Lasers in Periodontics


In response to the above-mentioned article, I would like to present a different point of view on numerous issues concerning the utility of lasers in periodontal therapy based on evidence in the literature.

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The author states in the abstract that the purpose of the paper was “to determine the state of the science concerning the application of lasers to common oral soft tissue problems, root surface detoxification, and the treatment of chronic periodontitis.” However, he did not address many issues, such as detection and removal of calculus, reduction of periodontopathogens in the pocket, control of the junctional epithelium migration, treatment of gingival overgrowth, and management of peri-implant lesions. These subjects should be included in a comprehensive review paper on laser therapy. The author indicates that studies dealing with “commercial laser technology” were not incorporated into this paper. This is a sensitive issue because all trademarked products used professionally are in a sense tainted commercially, because they were developed by industry. However, an independent literature review should include all studies based upon their merit. The author lists different available laser wavelengths with applications in clinical dentistry and periodontics (Table 3). However, there are other wavelengths with clinical applications in dentistry, such as the 980-nm diode laser (periodontics, oral surgery, and implantology) and the 9.6-μm carbon dioxide (CO₂) laser (decay removal), as well as photodynamic therapy (PDT), that are useful in different disciplines (oral medicine and periodontics). Wound healing studies (including studies from our research group) that demonstrated delayed healing in extraoral (and not intraoral) wounds (rat skin) should not be included in this literature review for the field of periodontics because there are differences with respect to tissue responses and wound contraction in the skin. In these studies, the rate of wound healing after the neodymium-doped-yttrium, aluminium, and garnet (Nd:YAG) laser irradiation and scalpel incisions was similar if the power parameters were used properly. However, there was no or less skin pigmentation in the lased tissues of low power compared to the scalpel incisions. Some other studies related to wound healing of intraoral tissues after laser therapy also were not addressed. These studies pertaining to rat mucosa demonstrated (using immunohistochemical techniques) that when laser therapy was compared to scalpel incisions, the connective tissue matrix responded significantly better to laser therapy. Histologic documentation also showed that after laser therapy, there initially was less rapid reepithelialization because of factors such as reduced inflammatory tissue response, but the final wound tissue had less wound contraction. Other investigators addressing intraoral wound healing also showed less contraction after CO₂ laser wounds compared to other surgical methods. With regard to bacterial reduction using adjunctive laser therapy, publications by Ben Hatit et al., Moritz et al., and Gutknecht et al. showed statistically significant reduction of periodontopathogenic bacteria in the laser-assisted therapy compared to conventional treatment. Laser treatment alone does not replace conventional scaling and root planing (no significant reduction of the bacteria), but it may be an adjunct to classic periodontal therapy. Calculus removal is important in the treatment of periodontitis. However, only selective power parameters and laser wavelengths, like the erbium-doped:YAG (Er:YAG) and erbium, chromium-doped:yttrium, scandium, gallium, and garnet (Er,Cr:YSGG), may be used for calculus removal. In contrast to what was stated in the review paper, the defocused, non-contact mode of the laser and not very high power parameters can be effective for calculus removal without damaging root cementum. This was demonstrated by Aoki et al. using 30 mJ per pulse and 10 Hz frequency effectively with the Er:YAG laser. They were able to remove calculus selectively without damaging the cementum. Removal of epithelium in the pocket has been documented after laser irradiation using different laser wavelengths. In general, in periodontics, several studies have focused on the control of epithelial migration as it relates to wound healing. However, it is not scientifically proven that control of epithelial migration is a key factor for connective tissue regeneration. Other authors stated that stabilization of the coagulum is important to get regeneration of the periodontium. In this regard, coagulation and clot stabilization may be enhanced by the different laser systems. This is due to better absorption by hemoglobin, based on the physical properties of the correct wavelength and the tissue interactions. Independent of the laser technology (laser equipment or also laser wavelength), authors such as Gold and Vilardi using an Nd:YAG laser and Romanos et al. with the 980-nm diode laser, demonstrated better removal of the pocket epithelium com-
pared to conventional techniques. In addition, Rossman et al.9 and Israel et al.8 were able to control epithelial migration into the periodontal tissues using the CO2 laser. This also was demonstrated histologically in monkeys9 and in block sections of humans.10 In addition, the group of diode lasers (810 and 980 nm) represents laser wavelengths with different applications and specific physical properties, which were not addressed in this paper. There are also high- and low-intensity diode lasers with applications supporting the use of lasers in the healing of intraoral wounds.17 The mechanisms of such wavelengths are complex and have to be studied more extensively, but they seem to have good effects on periodontal tissues. Specifically, the use of low-intensity 670-nm diode laser in PDT demonstrated positive results in a clinical study with 30 periodontitis patients.18 This laser wavelength and the concept of PDT were omitted from the review paper. Gingival overgrowth, which may be a side effect of medications, can be removed effectively with the CO2 laser.9 For patients with large hyperplasias, the CO2 laser is the definitive treatment of choice. Furthermore, lasers can make treatment of hospital-based patients safer, by avoiding bleeding, and simpler; they also can be used in private practice, resulting in a reduction in the cost of dental services.20 Another area of periodontology is implant dentistry. The author presents the “second-stage exposure of dental implants” as a soft tissue application of the laser. However, there are other applications of laser therapy, like decreasing bacteria on implant surfaces21 without increasing the temperature of the implant.22 In addition, the laser can be used to decontaminate the implant surface without damaging it.23,24 Important, and not addressed in the review paper, is the fact that successful treatment of peri-implant defects and reosseointegration have been documented histologically after CO2 laser irradiation25–27 as well as clinically with the CO2 laser and the 980-nm28 and 810-nm29 diode laser.30 In conclusion, according to the author’s selection criteria for “evidence-based laser dentistry,” 278 articles were selected for evaluation for this review; however, only 120 articles (<50% of the “evidence”) were addressed. It is my opinion that the articles selected in this literature review do not adequately represent the body of knowledge that is available regarding the application of lasers in periodontics. In this regard, readers need to analyze the literature critically. Because of time and space limitations, it is not possible to review the literature comprehensively in a letter format. I hope I have demonstrated that additional information is available that can and should affect one’s opinion concerning the use of lasers in periodontal therapy. I agree with Dr. Cobb that we need more randomized, well-controlled, blinded, multicenter clinical trials to gather additional evidence pertaining to the efficacy of laser therapy before lasers become the gold standard in daily practice. Finally, let us remember that science is the search for truth, and it is dynamic and constantly changing; in this regard, it is important that we keep an open mind to emerging technologies and apply therapies that are best for our patients.

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References
Author's Response

In reply to Dr. Romanos’ letter, allow me to make a few observations. First, I would be remiss not to thank Dr. Romanos for taking the time and effort to write such a detailed letter and critique of the review. Second, it seems appropriate to note that the literature review manuscript was submitted to the Journal of Periodontology in November 2005 and was published in April 2006. Consequently, as I write this reply (January 2007) the review is already 14 months out of date, making this exchange of letters to the editor rather academic. Several reviews of the literature have been published in recent years, including the one in question, all having some defect of coverage and detail.1–3 In defense of those who assume the responsibility for writing an extensive review of the literature, I would suggest that several constraints must be considered by the author. Two obvious constraints are the number of published articles associated with the subject (many of which repeat previously published information) and the length of the resulting manuscript. Usually, the latter is determined by the depth and detail of analysis and author-imposed limits on subject areas. In addition, the author may choose, as I did, to limit the extent of the review by imposing time restrictions on the historical search; e.g., the review in question limited articles to those published between 1990 and 2005. Obviously, one could choose to write a book on the subject of lasers and their dental applications in general, or, more specifically, in periodontics. Such was not my intention. Dr. Romanos begins by correctly noting the lack of coverage concerning detection and removal of calculus, control of junctional epithelium migration, treatment of gingival overgrowth, and management of peri-implant lesions. In addition, I would bring to Dr. Romanos’ attention that the review also did not cover low-level laser therapy (LLLT), PDT, or laser therapy of specific mucous membrane pathologies. Thus, one might say the review is what it is. I consciously placed limits on the consideration of specific subject areas and chose not to review these particular topics. The three journal-assigned reviewers of the original manuscript apparently agreed with my decision because they did not suggest that these areas be considered for inclusion in the review. Concerning the statement about studies dealing with “commercial laser technology,” I think that Dr. Romanos misinterpreted my intention. I was referring to papers that dealt with specific mechanical design features, advantages of one laser wavelength over another for use in private practice, and articles reporting various applications in the format of uncontrolled case reports, all commonly sponsored by a commercial vendor. In my opinion, this approach is appropriate for a peer-review publication. Dr. Romanos correctly notes that Table 3 in the review is inaccurate to the extent that it does not list the 980-nm diode or 9.6-μm CO2 lasers. The semiconductor diode lasers are listed under the heading of laser type and include the 980-μm wavelength (gallium, aluminum, arsenide [GaAlAs]). However, the range of diode wavelengths is listed as 635 to 950 nm, which obviously is not correct. I take full responsibility for this oversight. The 9.6-μm CO2 laser was not listed because, as Dr. Romanos states, it is used primarily for the removal of dental caries. One may argue correctly that Table 3, as titled, should have included the 9.6-μm CO2 laser. However, I assumed that the title of the literature review implied that only those lasers with application to periodontics would be considered. The 980-nm diode laser is manufactured in Germany and is readily available to the European community. I am not aware of the American market endorsing the instrument through its use and application. In a recent “Buyers’ Guide to Dental Lasers” published in the December 2006 issue of Dentistry Today, the 980-nm diode laser was not listed. Thus, it has yet to make a significant entry into the American market. With regard to wound healing studies, one may argue, as Dr. Romanos states, that the cited healing studies involving rat skin should not have been included in the review because of differences in histology of skin and oral mucosa. However, if one excludes those studies involving skin, the literature is left with few laser wound healing studies that specifically involve the oral soft tissues. Despite specific histologic differences, skin and gingiva are remarkably similar with regard to density and collagen types found in the subepithelial connective tissues. Obviously, minus the lamina lucida, hair follicles, and specific hyperkeratotic areas found in skin, the epithelial layers of skin and gingiva are very similar. Further, the Fisher et al.4 and Luomanen et al.5 studies referred to by Dr. Romanos were published in 1983 and 1987, respectively, and lie well outside of the stated time limits of the review (1990 to 2005). The Zeinoun et al.6 study involved CO2 laser wounding of dorsal rat tongue using 5 W of energy in a continuous waveform. The energy density derived from these parameters appears to be excessive and may have affected the infiltration of myofibroblasts into the wound area, and, therefore, the amount of wound contraction, or lack thereof. Ultimately, I chose not to include the Zeinoun et al.6 study for two reasons: 1) the dorsal tongue of the rat, or any other mammal, is not representative of the gingivae or alveolar mucosa. The dorsal tongue generally exhibits a less dense and significantly thinner lamina propria than do gingivae and alveolar mucosa; and 2) the existent tongue muscle layer likely affects healing of tongue wounds by providing a potential source of progenitor cells and an extensive muscle-associated blood supply not found in gingivae and alveolar mucosa. Dr. Romanos states that the Ben Hatit et al.7, Moritz et al.8 and Gutknecht et al.9 studies demonstrated statistically
significant reductions in the levels of periodontopathogenic bacteria following laser therapy versus conventional treatment. All three studies were referred to in Table 4 of the review. However, there are significant defects within each study that, in my opinion, affect their clinical significance. In fact, these three studies are good examples of what is wrong with much of the research involving lasers for periodontal therapy. The Ben-Hatit et al. study used an Nd:YAG laser at four different energy settings, i.e., 0.8, 1.0, 1.2, and 1.5 W, as an adjunctive treatment with scaling and root planning (SRP). The control consisted of SRP alone. The study reported significant reductions up to 10 weeks in Actinobacillus actinomycetemcomitans (Aa), Treponema denticola, Bacteroides forsythus (now Tannerella forsythensis), and Porphyromonas gingivalis following SRP and the adjunctive use of an Nd:YAG laser at 0.8 and 1.0 W but not at 1.2 and 1.5 W. When using the higher-energy settings (particularly 1.5 W), some bacteria increased and others decreased, suggesting a paradoxical result. Having said this, the experimental design does not lend itself to appropriate statistical analysis. The design used 14 patients with a total of 150 periodontal pockets. One hundred of these pockets were assigned randomly to four different laser treatment groups. By assigning pockets rather than patients and not accounting for within-subject variations, the design results in dependency among data points and does not account for possible nesting of treatment sites. This violates a fundamental concept in statistical analysis in that proper analysis requires independent data, not dependent. Thus, the study is reduced to descriptive analysis that inherently does not allow conclusions about the effectiveness of treatment, which, in turn, requires inferential statistical analysis. Almost a casual observation is that of the four bacteria evaluated, Aa seemed to be resistant to the effects of the laser. Based on the poor and inappropriate statistical analysis, the inconsistent result obtained at 1.5 W compared to the lesser energy settings, and the relatively short time span of the study (10 weeks), I chose to report a lack of clinical significance in Table 4 of the review. Lastly, it seems unreasonable to expect a patient to undergo laser therapy every 3 months to maintain a decreased level of periodontopathogenic bacteria. The Moritz et al. study, although a commonly cited article, has a fatal flaw in the design, namely that of inappropriate controls. Further, there was a poor distribution of test and control subjects, the former consisting of 37 subjects and the latter consisting of 13 subjects. Why not an equal distribution between test and control subjects? The diode laser (805 nm)-treated sites were treated at 1 week and 2 and 4 months following initial selection into the study, whereas the control group rinsed with hydrogen peroxide at the same time periods. In other words, the test group received subgingival treatment whereas the control group, over the same time period, received a form of supragingival therapy. In my mind, this type of experimental design introduces a severe bias favoring the diode laser. The paper makes no mention of a blinded or calibrated examiner. Lastly, if...
statistically significant results were obtained, the paper does not state it in those terms. I am not inclined to analyze their data to determine whether a significance exists. Data analysis is the job of the authors. Indeed, the data are basically descriptive with no apparent attempt at data analysis. Thus, Table 4 of the review lists the antimicrobial effect of the Moritz et al. study as demonstrating no significant difference between treatment groups. The Gutknecht et al. study stated that statistical significance was achieved at 1 week and 1 month, but not at 3 and 6 months post-laser treatment versus subgingival “curette.” Also reported was a lack of a significant difference in probing depths and bleeding on probing (BOP) between the two therapies, although the number of BOP-positive sites was consistently greater in conventional therapy sites. The study did not report using blinded or calibrated evaluators. Although the study used an elegant experimental four-quadrant design, the statistical analysis is severely flawed. The authors used a “monofactorial variance analysis” followed by unpaired t tests to compare pre- and post-treatment results within each treatment group and a further t test to compare post-treatment results between the laser-treated quadrants versus control. The experimental design, in fact, calls for a two-factor repeated measures analysis of variance (i.e., time and treatment). Multiple t tests in a multifactor design produce an inflated family-wise error rate. In this study, the multiple t test produced a probability of falsely rejecting the null hypothesis of \( \alpha = 0.05 \) (1–0.05) \( \alpha = 0.05 \) (1–0.05) \( \alpha = 0.05 \) or (0.05) (0.05) \( \alpha = 0.81 \), rather than the traditional 5%, which would have been the error rate for a repeated measures analysis of variance. In addition, given the periods of evaluation, i.e., 1 week and 1, 3, and 6 months, the latter two would, in my opinion, be the more important clinical intervals, unless of course one proposes to subject patients to repeated laser therapy at 3-month intervals. Thus, Table 4 of the review lists the antimicrobial results of the Gutknecht et al. study as showing no significant difference between treatment groups. Despite what I consider to be a lack of clinical significance in these three studies and several other clinical trials cited in the review, the aggregate of their results was the reason for the statement that “Current evidence does suggest that use of the Nd:YAG or Er:YAG wavelengths for treatment of chronic periodontitis may be equivalent to scaling and root planning (SRP) with respect to reduction in probing depth and subgingival bacterial populations.” I have received numerous verbal comments, e-mails, and letters stating that I was either too conservative or not conservative enough in making this statement. Obviously, it is difficult to satisfy all critics. With respect to laser removal of calculus, I would refer Dr. Romanos to page 550 of the review, the section labeled “Laser-induced root surface modifications,” first paragraph. I state, as does Dr. Romanos, that the Er:YAG laser is the “instrument of choice for effective removal of calculus, for root etching, and for creation of a biocompatible surface for cell or tissue reattachment. This latter statement is supported by Aoki et al. in their definitive review of the literature.” Thus, Dr. Romanos and I seem to be in agreement. I consciously chose not to review the literature concerning deep epithelialization of the periodontal pocket wall. The literature in this arena is scant, although of interest and possible significance. The fact remains that no human randomized, blinded, controlled, longitudinal clinical trials have been conducted that address this specific issue. Issues regarding the different wavelength diode lasers and their specific physical properties and soft tissue interactions, LLLT, PDT, and the use of lasers in implant dentistry purposely were not addressed. Regarding Dr. Romanos’ opinion that the percentage of cited articles in the review is insufficient to represent the body of knowledge, I respectfully disagree. Having read, evaluated, and made the choice not to include all of the available articles, in my opinion, does not change the conclusions of the review. Indeed, many of the articles not cited added no new information and simply repeated previously reported knowledge. It is redundant and unnecessary to include such articles simply because they exist. As Dr. Romanos has published extensively in the fields of dental lasers and dental implants, I might suggest he or one of his colleagues consider a review of the literature combining these two areas of interest. There seem to be a sufficient number of articles and interesting research regarding the application of various lasers to implantology to warrant such a review. Finally, I am relieved to find that Dr. Romanos and I can agree on several issues, e.g., the need for properly designed longitudinal clinical trials; that science is a search for truth; that science, and by extension truth, are dynamic and constantly changing; and the need for maintaining an open mind, ready to accept and apply new ideas and concepts based on good science to patient treatment.

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References
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