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ADVANCING SURGICAL STANDARDS

Letters and comments

Endonasal dacryocystorhinostomy with and without stenting

MI Syed[1], J Hendry[2], AJ Cain[2], AT Williams[1]
[1]NHS Lothian, UK
[2]NHS Highland, UK
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CORRESPONDENCE TO
Mohammed Syed, E: iqbalms@hotmail.com

COMMENT ON
Mohamad SH, Khan I, Shakeel M, Nandapalan V.
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We read the above article with interest as we conducted a study[1] and a Cochrane review on the same topic. There were aspects in the paper that are unclear and leave us rather puzzled.

The authors state that all the cases were performed by the same surgeon but they did not state how the surgeon decided that stents were needed in a particular case and whether these stents were taken out or left in permanently?

Furthermore, the authors state that the group of patients without stents had a greater subjective success rate than those with stents but have given no logical explanation as to why the group of patients without stents had a significantly better outcome. This finding is in stark contrast to other studies including randomised controlled trials on the subject that reported no significant differences in outcomes between the two groups or even a slightly better outcome in the patients with stents.[2,3] We wonder whether the stents inserted were removed prematurely, which would account for a higher rate of rhinostomy closure.

The authors also state that the postsaccal blockage was assessed by sac washout, probing and dacrocystography. While dacrocystography reliably shows morphologic characteristics of the nasolacrimal system, revealing congenital or acquired stenosis, in our experience, it gives no additional information in management of patients undergoing dacryocystorhinostomy. Moreover, delivery of ionising radiation occurs with this technique; the absorbed dose to the lens has been calculated as 0.04–0.2mSv for dacryocystography.[4]

Statistically, the study is underpowered (n=128). The overall success rate was 82% objectively. On that basis, at the p=0.05 level, taking 5% as an effective clinical difference (using a beta of 50%), a sample size of 160 would be needed to show a clinically worthwhile difference between two treatments.[5] We therefore believe the conclusion the authors draw from their study is based on unreliable data.

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AUTHORS' RESPONSE

S Mohamad[1], I Khan[2], M Shakeel[1]
[1]NHS Tayside, UK
[2]Nottingham University Hospitals NHS Trust, UK
doi: XXX

CORRESPONDENCE TO
Shwan Mohamad, E: shwanbashoory@yahoo.co.uk

We read the response by Syed *et al* to our study with interest and are surprised by the conclusions they have drawn from our paper.

It was stated clearly in our article that between 2002 and 2005 the senior author performed dacryocystorhinostomy (DCR) with a stent. As his success rate was lower than comparable evidence, he decided to change his practice in the hope of improving his results and performed DCR without a stent between 2005 and 2006.

Syed *et al*'s queries regarding stents (including removal time) have already been addressed in the methods section of our paper. In the stented group, the stents were removed at three months following surgery. We believe and understand that this is not premature as stent removal can vary from 4 to 24 weeks postoperatively.[1,2]

As for Syed *et al*'s comment on higher subjective success in the non-stented group, it was stated clearly in our publication that the use of stents was associated with eye irritation, displacement of the tube at the medial canthus, nasal crusting and granulation formation at the rhinostomy orifice, which can affect the outcome. This has been supported by the literature in that a stent can be the reason

for surgical failure owing to causing granulation tissue formation, synechia formation and punctual erosion.[2–4]

Syed *et al*'s comparison of our study with contradictory evidence in the literature including their own study seems selective. There is clear evidence available in the literature for and against the use of stents in DCR and this was acknowledged in our introduction. Several studies (including a prospective randomised study) show a higher success rate in DCR without stents.[5–9] Our study concluded that stents are not necessary for primary DCR. This conclusion has been supported by two meta-analyses.[10,11]

The postsaccal blockage for our patients was tested by the ophthalmologists, who used dacryocystography where indicated. This was a very small group of patients and was deemed too insignificant a finding to be elaborated on in our article.

Generally, a retrospective power calculation is not advised. It is not regarded as good practice and if the result of a retrospective study is significant, power is of no interest.[12–14] It would appear that prospective power has been confused with retrospective power. Depending on how retrospective power is calculated, it might be legitimate to use it to estimate the power and sample size for a future study but it cannot be used legitimately as describing the power of the study from which it is calculated.[15]

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Retained surgical sponges, needles and instruments

VM Steelman
University of Iowa, US

doi XXX

CORRESPONDENCE TO
Victoria Steelman, E: victoria-steelman@uiowa.edu

COMMENT ON
Hariharan D, Lobo DN.
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I read with interest the review by Hariharan and Lobo in which they discuss the incidence of retained surgical items and the seriousness of outcomes to patients, particularly when sponges are retained. Clearly, this issue has not been resolved and requires attention. The authors rightfully point out that the surgical count, a primary preventive measure, has limitations. Discrepancies in the count are a common event and the sensitivity of the surgical count is only 77%.[1] We conducted a healthcare failure mode and effect analysis that identified potential failures in the processes of preventing retained sponges.[2] Distraction and multitasking were the most frequent causes, and are especially difficult, if not impossible, to eliminate.

In their algorithm, Hariharan and Lobo propose using a standardised count process, the surgeon confirming the final count, and the use of radiography if the surgical count is incorrect. This poses a challenge clinically. In addition to the limitations of the count, the sensitivity of intraoperative radiography for detection of a retained surgical item is only 67%.[3] If we rely on these two interventions, we will not likely eliminate retained surgical items. The more sensitive postoperative survey images are taken outside of the operating theatre. This would require tremendous expense and a return trip to theatre if an item is identified. The algorithm would be enhanced by including methodological wound exploration by the surgeon, to search for sponges prior to closure, and a hard stop when a count is reported as being incorrect.

We should also comprehensively evaluate adjunct technology. There are currently three adjunct technologies available to supplement the current processes for prevention of retained sponges. Hariharan and Lobo provide a comprehensive review of the evidence regarding two: the barcoded counting system and the radiofrequency identification system. The third, a radiofrequency (RF) detection system, involves low energy RF chips sewn into sponges and a scanner for detection of the sponges. Two scanners are available: a wand that is passed over the patient and a mat that is placed under the patient.

Studies have found the sensitivity and specificity of the RF wand to be 100%, even in morbidly obese subjects.[4,5] The mat is slightly less sensitive for detection in morbidly obese than in non-morbidly obese patients (97% vs 100%).[5]