Radiofrequency: The Next Step for Gingival Depigmentation

Sir,

We read with interest the article by Mahesh *et al*.[1] Congratulations to the authors for giving a new insight into the use of radiofrequency to reduce gingival pigmentation. However, we feel that there are some points that may lead to more accurate outcomes in future investigations.

First, the authors studied 8 gingival units or area from right maxillary first premolar to left maxillary first premolar (or tooth 14-24) in 4 patients. Hence, there would have been totally 32 gingival units to be analysed. However, the authors included only 28 gingival units in their study. We would like to ask the authors to explain the reason why the remaining 4 gingival units were excluded because the study's exclusion criteria were long term systemic illness, genetic pigment disorders and smoking. No exclusion criteria based on "local" gingival defects was described. It is noteworthy that missing data can introduce potential bias, random and systematic errors, and hence, threaten the study validity. Both parameter estimates (e.g., relative risk, odds ratios, or mean values) and statistical inference (confidence intervals (CI), *P* values) may be affected.[2]

Second, to reach their study goals, the authors used the "split-mouth" design. Each of the two treatments was assigned to either the right or the left half of the mouth divided by the mid-sagittal plane between the central incisor teeth. This design increases the study power because it removes inter-individual variability. However, symmetrical disease patterns among all segments of the dentition are necessary for research using a "split-mouth" design.[3] Had the gingival pigmentation been asymmetrical, this would have skewed the study results. Moreover, the study outcomes seem to be blind because they were presented as mean gingival pigmentation scores (MGPS). The MGPS from all subjects/patients were mixed together, and thereby, may be skewed by different "baseline" pigmentation among the subjects.

It is known that "information bias" can occur in a "split-mouth" study. The examiner who evaluates the treatment results could be influenced if he or she knew which treatment was used on which side and had a pre-existing belief about which treatment was more efficacious.[2] Mahesh *et al*.[1] should describe who the outcome evaluator was. For details on pitfalls and limitations of the "split-mouth" design, we refer interested readers to the recent review by Lesaffre *et al*.[3]

Third, any impact can be lost due to the inability to demonstrate any significant difference. As Baccaglini *et al*.[2] remind us, many oral-medicine studies have no statistical analyses, even though it is possible and informative to perform them. Many reports also lack basic univariate analyses, so that it is not clear to whom the results may be generalisable, or the analyses do not fully relate to the hypotheses or the study design.[2]

It is, therefore, useful for readers if Mahesh *et al*.[1] kindly add the statistical analysis on differences between pre-and post-treatment in "each" patient and between the two treatment methods. The differences of MGPS "alone" seem to be inadequate to conclude that one therapeutic method offers superior benefit or efficacy than the other. Another important point is that the statistical analysis of a "split-mouth" design is more complex than that of the "whole-mouth" design. It should capitalise on the within-patient correlation (ρ) and site effect.[3]

Moreover, mathematic calculation may help predict the recurrent pigmentation. It is possible that "baseline" pigmentation recurs after several months or years later, regardless of treatment methods. Considerably more researches are desirable to assess the recurrent pigmentation, especially after radiofrequency before using or recommending it as a routine practice. This information should be given to every patient before signing the consent form.

Fourth, the small sample size reduces statistical power. It is generally accepted that to detect a 50-80% relative efficacy benefit of one treatment over another, sample size in controlled therapeutic trials should range from 50 to 200.[4,5] A small sample size may not be able to detect some complications. Undesirable outcomes may be observed after more cases are collected. Recently, one nocturnal sudden cardiac death and one secondary bleeding after tongue base reduction with radiofrequency tissue ablation on 193 obstructive sleep apnoea patients were reported.[6] Ward *et al*.[7] reported a case of trigeminal neuralgia developing meningitis after percutaneous radiofrequency thermocoagulation at the Gasserian ganglion. A possible explanation is that the radiofrequency needle introduced oral commensal bacteria into the middle cranial fossa. Although

Mahesh *et al*.[1] demonstrated wonderful outcomes, further well-designed, prospective studies with more patients are necessary to confirm their study results, and treatment safety.

Fifth, in our experience, whichever a secondary cleft rhinoplasty technique was used, patient satisfaction was always high.[8-10] Objective or clinical improvement in the operator's eyes may not be linked to the patient's satisfaction/quality of life. Patients with obvious gingival depigmentation may not be satisfied when experiencing considerable amount of post-operative inconvenience or disability, such as pain, bleeding or inability to go to work. It would be interesting for future works to correlate patient's satisfaction/quality of life with therapeutic methods. Cost-effectiveness analysis of radiofrequency for gingival depigmentation also merits further exploration.

Lastly, ethical approval, financial support and conflicts of interest of this study were not mentioned. However, this is not surprising. Our recent investigative series have shown the lack of disclosures of human subject protection (obtaining ethical approval and subject's consent), financial conflicts, and academic-industry relationship in oral-maxillofacial surgery journals and innovations.[11-15]

The World Medical Association (WMA)'s Declaration of Helsinki,[16] which is also cited by this journal in its Instructions to Authors, states that "The research protocol must be submitted for consideration, comment, guidance and approval to a research ethics committee before the study begins" and "…They (authors, editors and publishers) should adhere to accepted guidelines for ethical reporting. Sources of funding, institutional affiliations and conflicts of interest should be declared in the publication. Reports of research not in accordance with the principles of this Declaration should not be accepted for publication".

The WMA also recommends that all members of the medical profession peruse its Declaration of Geneva and the United Nations Universal Declaration of Human Rights. According to these 2 ethical statements, we as medical personnel must maintain the utmost respect for human life and do not use our medical knowledge to violate human rights and civil liberties, even under threat. This suggests that mixing an innovation (with unknown long-term outcomes and hazards) with routine practice is less than ideal and, in many instances, unethical.[16-18]

Taken together, Mahesh *et al*.[1] presented an excellent analysis and interesting information on outcomes of an innovation, but their study results need to be interpreted with caution.

Poramate Pitak-Arnnop, Kittipong Dhanuthai[1], Niels Christian Pausch[2]

Department of Oral and Maxillofacial Surgery, UKGM GmbH, University Hospital of Marburg, Faculty of Medicine, Philipps University, Marburg, [2]Department of Oral, Craniomaxillofacial and Facial Plastic Surgery, Faculty of Medicine, University Hospital of Leipzig, Leipzig, Germany, [1]Department of Oral Pathology, Faculty of Dentistry, Chulalongkorn University, Bangkok, Thailand
E-mail: poramate.pitakarnnop@gmail.com

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