Outcome of Endodontic Surgery: A Meta-analysis of the Literature—Part 1: Comparison of Traditional Root-end Surgery and Endodontic Microsurgery

To the Editor:

This letter is written in response to the review article published by Setzer et al in the November 2010 issue (J Endod 2010;36:1757–65) on the outcome of endodontic surgery. The authors should be applauded for their efforts to thoroughly review the literature in order to summarize for readers the evidence base supporting the outcome of endodontic surgery. Such initiatives should be encouraged to promote a culture of evidence-based practice, which in essence is "…the conscientious, explicit and judicious use of CURRENT BEST EVIDENCE in making decisions about the care of individual patients" (1).

The authors rightly focus their research question on the comparison between two main approaches to apical surgery: traditional (ie, root-end cavity preparation with burs, root-end filling with amalgam, and the use of no or low magnification) and microsurgical (ie, root-end cavity preparation with ultrasonics; root-end filling with IRM, super-EBA, or mineral trioxide aggregate; and the use of a microscope or endoscope for magnification and illumination). This comparison unequivocally yields better outcomes for the latter than the former. In this sense, the review not only highlights the current evidence, but it also shows why this evidence (and not outdated studies) is key to evidence-based practice.

The aforementioned value of this systematic review notwithstanding, it falls short on the inclusion criteria and interpretation of studies. The resulting outcomes reported for endodontic microsurgery are likely to be overestimated, which can mislead readers to develop unrealistic expectations.

To this end, the authors disregard the requirement for the best evidence to support evidence-based practice. The level of evidence of any given study critically depends on the study's methodological rigor (2). Yet, while attempting to conduct a rigorous search, the authors ignored critical methodological limitations of the studies they selected as evidence for endodontic microsurgery. One key methodological requirement is rigorous outcome assessment to minimize measurement bias (3). Outcomes must be assessed by blinded and calibrated examiners to ascertain consistency and objectivity, and the follow-up period must be sufficiently long to detect the outcome of interest and the potential risks. Another key methodological requirement is rigorous assembly of the study sample (patients) without bias for a favorable or unfavorable outcome. Unfortunately, all nine studies the authors selected as evidence for endodontic microsurgery do not satisfy these key methodological requirements as specified below.

Six of the nine selected studies report outcomes after follow-up periods of 1 year or less, considering Rubinstein and Kim's strategy to classify teeth considered healed at 3 or 6 months as success without further follow-up (4). In a seventh study (5), 40% of the cases included were observed for just 1 year. The review authors appear to disregard what has been well documented in the literature; although healing peaks in the first year after apical surgery, a reversal to disease occurs in 5% to 25% of the apparently healed cases within 4 years after treatment (6–11). Thus, the long-term outcomes sought by patients and treatment providers as evidence are expected to be lower than those presented by the review authors based on the short-term studies they selected.

In one of the studies (12), the authors state in the Discussion that "In this study, before surgery was performed, if an existing root filing was deemed inadequate, it was replaced." Elsewhere, the same authors state that in 23 patients "preparatory nonsurgical treatment was successful." These comments suggest that a portion of the study sample had retreatment performed before surgery (personal communication with an endodontist involved in the study suggested that in fact, the majority of teeth were previously retreated, but a query to this effect sent two years ago to Dr. Chong has not been answered). It should be considered that in some or many of the previously retreated teeth, there could be "extraradicular infection, such as bacterial plaque on the apical root surface or bacteria within the lesion itself" as described by the authors of the systematic review. In these specific clinical situations, apical surgery predictably eliminates the source of infection resulting in complete healing in over 90% of cases as shown by Zuolo et al (13). The outcome in these specific cases is not applicable to the common cases when apical surgery is performed after only initial root canal treatment in which the source of the infection is most likely within the filled root canals and the treatment aims to seal the infected canal with the root-end filling. The proportion of teeth with previous retreatment was not specified in any of the nine selected studies, which is a methodological oversight. The chance that previously retreated teeth were included in the samples studied, possibly in high proportions like those implied for Chong et al (12), suggests that the outcomes of surgery performed after only initial treatment with persistent root canal infection (which is the most common case) may be lower than reported in the selected studies reviewed.

Although the review authors suggest that they excluded "studies based on population that was part of an earlier publication" (exclusion criterion 12), they may have misinterpreted the three publications by the Italian group of Taschieri et al in 2005, 2006, and 2008 (14–16), which apparently have repeated a previous population in subsequent articles. Testori et al (15) in their 2006 article state that "all patients requiring surgical treatment were recruited during a period of 22 months from December 2001 to October 2003," and Testori et al (16) in their 2008 article state that "all patients requiring endondontic surgical treatment were recruited during a period of 36 months from December 2001 to December 2004." The overlap in recruitment periods suggests that the 39 teeth reported on in 2006 (15) were also included among the 50 teeth reported on in 2008 (16). No deductions can be made on the 2005 report of the same group (14), but because it is a preliminary report, it can be assumed that their 28 teeth were also included in the subsequent reports. Thus, at best, only one of the three Italian articles should be selected as evidence, which will lower the relative weight of the Italian group's reported outcomes in the range of 91% to 95%.

Finally, there is a major concern with regards to the classification of the outcomes observed in the majority of the studies selected by the review authors as evidence for endodontic microsurgery. According to Rud et al (17) and Molven et al (18), incomplete healing (repair with fibrous scar tissue) is considered as success, but its incidence is low; Molven's group reports an incidence of 7% (19) and Jensen et al (20), Yazdi et al (11), and Wesson and Gale (10) all report an incidence of 5% of cases classified as incomplete healing. Normally, researchers report results with a breakdown of the four categories of outcome (complete, incomplete, uncertain, and unsatisfactory healing), but in four of the studies selected by the reviewers as evidence for endodontic microsurgery (4, 14–16), the success is reported without a breakdown of complete

and incomplete healing. In a further three of the studies, incomplete healing ranged from 13% to 27% (5, 12, 21), which is considerably higher than the incidence suggested elsewhere (10, 11, 19, 20). Why the concern? Cases that present with uncertain or unsatisfactory healing (failure) can easily be misclassified as scars (incomplete healing = success) (22). An example of such misclassification is evident in the article by Rubinstein and Kim (9) in which Figure 5 shows a mandibular first premolar originally classified as scar but later recognized as persistent apical periodontitis. A similar misclassification is also reported by Molven et al (23). The possibility cannot be ignored that the success reported for endodontic microsurgery based on the studies selected is increased by 5% to 20% because of failed cases misclassified as scars and grouped as successful.

In summary, the authors claim that "the…endodontic microsurgery…success rate of 93.52% lies…in the range…presented…for primary endodontic treatment without periapical lesion," suggesting that endodontic microsurgery (usually performed on teeth with infected root canals) is as successful as root canal treatment in teeth with noninfected canals (no apical periodontitis) and implying that it is more successful than root canal treatment and retreatment of teeth with infected canals. This conclusion is as presumptuous as it is misleading. The results of the systematic review, at best, can be considered to suggest the short-term outcome of endodontic microsurgery, overestimating the actual long-term outcomes that need to be considered as evidence to support surgical treatment versus alternatives. Even the short-term outcome reported may be inflated when the possibility of cases that are not healed but misclassified as scar (success) is taken into account. It is reasonable to assume that the realistic long-term outcome is 10% to 15% lower and thus expected to be in the range of 80% to 85%. This is still a better outcome than reported for the traditional approach, and it is likely to be attainable in the long-term, unlike the outcome suggested by the review authors.

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Reply to Dr Friedman

To the Editor:

We have to thank Dr Friedman for the time and effort spent to address methodology and study design of the systematic review and meta-analysis on the outcome of endodontic surgery we published in the November 2010 issue of the *Journal of Endodontics*. Specifically, Dr Friedman puts forward critical remarks on the expected outcome of root-end surgery with modern techniques such as high-power magnification and illumination, ultrasonic root-end cavity instrumentation, and root-end filling with mineral trioxide aggregate, Super-EBA, or intermediate restorative material; in the following reply they are summarized and abbreviated as endodontic microsurgery.

Our study methodology complies with the AMSTAR criteria for systematic reviews as described by Shea et al (1). Before submission, the AMSTAR score was used for self-assessment of the manuscript. Suebnukarn et al (2) published a systematic evaluation of the quality of meta-analyses in endodontics in the *Journal of Endodontics* in 2010 by using the AMSTAR scoring system. Compared with the meta-analyses investigated in this publication, the manuscript showed an AMSTAR score of 10 out of the maximum score of 11, matching the highest score achieved by only 1 of the total 16 meta-analyses under examination.

All studies included in the meta-analysis of the cumulative success of endodontic microsurgery are eligible and well-suited for systematic review. These publications derive from well-respected, peer-review research journals and are exclusively either prospective studies or randomized controlled trials, therefore best evidence. Studies of identical