Author's response to reviews

Title: The Role of Surgical Audit in Improving Patient Management; Nasal Haemorrhage: An Audit Study

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Author's response to reviews: see over
We would like to thank the reviewers for reading the article and their critique that we have grouped into the relevant areas of discussion.

**General**

**Robert Sawyer Reviewer:** General: Well-written, focused, good methods.

**Beth Ann Ann Swan Reviewer:** Is the question posed by the authors new and well defined? Yes, the question is new and defined - exploring the implementation of a new protocol for epistaxis treatment requiring no admission to the hospital.

**Jan Van der Meulen Reviewer:** I'd like to commend the authors for this short paper that aims to describe the effects of a change in guidelines for the treatment of epistaxis in an A&E Department. It is potentially an example of how carefully devised changes in local guidelines can have a considerable impact on the use of hospital beds without impinging on the quality of care.

We agree with all three reviewers that this is an important area to study especially since epistaxis referrals to A&E/Surgical & Head and Neck departments do make up a large number of admissions which can, and usually do occupy elective beds. The findings would be of interest to clinicians who looks after patients who may potential have bleeds since the protocol can be followed by those with relatively little experience or limited equipment. We present a surgical audit paper to investigate the effect of a change in policy upon admission rates. Audit is now considered a pre-requisite of clinical governance and is now strongly encouraged in both the UK and US systems as a part of the evidence based data gathering. We felt we were able to present an audit that could be ‘modelled’ to other surgically relevant areas. Understandably it is possible to confuse the paper with a formal research study and apply the same stringencies. This is a novel study.
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Robert Sawyer Reviewer: None.
Beth Ann Ann Swan Reviewer: The methods are appropriate, more description would strengthen the manuscript. For example, the power analysis/sample size calculation is not included in the article. The sample size of 60 seems inadequate. Also, the authors state that 10 out of 44 patients had complications. Although they were managed at home and did not require hospital admission, continued bleeding is a complication. Also, it appears that day #2 was when the majority of complications occurred. However, at the end of the manuscript, the authors state that there was "only 1 significant complication." Definitions of complications versus significant complications would strengthen the manuscript. The authors did conduct an retrospective audit, but they also developed, implemented, and evaluated a new protocol.

Jan Van der Meulen Reviewer: It is unfortunate that this simple message is not very well conveyed in the submitted manuscript. This manuscript is not easy to digest for a number of reasons. First, the methods and results are poorly presented. The focus of both these sections should be on the before after study setting out how what the outcomes were and how they were compared. In its current form, the manuscript contains too much clinical detail which obfuscates the main results. Second, I expected to find explicit information about the number of readmissions in patients sent home. This information is absent or alternatively so poorly presented that the reader has to do a lot of guess work. The statistical analysis should be limited to the number/percentage of patients sent home before and after the introduction of the guideline and of those that experienced a readmission or an other untowards event/complication. Third, the manuscript should provide more information about how the analysis was carried out and less information about the statistical results (no need to report degrees of freedom and the chi squared statistics; p-values will do). Fourth, the Discussion should highlight the key results, the methodological limitation, comparisons with other studies (if relevant) and the clinical implications. Now, it presents a large number of clinical observations most of which are irrelevant in this context in no particular order.

We have changed the text to convey the meaning of significant complications as suggested by the reviewer. This article is written as a surgical audit, which by definition is different from a research article. We have tightly followed the outline for audit as presented in the British Medical Journal, however since this was a surgical audit we felt that this journal would be the most suitable considering the demographics of its readership. Audit to be valid would benchmark (previous admission rate), introduce a change in practice (after establishing an evidence base to improve practice), then re-review (admission rate during the change in management) and reflect for further improvement (modify the protocol to reduce complications). We believe that this is the reason why one of the reviewers found the article difficult to follow. We do feel that some of the criticisms are very constructive and have added a figure to clarify this. We have added the clinical detail that we thought was very relevant to illustrate to the junior reader the difficulties of performing audit.
As such, an audit analysis would have to include benchmarking data (technically this is not a retrospective audit but the review part of the audit process) and the change in epistaxis management and review is actually a validating tool. The analysis of data was carried out after statistical advice which suggested that a simple Chi Squared test would suffice to answer or refute the null hypothesis, the analysis was aided by use of a statistical software package (Graph Pad Prism 4.0). We were advised after review of the previous admission rates that a sample size of 46 would have been sufficient to power the study, however since we wanted to adjust for seasonal variations that are known to occur the study was carried out for seven months and 60 patients were recruited. Please note that formal sample size estimates are usually not a re-requisite for audit to be performed. Further analysis using a paired (for time of year), non-parametric t-test (Wilcoxon signed rank test) confirmed a significant difference achieved in admission rate.
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Robert Sawyer Reviewer: None
Beth Ann Ann Swan Reviewer:-
Jan Van der Meulen Reviewer: Table 1 needs information about the number of patients included. Figures 1 and 2 do not seem to add any information over and above the information that is already in the text.

Review of table one reveals that the number of patients was already presented in the data concerning the male: female admitted / discharged with the individual numbers in each group shown. We have now modified the table to make this more noticeable. We would prefer to retain figures 1 and 2 because they graphically summarise the message conveyed in the text. We have added a further figure to further aid understanding as suggested by the reviewers.
Robert Sawyer Reviewer: Statistical review: No, the manuscript does not need to be seen by a statistician.
Beth Ann Ann Swan Reviewer: Data appear sound. The manuscript did not present or discuss "controls" for the study. Are the discussion and conclusions well balanced and adequately supported by the data? The manuscript does present statistically significant findings (the N was small), however, were they clinically significant? This question was not addressed thoroughly except to say that 201 hospital days could be potentially saved.
Jan Van der Meulen Reviewer: No, the manuscript does not need to be seen by a statistician.

We did take statistical advice before analysis of the results. We have used the Chi Squared test because many clinicians are familiar with the test (observed, expected data). We used a further test to show any significance of the results. The results were significant and the text has been modified as the reviewer has suggested to show this. We decided to discuss conclusions derived from only our data. We agree, that it may be common sense that less time spent in hospital would reduce nosocomial infections and be more acceptable to a majority of our patients however this did not derive directly from the presented data and would be an important question for future research studies. Furthermore, this was an audit study hoping to show how a change in procedure may result in more effective treatment. Our discussion was based around the procedure of the audit since we felt that most clinicians would already appreciate the clinical consequences of haemorrhage and the clinical significance of controlling it.

Robert Sawyer Reviewer: Acceptable Quality of written English:
Beth Ann Ann Swan Reviewer: Is the writing acceptable? Yes, there are a few run-on, very long sentences, i.e. abstract and background sections.
Jan Van der Meulen Reviewer: Quality of written English: Not suitable for publication unless extensively edited

As previously observed, there was a range of comments from the various reviewers from a ‘well written focussed’ article to one who suggested that there was an ‘unsuitable quality of written English’ for publication. Since all the senior authors were indigenous English speakers who all work in English Hospitals we found this rather interesting, however we have taken independent expert advice and have changed sentence length and structure especially in the abstract and background sections as suggested by one of the reviewers. It may be that our English style rather than content was the issue.
Epistaxis is a very common condition experienced by almost all people. Its management and sequelae when severe tends to be on the curriculum of most surgical training programmes. Inpatient admission after packing can take up considerable surgical resources and may not be beneficial to patients who are exposed to hospital-acquired infections. This would be of interest to health economists, hospital managers, A&E, Surgical and surgical speciality staff or any discipline affected by unnecessary bed occupancy reducing elective work.

We have answered and made the appropriate changes to the text to conform to the relevant criticisms made by the reviewers. We found their reviews interesting and informative and thank them again. We feel they have improved the paper.