

## LETTERS

**J**ADA welcomes letters from readers on topics of current interest in dentistry. The Journal reserves the right to edit all communications and requires that all letters be signed. The views expressed are those of the letter writer and do not necessarily reflect the opinion or official policy of the Association. Brevity is appreciated.

### NITROUS OXIDE AND ALCOHOL

Dr. James P. Zacny and colleagues' January JADA article, "Preoperative Dental Anxiety and Mood Changes During Nitrous Oxide Inhalation," confirms the usefulness of analgesic nitrous oxide for treating anxious dental patients—an indication that has proved itself to be safe and effective over many years, provided the correct equipment and technique are used.<sup>1</sup>

It was, therefore, with some concern that I read in the article that "the initial concentration was determined largely on the basis of answers to questions related to alcohol use. For example, if a patient reported no alcohol use or very limited use ... the initial concentration was between 10 and 20 percent nitrous oxide ..." while with "... more consistent or nightly alcohol consumption, the initial concentration was between 30 and 40 percent nitrous oxide ... ."

We and others have been using analgesic nitrous oxide for treating alcohol<sup>2,3</sup> and other substance abuse withdrawal states<sup>4</sup> for many years. Although there is cross-tolerance between alcohol and other substances of

abuse and analgesic nitrous oxide,<sup>2,4</sup> we have found that this cannot be used as a general guide for estimating the initial or, for that matter, the final dose of nitrous oxide to produce comfortable conscious patients when using nitrous oxide sedation.<sup>3</sup>

Indeed, in our work with alcohol withdrawal, we have found that a concentration as low as 15 percent nitrous oxide in oxygen<sup>2</sup> can produce a good therapeutic effect even in heavy drinkers who could be classified as having alcohol dependence disorder.<sup>2,3</sup>

We and others<sup>1-4</sup> therefore consider it important to slowly and carefully titrate the concentrations of nitrous oxide to the patient's clinical requirements. One should not assume anything regarding the patient's requirements based on his or her alcohol consumption or, indeed, on any other criterion except the patient's response during titration.<sup>1-4</sup>

Careful titration avoids excessive sedation and/or dysphoria, which can, in some cases, produce excitation and even panic,<sup>1,2</sup> the exact opposite of the goal of a relaxed patient. Such patients may avoid further exposure to the gas, thus preventing them from obtaining the benefits of competent nitrous oxide sedation during future treatments.

It also is not clear from the article whether a dental anesthesia or dental sedation flowmeter was used. This should be stated because it is our view that, under usual clinical conditions, conscious sedation with nitrous oxide should only be administered using custom-designed equipment having a built-in fail-safe device.<sup>1-4</sup> The

use of such equipment should be obligatory. Although the technique has an unrivaled safety record,<sup>1</sup> it further enhances safety by reducing the likelihood of anesthesia and/or hypoxia.<sup>1-4</sup> The use of the term "dental anesthesia machine"<sup>5</sup> also confuses the concepts of anesthesia and analgesia, which are quite distinct states and which are far too often confused.<sup>4</sup>

It is for this reason that my colleague, F. J. Lichtigfeld, and I introduced the term "psychotropic analgesic nitrous oxide" so that it would be clear at all times that we were using a technique where the patient is conscious throughout administration of nitrous oxide for neuropsychiatric applications.<sup>4</sup>

**Mark A. Gillman, B.D.S.,  
M.Sc., D.Sc.**  
**Executive Director  
South African Brain  
Research Institute  
Johannesburg**

1. Clark MS, Brunick AL. Handbook of nitrous oxide and oxygen sedation. St. Louis: Mosby; 1999.

2. Gillman MA, Lichtigfeld FJ. Analgesic nitrous oxide for alcohol withdrawal: a critical appraisal after 10 years' use. *Postgrad Med J* 1990;66(777):543-6.

3. Gillman MA, Lichtigfeld FJ. Randomized double-blind trial of psychotropic analgesic nitrous oxide compared with diazepam for alcohol withdrawal state. *J Subst Abuse Treat* 2002;22(3):129-34.

4. Gillman MA, Lichtigfeld FJ. Opioid properties of psychotropic analgesic nitrous oxide (laughing gas). *Perspect Biol Med* 1994; 38(1):125-38.

**Authors' response:** One of Dr. Gillman's concerns was that the dentist based the initial nitrous oxide concentration solely on a patient's alcohol consumption. There was another criterion, too, and that was a patient's desire to be "in control." A person who was uncomfortable about not being in control was started out at a lower concentration.

Regardless of what concentra-

tion was used at the start, the dentist (L.G.) queried each patient five minutes after the patient inhaled the nitrous oxide/oxygen mixture to see how she or he was feeling. Based on the answers given, the dentist adjusted (that is, titrated) the nitrous oxide concentration to achieve a comfort level satisfactory to the patient. As we stated in the article, "sometimes the dentist increased or decreased the nitrous oxide concentration during the procedure, based on his assessment of the comfort level of the patient." The patient's alcohol consumption was not a criterion at that point.

A second concern raised by Dr. Gillman is that the dentist may have been using a machine lacking "built-in fail-safe devices" so that either hypoxia or anesthesia might have occurred with our patients. We used a Porter Analgesia Flowmeter Model MXR-1 (Porter Instrument Company Inc., Hatfield, Pa.) nitrous oxide-oxygen delivery system, with built-in fail-safe devices.

We erred in the article when we said the maximum amount of nitrous oxide that could be delivered was 60 percent; in point of fact it was 65 percent. Dr. Graham, as a matter of course, does not use nitrous oxide concentrations higher than 50 percent to avoid the risks of anesthesia.

**James P. Zacny, Ph.D.**  
**Department of Anesthesia**  
**and Critical Care**  
**The University of Chicago**  
**Lou Graham, D.D.S.**  
**Department of Surgery**  
**The University of Chicago**

#### **JADA INDUSTRY ADVISORY BOARD**

Despite all of the laudatory com-

ments praising the formation of the JADA Industry Advisory Board ("JADA Unveils Industry Advisory Board," News, May JADA), the proposed collaboration is a very bad idea for JADA and does ethical disservice to the profession of dentistry.

According to JADA Editor Marjorie Jeffcoat, the Board will serve to expedite publication of submissions from dental manufacturers to "enhance" delivery of new product information to practicing dentists.

There is little comfort in Dr. Jeffcoat's assurances that "these are not intended to be advertisements but will be peer-reviewed articles for the practitioner."

It is most surprising that the editor seems unaware of the many, well-documented shortcomings of the peer-review process. In fact, *The Journal of the American Medical Association* devoted its entire June 5 issue to the numerous problems associated with peer review.<sup>1</sup> Errors were common and "authors often failed to discuss limitations of their findings."<sup>2</sup> Editorially, *JAMA* declared that the system is "prone to abuse."<sup>3</sup>

When interviewed by the Associated Press, *JAMA* Editor Catherine DeAngelis said that "problems are most likely to occur in research funded by drug companies, which have a vested interest in findings that make their products look good." Concern was expressed by journal editors "that manufacturers sometimes unduly influence how researchers report study results, and even suppress unfavorable findings."<sup>4</sup>

This is exactly what happened when it was disclosed that researchers reporting on cyclo-oxygenase-2 inhibitors (an antiarthritis treatment) had

failed to provide complete research data in their article and repressed significant untoward information.<sup>5</sup> The reviewers, therefore, did not have access to the raw data when they reviewed the manuscript.<sup>6</sup> The then-published article was widely circulated and believed,<sup>5</sup> apparently giving the product an unjustifiable competitive advantage.

It is quite possible that, in the future, as a result of manufacturer channeling and undue reliance on a flawed peer-review process, Dr. Jeffcoat may be placing the imprimatur of our highly respected journal inappropriately on marginal or highly suspect scientific articles.

Dr. Jeffcoat seems particularly eager to move "time-sensitive" product material to practitioners. In my 40 years of practice, it has not been my personal experience that manufacturers have ever had any difficulty communicating by any and all available means in a most timely manner. As do most dentists, I have filled closets with products that did not perform as anticipated. Personally, I would prefer that JADA and its Advisory Board fast-track information to us about products that don't function as reported.

Finally, it must be pointed out that two-thirds of the members of the JADA Industry Advisory Board are employees of dental product manufacturers. While they may very well be outstanding individuals, it is their companies that put the clothes on their backs and the food on their tables. As a result, their primary allegiance must understandably be to their employers.

**Arthur L. Yeager, D.M.D.**  
**Edison, N.J.**

1. Peer Review Congress IV. JAMA 2002;287(theme issue):2739-898.
2. Altman LK. When peer review yields unsound science. New York Times. June 11, 2002. Available at: "www.nytimes.com/2002/06/11/health/policy/11DOC.html". Accessed Aug. 22, 2002.
3. Rennie D. Peer review in Prague. JAMA 1998;280(3):214-5. Cited by: Rennie D. Fourth international congress on peer review in biomedical publications. JAMA 2002;287(21):2759-60.
4. Tanner L. Medical journal gets self-critical. June 5, 2002. Available at: "www.cbsnews.com/stories/2002/06/05/health/main511139.shtml". Accessed Aug. 8, 2002.
5. Juni P, Rutjes AW, Dieppe PA. Are selective COX 2 inhibitors superior to traditional non steroidal anti-inflammatory drugs? BMJ 2002;324(7349):1287-8.
6. Okie S. Full study altered picture of drug. Washington Post. Aug. 5, 2001:A11.

**Editor's note:** We thank Dr. Yeager for his letter and appreciate his concerns. For the record, JADA uses several groups of experts—associate editors, the editorial advisory board, outside reviewers and the Industry Advisory Board—to help identify areas of interest to Journal readers. Regardless of the source of a manuscript—university, government, industry or clinician—the criteria for publication are the same. The Industry Advisory Board was set up to alert the editor of trends and for possible publication in JADA, not to make final publication decisions. The profession is crying out for data that underlie new procedures, drugs and devices. Only by seeing information from all sources can JADA help the clinician make informed decisions.

### TENS REVISITED

In their May JADA article, "Using Transcutaneous Electrical Nerve Stimulation to Prevent Postoperative Pain," Drs. Wayne Herman, Joseph Konzelman and Robert Comer neglected to mention some very important research. Although they are correct that transcutaneous electrical nerve stimula-

tion, or TENS, is widely used, these devices have not been found to work any better than placebo therapy.<sup>1</sup>

In 1997, Feine and colleagues<sup>2</sup> reviewed the scientific support for the use of a number of treatment options, including TENS, low-intensity laser, thermal agents such as hot and cold packs, and ultrasound. The study drew on research from both the medical and dental literature. The study concluded that "no good evidence exists that any of the treatments under review are capable of curing or even significantly reducing symptoms of chronic musculoskeletal conditions including TMDs [temporomandibular disorders]."

The well-established efficacy of placebos in the management of pain should make dentists suspicious of all anecdotal reports of pain reduction.

**John E. Dodes, D.D.S.  
Forest Hills, N.Y.**

1. Deyo RA, Walsh NE, Martin DC, Schoenfeld LS, Ramamurthy S. A controlled trial of transcutaneous electrical nerve stimulation (TENS) and exercise for chronic low back pain. N Engl J Med 1990;322(23):1627-34.

2. Feine JS, Widmer CG, Lund JP. Physical therapy: a critique. Oral Surg Oral Med Oral Pathol Oral Radiol Endod 1997;83(1):123-7.

**Authors' response:** We would like to thank Dr. Dodes for his interest in our article and the opportunity to comment on some of the issues that he raised.

Dr. Dodes refers to a 1990 article by Dedo and colleagues<sup>1</sup> that states that the effect of transcutaneous nerve stimulation, or TENS, for treatment of low-back pain is no greater than placebo. Since this article was written, a search of MEDLINE shows that an additional 700 articles have been published on the use of TENS, the large ma-

majority of which demonstrate efficacy.

While it is generally accepted that one can find articles in the literature to support almost any position that one wants, we would caution against placing too much importance on any single article and prefer to assess the aggregate body of literature.

We certainly support the concept of evidence-based practice and the need for placebo controls. This has proven difficult for TENS techniques, because it is very apparent to the patient when the device is active.

However, to cite some of the literature, in a Cochrane-based literature review of the effectiveness of TENS on knee osteoarthritis, Osiri and colleagues<sup>2</sup> concluded that TENS was effective in pain control over placebo.

Additionally, Ghoname and colleagues<sup>3</sup> designed a sham-controlled study with a randomized crossover design to test the analgesic response of TENS therapy for the treatment of low-back pain. While the sham-TENS treatments failed to produce positive changes, all the test frequencies of TENS produced significant decreases in the severity of pain, increases in physical activity, improvements in the quality of sleep and decreases in oral analgesic requirements.

In a subsequent article to the one cited, Feine and Lund<sup>4</sup> reviewed articles and controlled clinical trials for temporomandibular disorders and similar chronic musculoskeletal pain disorders. They concluded that "although little evidence was found that any specific therapy had long-term efficacy greater than placebo, we did find strong

evidence that symptoms improve during treatment with most forms of physical therapy, including placebo.” In effect, they indicated that no form of physical therapy was better than placebo, but patients did improve with treatment.

The comment about relying on anecdotal reports for pain reduction is problematic. Pain by its very nature is subjective, and reduction in pain effectively can only be ascertained by subjective inquiry. While visual analogue scales and other such devices attempt to objectify the patient information, in the final analysis, any report of reduction of pain is subjective input.

Finally, we would like to point out that our article was simply a description of a preventive technique to avoid muscular pain from lengthy dental procedures. We did not attempt to review the TENS literature in its entirety, nor did we advocate routine use of TENS for treatment of pain in the head and neck area. We do believe, though, that the literature clearly supports the use of TENS for relief of chronic pain and treatment of musculoskeletal disorders.

We hope that these comments adequately address the raised concerns.

**Wayne W. Herman, D.D.S.,  
M.S.**  
**Associate Professor**  
**Department of Oral**  
**Diagnosis and Patient**  
**Services**  
**School of Dentistry**  
**Medical College of**  
**Georgia**  
**Augusta**

1. Deyo RA, Walsh NE, Martin DC, Schoenfeld LS, Ramamurthy S. A controlled trial of transcutaneous electrical nerve stimulation (TENS) and exercise for chronic low back pain. *N Engl J Med* 1990;322(23):1627-34.

2. Osiri M, Welch V, Brosseau L, et al. Transcutaneous electrical nerve stimulation for knee osteoarthritis. *Cochrane Database Syst Rev* 2000;(4):CD002833.

3. Ghoname ES, Craig WF, White PF, et al. The effect of stimulus frequency on the analgesic response to percutaneous electrical nerve stimulation in patients with chronic low back pain. *Anesth Analg* 1999;88(4):841-6.

4. Feine JS, Lund JP. An assessment of the efficacy of physical therapy and physical modalities for the control of chronic musculoskeletal pain. *Pain* 1997;71(1):5-23.

## CURING LIGHTS

I read Dr. Gordon Christensen’s June JADA article, “The Curing Light Dilemma,” with great interest. I agree that there is confusion among practitioners regarding the optimal light-curing technique to utilize in their restorative procedures (high energy, low energy, ramped, stepped, pulsed, light-emitting diode and so forth).

Studies show that when the total energy (joules) delivered into a composite is the same—whether quickly for short periods or slowly over longer periods—the physical properties of the polymerized composite are the same.<sup>1,2</sup> This holds true for clinically relevant exposure times and power densities. The total energy delivered is determined by multiplying exposure time by the power density of the light source (milliwatts per square centimeter).

The relevant question is whether direct composite restoratives can be polymerized quickly, with high-energy light sources, without untoward effects on materials and/or on biophysical properties of the teeth. If the answer is yes, then it would seem to make sense to use the fastest means possible to initiate and complete polymerization since it saves us time. If composite materials did not shrink, and assuming the device delivering the initiating en-

ergy does not generate potentially harmful heat, then there would be no controversy. Faster would be better.

But composite restorative materials do shrink (generally 2 percent to 5 percent by volume), and some devices do generate considerable heat; herein lies the controversy. Furthermore, the wavelength of the light delivered, the specific wavelength sensitivity of the photoinitiators, monomer reaction speed, modulus of the composite restorative and other variables also must be taken into consideration.

I disagree with Dr. Christensen’s assertion that “current clinical usage, as well as research, have disproved the allegations of damage caused by the faster lights.” In fact, it all depends on whose research you look at. With all due respect to Dr. Christensen, he cites only one reference (his own Clinical Research Associates study<sup>3</sup>) to support his contention that high-intensity lights do not have an adverse effect on the integrity of bonded composite restorations and/or tooth tissues.

While there is equivocation in the literature, the majority of studies I’ve read (a sampling is cited here<sup>4-12</sup>) and presentations at the most recent International Association for Dental Research/American Association for Dental Research meeting,<sup>13,14</sup> indicate clear advantages when utilizing lower-energy/longer-duration exposure times in terms of reducing shrinkage stress, marginal integrity and gap formation (at least in the laboratory).

I also think it’s irresponsible and misleading to suggest to clinicians that about three sec-

onds is all that is required to cure composites with high-intensity light sources such as plasma arc curing, or PAC, lights. The fact is, there are a number of composites (and some adhesive systems) that will not cure with high-intensity light sources like PAC lights and argon lasers due to photoinitiator and light wavelength incompatibility.

Even when they are compatible, many studies have shown that three seconds of light curing is inadequate for many composite restoratives.<sup>15-17</sup> Also, what if the light tip is a significant distance (as is often the case) from the surface of the composite during polymerization? Is three seconds enough in this scenario? One thing I know for sure is that composites cannot be overpolymerized, but they can definitely be underpolymerized.

What no one knows, including Dr. Christensen, is what the long-term clinical ramifications of high-intensity/short-duration polymerization will be. I am unaware of any long-term clinical studies that address this issue.

However, I do agree that anecdotal evidence in the short-term seems to support the use of high-intensity lights in certain situations. I have found, with some exceptions, that those using high-intensity lights are generally happy with the systems. But what will their restorations look like in five or 10 years? Dentistry is replete with techniques and materials that looked favorable initially, only to demonstrate significant problems and unacceptable failure rates as time went by (for example, all-ceramic bridges).

My intent here is not to impugn the beliefs of Dr. Christensen or refute his cited Clinical Research Associates study. I simply wish to make clear that the jury is still out on this issue. In fact, I know very intelligent and reasonable individuals who argue quite passionately and convincingly on both sides of this controversy. The objective individual recognizes and acknowledges equivocation when it exists despite his personal preference or beliefs.

**Gary Alex, D.M.D.  
Huntington, N.Y.**

1. Nomoto R, Uchida K, Hirasawa T. Effect of light intensity on polymerization of light-cured composite resins. *Dent Mater J* 1994;13(2):198-205.
2. Suh BI, Cripe CA, Yin R. Light intensity and exposure time effects on light-cured composites (abstract 73). *J Dent Res* 1998;77 (special issue B):641.
3. Clinical Research Associates. LED resin-curing lights: 2002 update. *CRA Newsletter* 2002;26(3):3.
4. Burgess JO, DeGoes M, Walker R, Rippas AH. An evaluation of four light-curing units comparing soft and hard curing. *Pract Periodontics Aesthet Dent* 1999;11(1):125-32.
5. Unterbrink GL, Muesner R. Influence of light intensity on two restorative systems. *J Dent* 1995;23(3):183-9.
6. Kinomoto Y, Torii M, Takeshige F, Ebisu S. Comparison of polymerization contraction stress between self- and light-curing composites. *J Dent* 1999;27(5):383-9.
7. Mehl A, Hickel R, Kunzelmann KH. Physical properties and gap formation of light-cured composites with and without 'soft-start-polymerization.' *J Dent* 1997;25(3-4):321-30.
8. Koran P, Kurschner R. Effect of sequential versus continuous irradiation of a light-cured resin composite on shrinkage, viscosity, adhesion, and degree of polymerization. *Am J Dent* 1998;11(1):17-22.
9. Kanca J III, Suh B, Vinson W. Pulse activation of resin composite: reducing stresses at cavosurface interfaces (abstract 678). *J Dent Res* 1998;77(special issue B):190.
10. Uno S, Asmussen E. Marginal adaptation of a restorative resin polymerized at a reduced rate. *Scand J Dent Res* 1991;99(5):440-4.
11. Price RB, Bannerman RA, Rizkalla AS, Hall GC. Effect of stepped vs. continuous light curing exposure on bond strengths to dentin. *Am J Dent* 2000;13(3):123-8.
12. Sahafi A, Peutzfeldt A, Asmussen E. Effect of pulse-delay curing on in vitro wall-to-wall contraction of composite in dentin cavity preparations. *Am J Dent* 2001;14(5):295-6.
13. Shinkai K, Katoh Y. Effect of light intensity on cavity adaptation of composite restoration (abstract 3136). *J Dent Res* 2002;81(special issue A):A-389.
14. Feng L, Suh BI. Why slower polymer-

ization of a dental composite produces a lower stress? (abstract 2448) *J Dent Res* 2002;81(special issue A):A-309.

15. Park SH, Krejci I, Lutz E. Micro-hardness of resin composites polymerized by plasma arc or conventional visible light curing. *Oper Dent* 2002;27(1):30-7.

16. Sharkey S, Ray N, Burke F, Ziada H, Hannigan A. Surface hardness of light-activated resin composites cured by two different visible-light sources: an in vitro study. *Quintessence Int* 2001;32(5):401-5.

17. Hofmann N, Hugo B, Schubert K, Klaiber B. Comparison between a plasma arc light source and conventional halogen curing units regarding flexural strength, modulus, and hardness of photoactivated resin composites. *Clin Oral Investig* 2000;4(3):140-7.

**Author's response:** I appreciate the opportunity to respond to Dr. Alex's letter. Dr. Alex has expressed his beliefs, and I respect them. I agree that no one knows the long-term effects of high-intensity resin curing. Clinical Research Associates' well-controlled study that I cited is the only real-world study to date. Using well-proven restorative resins cured with high- or low-intensity curing lights, the study shows no clinical differences in restoration quality over one year as placed by practicing dentists in day-to-day practice. The study has now been observed for 18 months with the same results.

At this time, tens of thousands of resin restorations have been cured with high-intensity plasma-arc curing lights by dentists around the world. Additionally, high-intensity lasers have been used for over a decade to cure resin. We are still awaiting any major negative clinical reports. How long do we have to wait to prove the value of this concept? If we wait only a short time, the slight polymerization shrinkage of resins will be reduced even more, and the entire concept will be a moot subject.

I agree that the profession needs to watch this and many other concepts carefully to de-

termine long-term results. In the meantime, there must be more significant challenges to which we can devote our attention than the yet unseen, but alleged problems with high-intensity curing lights.

If there is a legitimate clini-

cal problem with a technique or material, I am well-known to be among the first to announce that problem. In the meantime, relative to high-intensity curing lights, a long-used phrase from the CRA Newsletter still overrides theo-

ry and in-vitro studies: "Clinical Success Is the Final Test!"

**Gordon J. Christensen,  
D.D.S., M.S.D., Ph.D.  
Diplomate, American  
Board of Prosthodontists  
Provo, Utah**