest in the future of tonsillectomy, and because of this I will read their paper with interest.

In my study I make no pretence that it is anything other than a relatively small sample size. The Blackburn group use a technique that was an extension of their normal tonsillectomy technique, that being microscopic bipolar dissection, and given that I was keen to compare my series of coblation tonsillectomies with my established best practice i.e. bipolar dissection without the microscope and tying the lower pole, this was the technique I employed. With regard to the type of wand used, the Blackburn Group claim that we used the CoVak wand in all cases. In fact the paper quite clearly states that the newer Evac 70 suction-irrigating wand was indeed used in all cases, and this was indeed the case. Therefore the technique of coblation tonsillectomy used in my series differs from my normal technique of tonsillectomy only in the method of dissection used i.e. coblation rather than cold steel, and in haemostasis used i.e. coblation rather than bipolar diathermy. Having entered into the realms of coblation tonsillectomy I was therefore needless to say concerned when a relatively large number of sporadic secondary haemorrhages came to the fore, and naturally I felt I had a duty to report this. The use of a tie at a lower pole is my standard practice with the dissection technique and can in any event do nothing but add to any potential haemostasis. Given that using traditional techniques primary and secondary haemorrhage rates are in point of fact better than the national average within the department here, I felt it was reasonable to assume that the coblation process itself was in some way to blame.

The series I have published is obviously small but certainly not tiny. I did stop performing this technique when I felt ethically I could not continue. Initially I, like the Blackburn group, thought that my increased secondary haemorrhage rate was secondary to a learning curve effect. I therefore continued. However, one of the main worries with this work was the very sporadic nature in which the secondary haemorrhages occurred and statistically we have clearly demonstrated, I feel, that a learning curve effect could be factored out of these results. I readily acknowledge that some groups are enjoying very good results with

this technique and I applaud them. My small series is merely a cautionary tale and should be read and interpreted as that.

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Unusual complication of tonsillectomy: taste disturbance and the lingual branch of the glossopharyngeal nerve *JLO* 2003;117:314–17

Dear Sirs

We read with interest the article by Uzun *et al.* on post-tonsillectomy taste disturbances. Although this is considered a rare complication, with little literature evidence, it is probably under-reported. This is presumably because tonsillectomy patients are rarely followed up. In a recent study, 12 per cent of post-tonsillectomy patients reported taste disturbance with seven per cent lasting more than six months compared with none in the control group.¹ In our unit, a postal survey of patients' perspective of the effect of tonsillectomy on recurrent tonsillitis uncovered two cases of post-tonsillectomy taste disturbance amongst 66 respondents. The two patients who initially volunteered the occurrence of the taste disturbance in the free comments space were contacted by telephone for further details of their taste disturbance.

The first case was a 33-year-old man who noticed an aluminium metallic taste and general taste reduction that persisted for a year before slowly resolving. The second case was a 35-year-old woman who described her taste disturbance as rotten, likened to an abscess which persisted for about two and half years. They both had undergone bipolar dissection tonsillectomy, were otherwise well and not on any medication. This had minimal impact on their oral intake and general health. They were pleased overall with the operative outcome, that resolved the tonsillitis that had plagued them for 16 to 20 years. Another case encountered by the second author was a 23-year-old woman who again underwent a bipolar tonsillectomy for recurrent tonsillitis. Post-operatively, she complained of a bitter taste to all her foods. Blood tests including zinc were normal and formal taste testing resulted in poor responses to sweet compounds with good delineation of bitter, salt and sour compounds. This affected her diet severely; with a marked decrease in consumption of sweet foods. Two years post-operatively she was still suffering from taste disturbances and had lost 1½ stone in weight. She rarely ate at restaurants leading to an impact on her social life and was placed on antidepressants. The use of bipolar diathermy in all the three cases may be relevant.

Although, in general it seems that the health impact of taste disturbances may be minimal and usually resolves over a period of time, but if permanent and/or severe can result in profound social and psychological problems. The lingual branch of the glossopharyngeal nerve that conveys taste sensation from the posterior third of the tongue is found to be firmly adherent to the tonsillar capsule in 21.5 per cent of cases in a cadaver dissection.² A similar percentage of patients undergoing tonsillectomy are therefore potentially at risk of post-operative taste disturbance. Minimal trauma to the tonsillar bed during operation is highly imperative to reduce the occurrence of taste disturbance. More importantly, serious consideration should be given to warning patients pre-operatively about this risk as a routine.