LETTERS

JADA welcomes letters from readers on articles and other information that has appeared in The Journal. 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.

TO PROBE OR NOT TO PROBE

November JADA's Point/Counterpoint article, "Should a Dental Explorer Be Used to Probe Suspected Carious Lesions?," brings up an important topic (Hamilton JC, Stookey G. JADA 2005;136:1526-32). The topic is important because of the common use of an explorer (or probe, in British terminology) in the examination and treatment planning of dental patients. The essential difference between the two views expressed focuses on the pressure used on the explorer when examining for caries.

Dr. Hamilton correctly quotes statements from two of my publications,[1,2] but in a way that makes me seem to be a supporter of the use of an explorer for the diagnosis of secondary/recurrent carious lesions. It should be noted that I merely reported what clinicians used, rather than expressing an opinion about the suitability of using an explorer.

If anyone is interested in my view relating to the use of an explorer for diagnosis of secondary/recurrent caries, they should consult my October JADA article,[3] in which I state on page 1429: "It is important in this situation to keep in mind that an explorer will stick in any crevice, regardless of whether it is carious." These views also were expressed in the other two publications referred to by Dr. Hamilton.

So, where do I stand on the issue of using an explorer in diagnosing carious lesions? I believe it will be used by clinicians in the foreseeable future, much the way it was used in the past, but hopefully with less force than most clinicians have tended to exert in the past.

Ivar A. Mjör, DDS, MSD, MS, DrOdont
Professor, Academy 100 Eminent Scholar
College of Dentistry
University of Florida
Gainesville

1. Mjör IA. Frequency of secondary caries at various anatomical locations. Oper Dent 1985;10:88-92.
2. Mjör IA. The location of clinically diagnosed secondary caries. Quintessence Int 1998;29:313-7.
3. Mjör IA. Clinical diagnosis of recurrent caries. JADA 2005;136:1426-33.

INFORMED CONSENT

I want to voice my appreciation and express my agreement with Dr. Daniel Orr II and Mr. William Curtis and their November JADA article, "Obtaining Written Informed Consent for the Administration of Local Anesthetic in Dentistry" (JADA 2005;136:1568-71). The authors have successfully enlightened dental practitioners of the need to inform patients of the potential complications of administering local anesthetic. The current liability climate of practice in the United States mandates that informed consent be obtained for all treatment. The authors have provided advice that is correct and long overdue.

Bernard B. Dreiman, DDS
Marion, Ind.

RISK VERSUS COST?

I am writing in reference to November JADA's "Obtaining Written Informed Consent for the Administration of Local Anesthetic in Dentistry" (Orr DL II, Curtis WJ. JADA 2005;136:1568-71). There has to be a limit. We need to inform patients of potential risks where there is a reasonable chance of a "serious" adverse outcome. I do written informed consents for endodontic treatments and surgery. These take 10 to 15 minutes to do properly.

If I were to do the same with local anesthesia, I would lose two to three hours out of every day in this unproductive process. Informed consent is necessary when there is a significant chance of problems or where potential problems are devastating. General anesthesia in oral surgery offices resulted in one death in 740,213 cases of anesthesia from 1988 through 2003 (OMS National Insurance Company claims data, February 2004). Two other studies reported one death in 671,428 cases of anesthesia[1] and no deaths.[2]

If I were a patient going under general anesthesia, I would want to know this, so I could make an informed decision as to whether I would want general anesthestic.

The risk associated with local anesthesia is probably lower than the risk of experiencing an auto accident on the way to the dental appointment. It is slightly greater than being struck by a meteor. Should we not include the risk of driving and meteors

in our informed consent?

We must balance risk versus costs. In the case of local anesthesia, the risks are so low you cannot seriously suggest it is necessary.

Fred Quarnstrom, DDS
Seattle

1. Lytle JJ, Stamper EP. The 1988 anesthesia survey of the Southern California Society of Oral and Maxillofacial Surgeons. J Oral Maxillofac Surg 1989;47:834-42.
2. D'Eramo EM. Morbidity and mortality with outpatient anesthesia: the Massachusetts experience. J Oral Maxillofac Surg 1999;57:531-6.

PRACTICE IMPLICATIONS

I enjoyed the November JADA article by Dr. Daniel Orr II and Mr. William Curtis regarding "Obtaining Written Informed Consent for the Administration of Local Anesthetic in Dentistry" (JADA 2005;136: 1568-71). My congratulations to the authors for a very informative and interesting article.

The authors did an excellent job of providing current information regarding informed consent. However, I disagree with the practice implications that dentists may want to consider obtaining written informed consent for the administration of local anesthetic. While I agree that obtaining informed consent is standard procedure and necessary for many procedures, such as administering general anesthestic and and maxillofacial surgery, I disagree that such is necessary with respect to routine procedures in dentistry.

I believe that implied consent is the standard with regard to the majority of routine dental procedures.[1-3] Such procedures as placing amalgam and composite restorations are examples of routine dental procedures that, in my opinion, do not necessitate written informed consent. There is the possibility that your patient may have an allergic reaction to one of these materials. But the risk of such an allergic reaction is extremely rare.[4] Furthermore, the health risks imposed in routine procedures performed under local anesthesia are also minuscule.

In theory, the patient could experience needle trauma (neuritis or neuroma), transient amaurosis, needle track infection, a broken needle, an allergic reaction to the local anesthetic and, possibly, a toxic reaction to the local anesthetic. However, these are extremely unlikely, and even more unlikely when the procedures are performed competently.[5] Minor adverse reactions, such as minor pain, swelling and bruising, are unlikely but not uncommon[5] and, in my humble opinion, unlikely to result in a lawsuit either, with or without written informed consent.

Furthermore, written informed consent involves not only listing and explaining all the possible secondary effects of the procedure, but also explaining valid alternatives to the proposed procedure.[2-4] To my knowledge, there are limited alternatives to local anesthesia, consisting of no local anesthesia and no general anesthesia, both of which may lead to even greater patient risk.

The most important question is: Is written informed consent for routine dental procedures beneficial for our patients? Certainly, taking more time regarding this issue is going to increase the price of doing business.

The last question is: Is written informed consent for routine dental procedures such as administering local anesthetic beneficial for dentistry? I think that the authors of the article have certainly given us something to talk about and to think about.

Ronald S. Brown, DDS, MS
Professor
Howard University
College of Dentistry
Washington

1. Graskemper JP. Informed consent: a stepping stone in risk management. Compend Contin Educ Dent 2005;26:286,288-90.
2. Pollack BR. The patient's right to know, 2: a major dilemma continues unresolved. J Law Ethics Dent 1991;4:2-3.
3. Pollack BR. Risk management in the dental office. Dent Clin North Am 1985;29: 557-80.
4. Mjör IA. Biological side effects to materials used in dentistry. J R Coll Surg Edinb 1999;44:146-9.
5. Malamed SF. Handbook of local anesthesia. 5th ed. St. Louis: Mosby; 2004:333-60.

Author's response: Drs. Quarnstrom and Brown have offered excellent comments regarding the limits of informed consent, a dynamic area of dental legal flux that is under constant evaluation.

Perhaps we can expand "balance risk versus costs" to balance risk versus benefit as the consideration for most things done in practice, including patient procedures and provision of informed consent. This is what the article suggests readers do; that is, consider the risk versus benefit of providing or not providing informed consent in one's own practice setting.

As health professionals trying to practice efficiently, being temporally responsible in the provision of consent is obviously beneficial to all concerned. We also agree that when "devastating" complications occur, a lack of consent is problematic. As the article mentions, the administration of local anesthetic can result in death, and patients also have developed perhaps less devastating morbidity such as nerve damage.[1,2]

These rare complications also can occur secondary to "endo and surgery" and other dental procedures. Local anesthetics are just another means of producing complications already being addressed during the consent process. It wouldn't appear to be more time-consuming to add anesthesia to a list of etiologic factors on the routine written consent that one is already providing for patients.

With regard to implied consent being "standard" in dentistry, what we as dentists believe is only the first step in what is accepted by the community. Once a controversy forms, both defendant and plaintiff will be able to find experts willing to opine about dental standards. Ultimately, lay juries establish what the standards for the community are.

Our study showed that many dentists obtain consent for local anesthesia now. In addition, perhaps the most comparable nondental situation in this country is the practice of physician anesthesiologists, who are trained to obtain consent for the administration of all anesthetics, including head and neck local procedures.[3-7]

Finally, with regard to obtaining consent for administering anesthetic in dentistry, court experts argue that one treatment or another is what is "ordinarily" done. Courts have found that "ordinary" does not necessarily equate to what the majority of practitioners do.[8] Several treatment plans for a particular situation may be acceptable.

The comments of Drs. Brown, Dreiman and Quarnstrom are greatly appreciated.

Daniel L. Orr II, DDS, PhD, JD, MD
Clinical Professor
Oral and Maxillofacial Surgery and Anesthesiology for Dentistry
University of Nevada School of Medicine
Las Vegas

1. Blanton, PL, Jeske AH. Avoiding complications in local anesthesia induction: anatomical considerations. JADA 2003;134:888-93.
2. Dower JS Jr. A review of paresthesia in association with administration of local anesthesia. Dent Today 2003;22:64-9.
3. Dripps RD, Eckenhoff JE, Vandam LD. Introduction to anesthesia: The principles of safe practice. 4th ed. Philadelphia: Saunders; Co.;1972:38.
4. Birch AA, Tolmie JD. Anesthesia for the uninterested. Baltimore: University Park Press; 1976:171.
5. Albright GA. Anesthesia in obstetrics: Maternal, fetal, and neonatal aspects. 2nd ed. Boston: Butterworths; 1986:31.
6. Miller RD. Anesthesia. New York: Churchill Livingstone; 1981:2588.
7. Parker EO. Tips on how to avoid a lawsuit or successfully manage one for the interventional pain medicine specialist. Int Spine Injection Soc Newsletter 2003;4(5):36-7.
8. Williamson v. Elrod, 348 Ark. 307, 72 S.W. 3d 489 (2002).

LOCATING PAIN

I really enjoyed the November JADA case report by Drs. Tamar Roz, Leonard Schiffman and Sharon Schlossberg, "Spontaneous Dissection of the Internal Carotid Artery Manifesting as Pain in an Endodontically Treated Molar" (JADA 2005;136:1556-59).

I'm a general practitioner and see orofacial pain patients by referral, and I learned something new by reading this article. Hindsight is 20/20, but I wonder if a quicker and more exacting referral could have been made if Dr. Roz had used diagnostic blocking injections to give more information as to the site and source of the dental pain.

I find using anesthetic to be one of the most useful tools to help me figure out whether the site and source of pain are synonymous. In my experience, much time and many health care dollars are lost in the referral process. As general dentists, we are qualified to figure out the most appropriate referral.

An endodontist surely knows more about endodontics than I do, but may not know any more than a general practitioner does about the other causes of dental pain. If the general practitioner can find no objective findings to conclude a failed endodontic treatment, it most likely is not a failed endodontic treatment.

Using local anesthetic to anesthetize the suspect tooth would completely eliminate the patient's pain if, in fact, the tooth was the source of the pain. In this case, it would probably only relieve a small part of the pain. We could go one step further and give a second division block. If the pain was from the sinus, the block would then relieve the pain.

When a patient has unrelenting pain, all structures that are the site of pain can quickly become sensitized, so that any sensory input is perceived in the brain as pain; that is, percussing the tooth caused pain. Also, the patient is usually convinced that pain is coming from the tooth, further confusing the issue. I use anesthetic blocking techniques often to help me determine whether the source of the pain is the site of pain, or whether it is from a distant structure.

Dentists are the ideal practitioners to use anesthetic to confirm site and source of pain. Anesthesiologists are up there, but we give more injections in the head than all other health care practitioners. Once you gather all the data, trust your feelings and skip the referral to the endodontist, oral surgeon and otolaryngologist if your findings lead you to believe that the source of pain is from more

central structures. You might just save a life, as these good doctors did.

As long as the trigeminal nerve innervates dental structures and intracranial vascularity, there will be confusion. Anesthetic blocking techniques will help to clarify this. Good job to these practitioners for their astuteness and willingness to take the time to report it.

Kimberly R. Wright, DMD
West Linn, Ore.

Author's response: A diagnostic block injection is one of many tools that can be useful in the development of a diagnosis related to a whole host of medical and dental situations. In some instances it is not used, as the diagnosis is established by employing other more specific, rapidly reversible and reliable modalities that do not hinder the practitioner's ability to continue on and immediately apply other testing techniques, should the diagnostic block's results prove to be inconclusive.

The persistent numbness from a diagnostic block may result in a significant delay in interpreting other tests and in gathering additional information, even if a local anesthetic without the prolonging effects of adrenalin is administered. This is particularly true in a case such as the one described in our article, in which the problem was atypical pain of unknown origin.

As for the appropriateness of referrals made by the general practitioner, it is incumbent upon both generalists and specialists to remember that we must all be team players. When unusual situations present, it is imperative that we have the humility to seek consultation in whatever field we feel necessary, as our primary responsibility is not to our egos or to our specialties, but to our patients.

Because unusual situations are not all that unusual, and because most of us eventually will learn that we cannot be masters of all trades, we should never hesitate to ask a colleague to look over our shoulder. I suspect the patient in the case presented didn't mind the consultation either.

Tamar M. Roz, DDS
Woodmere, N.Y.

ORAL BRUSH BIOPSIES

I am writing in response to Dr. Charles Hapcook's November JADA column, "Risk Management Considerations for Oral Cancer" (JADA 2005;136:1566-7). Dr. Hapcook writes, "For abnormalities or suspicious lesions found during the evaluation, the dentist should either schedule the patient for a re-evaluation or properly refer the patient. Failure to follow these procedures on a timely basis can result in a more severe medical and dental consequence for the patient and an onerous legal consequence for the dentist, especially in the case of oral cancer."

Many JADA readers, no doubt, are well aware of the value of the oral brush biopsy in the early detection of precancerous and cancerous oral lesions. The great majority of oral abnormalities are not "suspicious" and, therefore, do not warrant referral or need for biopsy. Rather, dentists are faced, almost daily, with evaluating lesions that have minimal or no suspicious features, and no obvious etiology.

It is precisely these types of lesions that dentists should evaluate with the brush biopsy, since some will prove to be precancerous or cancerous, despite their benign appearance. These types of lesions develop in all ages, including in young patients, in those with no risk factors for oral cancer and, increasingly, in women.

Personally, I have found the brush biopsy to be an invaluable and reliable tool in my practice. The brush biopsy provides my referring dentists and, more importantly, my patients, assurance that a lesion is evaluated adequately at the time it is detected—not two weeks later. My patients are already extremely apprehensive about undergoing a surgical procedure in their mouth, but the noninvasive brush biopsy is a stress reliever for my patients and for me.

Dr. Hapcook writes about the legal consequences of misdiagnosing oral cancer, yet his omission of the brush biopsy is ironic, given an editorial by Glazer,[1] who writes, "Since the brush biopsy is not a difficult procedure to perform, requires no anesthesia, causes minimal or no bleeding or pain, and carries the ADA Seal of Acceptance, the failure to evaluate oral lesions that may be precancerous or cancerous, even when you do not suspect them of being so, is inexcusable, and makes you liable!"

Martin K. Bench, DDS
Westminster, Colo.

1. Glazer HS. Oral cancer: "Be sure or get sued." AGD Impact 2002;30(11):18.

Author's response: Thank you, Dr. Bench, for pointing out the full array of diagnostic armamentarium for oral cancer, including the brush biopsy. My article, however, was never intended to be a comprehensive guide to the diagnosis and treatment of oral cancer. This

has been covered much more extensively in a host of previous scientific articles.

The intent here is to raise dentists' consciousness of the impending risks of ignoring oral cancer, and its posttreatment considerations.

Having said that, I still believe that Dr. Bench's comments are well-founded to aid our colleagues in recognizing the full extent of diagnostic procedures available today.

Charles P. Hapcook Sr., DDS
President and Chief Executive Officer
Eastern Dentists Insurance
Westborough, Mass.

MINIMALLY INVASIVE DENTISTRY

Six cheers, not just three, to Dr. Gordon Christensen for his November JADA column, "The Advantages of Minimally Invasive Dentistry" (JADA 2005;136:1563-5). It is a wonderfully crafted call for moderation in treatment that should always put our patients' interests first: their clinical interests, their emotional interests and their financial interests.

We are of the same generation, he and I, and have lived through the emerging high-tech age of dental care of the mid-20th century to see so many of the wonderful advances that have made our profession more efficient and more productive, both for the practitioner and the patient. However, as members of a trusted and honored profession, we dentists must strive to avoid creating our patients' perception that we are providing "dentistry for the dentist," rather than "dentistry for the patient."

Dr. Christensen's column should be kept in the forefront of thinking of all who are graced with the privilege with the sponsorship and trust of their patients.

Richard M. Hochman, DDS (retired)
Savannah, Ga.

RESPONSE FROM THE AAP

The American Academy of Periodontology read with interest Dr. Gordon Christensen's November JADA column, "The Advantages of Minimally Invasive Dentistry" (JADA 2005;136:1563-5). On behalf of the Academy and the members and patients we serve, I find the anecdotal statements related to the dental community's increased interest in "accomplishing more treatment than required" and "placing implants," as well as "a decline in interest in periodontal therapy," insulting to my periodontist and general practitioner colleagues. Research shows that oftentimes patients' level of disease, or damage caused by disease, requires more extensive treatment.[1]

These insinuations of inappropriate or overtreatment violate the essential trust that constitutes the core of the doctor-patient relationship. As health care practitioners, we have taken a vow to recommend any treatment that has proven to be effective in improving the health of our patients, and that meets their needs and interests. As it relates to periodontal disease, we know that it is a chronic inflammatory condition that requires careful monitoring and treatment throughout a patient's life.

This is especially important for patients with inflammatory-related risk factors common to periodontal disease and general health conditions such as diabetes, cardiovascular disease and pregnancy. Oftentimes, these patients' care requires careful comanagement between the periodontist and referring dentist, so to suggest that most cases can be "easily" treated without tried and true "conventional periodontal therapy" is irresponsible and could negatively affect the health of our patients.

The Academy's mission, as supported by our periodontist and general practitioner members, is to advance the oral health and well-being of patients through expertise in periodontics, implants, periodontal medicine, periodontal plastic surgery and oral reconstructive surgery. Helping all patients achieve periodontal health is the keystone of this mission, with the supporting elements offering options for patients who wish to correct damage caused by the disease or to improve appearance.

I suggest that Dr. Christensen review the ADA Principles of Ethics and Code of Professional Conduct before he implies that the dental profession is in violation of these guiding principles. Of particular interest might be the principle of patient autonomy that states: "[T]he dentist's primary obligations include involving patients in treatment decisions in a meaningful way, with due consideration being given to the patient's needs, desires and abilities."[2] This is a standard, I believe, that most of us feel comfortable achieving every day.

Kenneth A. Krebs, DMD
President
American Academy of Periodontology
Chicago

1. Cobb CM, Carrara A, El-Annan E et al. Periodontal referral patterns, 1980 versus 2000: a preliminary study. J Periodontol 2003;74:1470-4.
2. American Dental Association. Principles of ethics and code of professional conduct with official advisory opinions revised to January 2005. Section 1: Principle—Patient autonomy. Available at: "www.ada.org/prof/prac/law/code/ada_code.pdf". Accessed Nov. 28, 2005.

Author's response: I appreciate Dr. Krebs' interest in the subject, and respect and agree with most of the views expressed in his letter. It appears, however, that Dr. Krebs has misunderstood the intent of my editorial.

I am sorry that I did not make the purpose of the editorial clear, and that I insulted him. Perhaps by being deeply engrossed in periodontics, he may have missed the overt overtreatment promoted in the "literature," on the lecture circuit and in financially oriented, commercially offered programs.[1]

When stating the objectives of the minimally invasive dentistry orientation, I stated on page 1563 that "the group is interested in promoting optimum, minimally invasive treatment for all patients in all areas and specialties of dentistry." This includes conventional and conservative periodontal therapy, either of which may be minimally invasive for the specific disease condition.

I have long supported, accomplished and taught optimal periodontal therapy; consulted with and referred to periodontists; and observed the variability in long-term results obtained by conventional periodontal therapy. When conventional periodontal therapy is indicated and the patient will accept it, the therapy should be accomplished. The challenges with this suggestion are the millions of patients in the United States who will not accept or cannot afford conventional periodontal therapy.

Additionally, on a broader scale, there are many patients who are receiving complex overall dental therapy without proper informed consent about the treatment alternatives for their specific needs, the advantages and disadvantages of each, the costs of each, and the potential results of doing no treatment at all.

Any observant practitioner knows that the untreated periodontal disease in America is overwhelming and that, with the exception of the small amount of periodontal treatment provided by the few highly skilled, competent periodontists and some general dentists, the disease goes largely untreated.

Fortunately, dental hygienists are providing some therapy for periodontal conditions to help satisfy the enormous need not being treated by periodontists or general dentists. Periodontal disease prevention and therapy is taught very well in U.S. dental schools; however, it is my candid observation from polling thousands of dentists in continuing education programs that the amount of periodontal therapy, including conservative and surgical concepts, provided by general practitioners in the United States is negligible.

In spite of the skill of the periodontal community of specialists in providing periodontal care, the obvious emphasis on implant placement by practicing periodontists has, in my opinion and observation, distracted from conventional periodontal therapy. The psychological and physiological reasons for this change are obvious.

After placing many implants, I can state that the surgical placement of implants in healthy patients with adequate bone is relatively simple, predictable and pleasing to the patient, while the outcome of conventional surgical periodontal therapy is far less predictable and satisfying to some patients.

My editorial was aimed at discouraging "overtreatment," not at depreciating the value of conventional treatment when indicated. One of our responsibilities to the public is to prevent or treat disease with minimal or no negative effects. The rampant overtreatment readily observed in many areas of dentistry from almost any dental "journal" or magazine is pathetic, in my opinion. I encourage practitioners to treat patients as we would like to be treated ourselves, without a dominant orientation toward money. I am sure that Dr. Krebs and the Academy would agree with that opinion.

In summary, I feel that the profession at large overtreats dental caries and knows about, but seldom treats, periodontal disease. With regard to periodontal therapy specifically, it is my opinion that methods need to be developed to motivate more conventional and conservative periodontal therapy by general dentists[2] for the vast majority of the U.S. population that is now untreated.

There are too few periodontists to handle the current need for periodontal therapy. Clearly, but "anecdotal[ly]" to use Dr. Krebs' word, implementation of periodontal therapy is now a significant void in our responsibility to the public. The need for periodontal therapy for the public is not diminishing.

I am willing to help the

Academy in their efforts to promote and encourage an increased interest in periodontal therapy throughout the profession. I see the problem; however, the global need for periodontal therapy is out of my realm of primary concern as a practicing prosthodontist, researcher and educator.

Dr. Krebs may be interested to know that I have had numerous positive letters, e-mails and calls about the editorial he criticized. Apparently, many dentists are in favor of minimally invasive dentistry, with its varied interpretations.

Gordon J. Christensen, DDS, MSD, PhD
Provo, Utah

1. Christensen GJ. I have had enough! Dent Town Magazine 2003;4(9):10, 12, 74-5.
2. Christensen GJ. Why do most GPs shun periodontics? JADA 1992;123(1):75-6.

CONCLUSIONS QUESTIONED

I know that Dr. Christensen has done a lot for the profession and has great influence on the practice of dentistry in the United States. His September JADA column, "Bonding to Dentin and Enamel: Where Does It Stand In 2005?" (JADA 2005;136:1299-1302), probably will change the way some dentists practice.

I find, however, that a number of his conclusions cannot be supported by the literature and, therefore, find fault with JADA for publishing them. Dr. Christensen should continue to offer his opinion in the CRA Newsletter. Each reader can then give his opinion pieces the confidence they find that they earn.

Following are specific quotations from Dr. Christensen's column with which I take issue:

Page 1300: "In vivo longevity studies on the retention of dentinal bonds are sorely needed."

I found 50 clinical studies of dentinal bonds in a short search on PubMed. The success rates have ranged from excellent to terrible. The no-wash systems, which Dr. Christensen advocates so strongly, have consistently been the systems with the higher failure rates, when compared with the total-etch systems. Most new bonding systems are tested clinically using noncarious cervical lesions as the model. These are a true test of dentin adhesion in a clinical setting. To say that there are no in vivo longevity studies is absolutely incorrect.

We have data sets from exfoliated primary teeth that indeed indicated that bond strength to dentin decreased over time. After the stresses and strains of polymerization shrinkage have been overcome, it may be that the 17 or 20 megapascals, number suggested as necessary for resin-based composite success is not required for good performance. Dental amalgam, which shrinks 50 to 100 times less than hybrid resin-based composites, has bench top bond strengths that are in the range of 4 to 6 MPa when bonded to dentin with partially filled resins.

Summitt and colleagues[1] followed large amalgam restorations bonded with Amalgambond Plus (Parkell, Farmingdale, N.Y.) with HPA for six years. There were 11 clinical failures due to loss of vitality, caries or adjacent cusp fracture. During those six years, none of the adhesively retained amalgams separated from the tooth.

Page 1301: "Total-etching dentinal bonds accomplished meticulously can be excellent,[2] but many of them require several steps that can be confusing in a busy practice."

Good dentistry is full of the need for meticulous attention to detail. In my roles as a clinician, mentor and teacher, I am very willing to accept a simpler technique at any time when the results are as good as, or better than, the more complicated system.

Tay,[3] Perdiogao and colleagues,[4] and De Munck and colleagues[5] have done extensive research on resin bonding. Based on their research and the research of others the etch-wash-primer-adhesive systems still are superior in reliability to the no-wash systems. The fact that no-wash is easier, but may be inferior, is missing from page 1301.

Page 1301: "Again, clinical in vivo research is needed to substantiate or refute the longevity of dentinal bonding to teeth in the mouth."

Long-term clinical trials are very expensive. Some research data will lose value should the formula for the tested bonding system be altered before the long-term clinical trial is completed. We do have clinical data on fourth-generation systems that show very good results. The amount of clinical research on no-wash systems is smaller, of shorter duration and demonstrates that the early performance of the no-wash systems was not as successful as the systems with separate etch, wash, prime and adhesive steps. Selected no-wash systems have approached the clinical success rate of the fourth-generation systems.[1,6-13]

Page 1301: "There appear to be well-founded reasons for clin-

icians' obvious lack of confidence in some well-controlled, peer-reviewed, in vitro studies of dentinal bonding…".

It is incorrect to intimate that laboratory research is never related to clinical performance of bonding systems. The first-generation dentin bonding systems and early no-wash systems had very low laboratory bond strengths and then equally unsatisfactory clinical performance. In the evaluation of two newer self-etch systems, Domnez and colleagues[14] placed the bonding agents on 24 teeth. Eight of the teeth were extracted the next day, and 16 of the teeth were extracted at one year. The bonding protocol was repeated in vitro on those extracted teeth. The study concluded that "there is no difference between the mechanism of degradation of self-etch adhesives in vivo or in vitro."

Clinical trials are the gold standard of medicine. However, there is certainly a great deal that can be learned from laboratory trials, prior to subjecting humans to new techniques or materials. If it performs poorly in the laboratory, the technique or material should never be used in humans.

In the rush to discover the "quick and easy," some dentin bonding systems have made it to the commercial market ignoring the inconsistent or poor performance in the laboratory.[6] That was a disservice to the public and to dentistry. Dr. Christensen's suggestion to discount all laboratory data also would be a disservice.

Page 1301: "When only a small amount of enamel is present on tooth preparations, I suggest placing mechanical retentive features, such as pins, potholes, channels or undercuts."

It is quite difficult for all but the most skilled dentist to get resin-based composite to go into small holes or channels. It is very likely that the dentin bonding agent will fill most of these "retentive areas." I cannot find clinical or laboratory data that demonstrates that the current bonding systems and current resin-based composites are improved with this macromechanical retention. The research data to support this recommendation are lacking.

Page 1302: "Some amalgams, especially spherical amalgams, are well-known to cause postoperative tooth sensitivity. Self-etching bonding agents prevent this sensitivity."

Tooth sensitivity related to amalgam restorations is very difficult to study, because most amalgam restorations are not reported as being associated with sensitive teeth at two-week study follow-up appointments.

I searched for literature references to show that "self-etching bonding agents" prevent postoperative sensitivity related to amalgam restorations, but failed to find support for that assertion in the refereed literature. Davis and Overton[13] found some decrease in sensitivity to a direct cold challenge of teeth with incomplete tooth fracture after Amalgambond Plus with HPA was used to bond amalgam restorations (20 bonded and 20 pin-retained amalgam restorations observed for one year). We concluded that the remaining dentin thickness was more likely the determining factor for less cold sensitivity with bonded amalgams (the pin channels were 2 millimeters into dentin for the control teeth), rather than the bonding agent.

Summitt and colleagues[1] in their six-year study did not find a difference in thermal sensitivity between pin-retained and bonded amalgam restorations. Those studies should not be extrapolated to include self-etching primers, since the 4-methacryloyloxyethyl trimellitate anhydride system that was used is a total-etch system.

Available clinical studies do not indicate that self-etching bonding agents decrease sensitivity in spherical amalgams.

J. D. Overton, DDS
Head, Division of Operative Dentistry
Department of Restorative Dentistry
Dental School
The University of Texas Health Science Center at San Antonio

1. Summitt JB, Burgess JO, Berry TG, Robbins JW, Osborne JW, Haveman CW. Six-year clinical evaluation of bonded and pin-retained complex amalgam restorations. Oper Dent 2004;29:261-8.
2. Christensen GJ. Tooth sensitivity related to class I and II resin restorations. JADA 1996;127:497-8.
3. Tay FR. Reducing steps in dentin bonding-what have we really gained? The Buonocore Memorial Lecture. Paper presented at: Annual Meeting of the Academy of Operative Dentistry; Feb. 24, 2005; Chicago.
4. Perdigao J, Gomes G, Duarte S Jr, Lopes MM. Enamel bond strengths of pairs of adhesives from the same manufacturer. Oper Dent 2005;30:492-9.
5. De Munck J, Van Landuyt K, Peumans M, et al A critical review of the durability of adhesion to tooth tissue: method and results. J Dent Res 2005;84:118-32.
6. Brackett WW, Covey DA, St German HA. Clinical performance of a combined etchant/adhesive in class V resin composite restorations (abstract 233). J Dent Res 2001;(special issue) 80:65.
7. Kubo S, Kawasaki K, Yokota H, Hayashi Y. Five-year clinical evaluation of two adhesive systems in non-carious cervical lesions. J Dent 2005 June 21 (electronic publication ahead of print).
8. Gallo JR, Burgess JO, Ripps AH, et al. Three-year clinical evaluation of a compomer and a resin composite as Class V filling materials. Oper Dent 2005,30:275-81.
9. Aw TC, Lepe X, Johnson GH, Mancl LA. A three-year clinical evaluation of two-bottle versus one-bottle adhesives. JADA 2005; 136:311-22.
10. Gordan VV, Shen C, Watson RE, Mjör IA. Four-year clinical evaluation of a self-etching primer and resin-based restorative

material. Am J Dent 2005;18:45-9.
11. Burgess JO, Gallo JR, Ripps AH, Walker RS, Ireland EJ. Clinical evaluation of four Class 5 restorative materials: 3-year recall. Am J Dent 2004;17:147-50.
12. Matis BA, Cochran MJ, Carlson TJ, Guba C, Eckert GJ. A three-year clinical evaluation of two dentin bonding agents. JADA 2004;135:451-7.
13. Davis R, Overton JD. Efficacy of bonded and non-bonded amalgam in the treatment of teeth with incomplete fractures. JADA 2000; 131:469-78.
14. Donmez N, Belli S, Pashley DH, Tay FR. Ultrastructural correlates of in vivo/in vitro bond degradation in self-etch adhesives. J Dent Res 2005;84:355-9.

Author's response: I appreciate Dr. Overton taking the time to critique my comments, and I respect his views. I welcome the comments of readers, and often agree with them. However, this letter has stimulated a few comments of my own relative to "in vivo" research, "in vitro" research, clinical observation and the state of so-called "evidence-based" research.

Rather than addressing in detail each of the critiques of my column expressed by Dr. Overton, I will provide an overview of a much more important issue he stimulated.

It has been 41 years since I, too, was "Head of Operative Dentistry" for the first time, and much has happened to mellow my acceptance of in vitro research projects and semiclinical in vivo projects that are conducted in a manner not related to the required speed of actual clinical practice. Many years ago, while attempting to climb the academic ladder with publications, I was engrossed with simply and rapidly accomplishing in vitro research, and I was generally impressed with my ability to "prove" the apparent reliability of concepts and techniques through laboratory research and clinical studies, accomplished at a meticulous and nonclinically practical pace.

Later, after attending two graduate schools, conducting many research projects and receiving a receiving a significant statistical education (I actually taught statistics for a while), I found I could "prove" almost anything by manipulating the research protocol in the right way and adapting the most lenient statistical programs to the data. I could probably relate the color of socks you wear to be statistically significant to the length of your finger. I then taught scientific method and writing, and had to select projects out of the literature for critique. A couple of the hundreds of classic examples of misleading research are:

Circa 1975: The in vitro two- and three-phase wear studies in the scientific literature repeatedly showed that Adaptic (Johnson & Johnson Personal Products; Skillman, N.J.), a large filler particle size resin-based composite, had superior wear characteristics to the product Isopast (Ivoclar Vivadent; Amherst, N.Y.), a then new silicon-dioxide filled microfill. A large-scale clinical in vivo project we at Clinical Research Associates (CRA) and then many others accomplished showed the reverse when observed in the mouth—microfills wore less. In other words, the "scientific" statistically significant literature presented in vitro data that was diametrically opposed to what really happened and what clinicians observed.

Circa 1979-1990: The in vitro scientific literature showed that polycarboxylate cements had far better physical properties than the then commonly used zinc phosphate cement. Unsuspecting clinicians, trusting the "scientific literature," changed to polycarboxylate. Seven to 10 years later, many of the polycarboxylate-cemented restorations "fell off." Again, in vitro data misled thousands of practitioners.

Now, let's move to present time. Every project in our research group, CRA, undergoes careful basic science research, followed by "real world" use and critique by clinical practicing dentists. Our own in vitro data show that several current dentin-bonding agents have mature, thermally cycled bonds to dentin ranging from 30 to 50 megapascals, while the respective enamel bonds with the same materials are only 20 to 30 MPa.

If I believed our own in vitro data to be clinically significant, I would say dentin bonds are stronger than enamel bonds. How wrong I would be! Any experienced clinician who has cut off a ceramic veneer bonded to enamel knows he or she cannot get it off without cutting it from the enamel. The same clinician cutting a veneer from a dentin surface finds the moment the rotary instrument touches the tooth, the veneer flips off. In other words, again the "scientific, in vitro" research, including our own, would mislead me.

My candid opinions at this time about judging whether research reported in the literature should be applied to evidence-based practice are as follows:

- In vitro research provides interesting and occasionally clinically applicable data, but anyone relying on it for guidance in clinical practice must be widely read and clinically experienced enough to interpret it. Additionally, in vitro research must be backed up with clinical data in order to have any practical

value.

- In vivo research is useful only if the investigators are clinically competent in a pragmatic manner, knowledgeable about popular clinical techniques and able to relate their clinical procedures to adequate practice management concepts. In my opinion, clinical research accomplished at a slow, nonfinancially practical level is of academic interest only, and is often misleading to practitioners.
- In vitro or in vivo research funded by companies or individuals with vested financial interests is often justifiably highly suspect and must be backed up with independently funded, clinically relevant, financially practical research. Unfortunately, we often see such biased research published in "peer-reviewed" journals.

In my opinion, some of the most reliable and useful clinical research in dentistry over the past half-century has come from clinical study clubs with clinically competent, research-oriented members who can document actual clinical success or failure with statistical support.

After nearly five decades of teaching, research and practice in dentistry, I am often appalled at some of the nonsense published in the dental literature and its minimal value to the profession. Such reports only illustrate the lack of clinical knowledge and actual long-term clinical experience of the investigators.

To sum up this tirade Dr. Overton stimulated: true, reliable, evidence-based research must have independently funded, multisource, preferably long-term clinical research; some in vitro research to predict or interpret clinical findings; and assurance that the investigators are honest, competent, nonbiased and nonfinancially oriented. Unfortunately, precious few such studies exist in the literature.

Ask any observant practicing dentist to respond to Dr. Overton's comments about my column. To state an old adage, "The proof is in the pudding."

"Clinical success is the final test" is a statement on every CRA Newsletter. Dr. Overton's statements about some of my "observations" in the recent column on bonding need significant observation and comment from real-world practicing dentists, not a smattering of miscellaneous "data" that anyone can find on PubMed.

I welcome the chance to discuss Dr. Overton's specific beliefs and questions with him, and to compare them with both clinical and laboratory research and, more importantly, to discuss how clinical observations verify or refute the "literature." Evidence-based dentistry requires mature interpretation of apparent or alleged truths. Often, investigators have good intentions, but lack the pragmatic clinical judgment to interpret their findings.

Gordon J. Christensen, DDS, MSD, PhD
Provo, Utah

A GOOD READ

If I read only Gordon Christensen's Observations" in JADA, such as his December "How to Kill a Tooth," (JADA 2005;136:1711-3), it would be well worth my reading.

Stephen B. Lindell, BS, DDS
Chicago