Reviewer's report

Title: Dysphagia in Patients After Dental Extraction: Surface Electromyography Study

Version: 1 Date: 18 April 2006

Reviewer: Giselle Carnaby-Mann

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Reviewers Report

Re: 'Dysphagia in Patients After Dental Extraction: Surface Electromyography Study '
Michael Vaiman, Oded Nahlieli and Eli Eliav
Head & Face Medicine
Research

General:
The authors present interesting surface EMG data on a sample of post dental surgery patients (n =40) in comparison with a control group (n = 40). While this is of interest to readers the focus of the paper needs to be clarified. Major revision is recommended.

Major Compulsory Revisions
A major issue with this paper lies in the failure to define “dysphagia” as referred to in this paper. It appears from the author’s discussion that they are in fact referring to odynophagia only. Dysphagia (as associated with swallowing deficits) refers to impairment in bolus flow associated with physiological precipitants. This does not appear to be present in the cohort of subjects evaluated. The authors are reporting a sample of patients who demonstrate a longer duration of swallow, higher myoelectric activity of some muscle activity, smaller bolus selection, and more dry swallows following drinking. All of these effects seem biologically linked to “tentative swallowing “associated with painful swallowing…not dysphagia as defined above. The authors need to change the title of the paper to reflect this and also clearly define the dysphagia in this study. The authors should consider the statement they make on page 12 , paragraph 3 , line 1 as the focus of this study “dysphagia in dental patients is of oral origin and does not affect the pharyngeal stage of the swallow. This should be included in the abstract also.

Another major issue is the lack of information provided on the controls subjects. How were these subjects acquired? What were their characteristics? Later on page 5 the authors term these persons as “healthy volunteers” yet nowhere do they discuss the impact of volunteer bias on their findings.

The authors statistics are concerning. Interjudge reliability is assessed only by % agreement which does not account for chance. A stronger measure for this data would be ICC, intraclass correlation. Similarly it is unclear on page 9 (paragraph 1) as to where the variables are derived for the “one-way” ANOVA…as 4 variables are specified. In documenting an ANOVA authors must include an F statistic and the degree of freedom for the test. In fact accuracy of statistical reporting is absent throughout this study.

Minor Essential Revisions
It is unclear why the authors on page 4 discuss the ROSS test only and do not mention any data in
regarding other clinical swallowing evaluation forms…this seems out of place and confuses the reader. The ROSS test was never designed to measure myoelectric muscle activity.

On page 6 – How was the influence of multiple “dental surgeons” assessments on subjects prior to participation in the study controlled for? Were these professionals provided with inclusion criteria for this study? If so what was this?

Page 7 – the authors refer to the placement of the surface EMG electrodes for the study, however the figure included does not display this placement. I encourage the authors to use another picture as this has been used in a previous publication and does not accurately depict the placements they describe in this current paper.

On page 8 the authors describe the simultaneous videofluoroscopic swallows of 5 volunteers. This discussion seems out of place and of little value. Why 5 subjects? This suggests “hand picking “ of subjects and thus bias. No data on the videofluoroscopic outcome of this procedure is included here. Omission of this section would help focus the reader.

Authors must consider the impact of multiple testing on the results of this study. When several dependent or independent statistical tests are being performed simultaneously in a study, adjustment for inflation of error should be performed. While a given alpha value may be appropriate for each individual comparison, it is not for the set of all comparisons. In order to avoid a lot of spurious positives, the alpha value needs to be lowered to account for the number of comparisons being performed. In this case alpha: p ≤ 0.0041 (accounting for 12 tests). Accounting of such issues would dramatically alter the findings of this study.

Spelling errors: page 2 (4th line from bottom)
Unclear wording:
· Page 4, paragraph 2 (4th line) line“ despite the lack…..
· Page 4 3rd paragraph line 7 ‘ significantly differs from casual for adults normal drinking” What does this mean?? What is casual for adults????
· Page 6, 1st paragraph, line 3 – this statement is poorly worded “ the subjects reported they are not thirsty”
· Page 14 under conclusions…there is no word as “objectivization”.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.