

LETTERS

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PREHISTORY AND THE THIRD MOLAR

Drs. Anthony R. Silvestri and Iqbal Singh stated that the major dental finding in prehistoric people was severe occlusal wear ("The Unresolved Problem of the Third Molar: Would People Be Better Off Without It?" April JADA). Their next conclusion, that the prevalence of functional third molars likely helped to prevent even more wear as a function of force distribution, is not valid in my view.

Using current observable concepts of occlusion, it is more likely that the extensive wear patterns result from premature occlusion torquing off the third molars and causing an anterior slide as being the wear culprit. The slide, coupled with dietary abrasiveness and greater muscular strength, is a more logical assessment.

Today's obvious mechanism for this conclusion is the second molar deflection that causes severe anterior wear in patients who normally have no anterior contacts in centric relation—our practices are full of these individuals.

**Glenn E. Vance Jr.,
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Authors' response: The most recent articles we reviewed suggest that the primary cause for the severe tooth wear seen in many of our prehistoric ancestors was an extremely abrasive diet, often with the inclusion of small bones that were chewed and pulverized.

The abrasive nature of their diets may have been compounded by the inclusion of fine gritty material such as sand or stones. The extreme and rapid wear also appears to have indirectly affected the progression of the periodontal breakdown. It may be important to remember that the wear patterns seen on teeth then were much different from the wear patterns seen today. Here are several additional observations made by Kerr¹ in examining skeletal remains and dentition from prehistoric and mediaeval Britain:

- Most of the enamel of the occlusal surfaces was worn away by 40 years of age.
- Supraeruption of teeth was a prominent feature as a compensatory mechanism for the rapid and severe wear.

The diet, culture and extreme rate of wear were so different from those of modern humans that we feel it may be inappropriate to draw parallels between the etiology of wear patterns seen in Dr. Vance's practice and those seen on teeth in dried fragments of skulls from our ancestors.

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1. Kerr NW. Dental pain and suffering prior to the advent of modern dentistry. *Br Dent J* 1998;184(8):397-9.

TOOTH WHITENING

There used to be a problem with "THE HAMBURGER" we would see on the billboard, and "the hamburger" we would actually get at the fast food restaurant. Twenty to 30 years ago, truth in advertising was a big issue. Now we see a photo of a four-inch-high, stacked burger, and we accept that the reality is a one-inch, foil-wrapped object that a specially trained person flattened just before he or she bagged it.

In my dental practice I expect to see truth in advertising, for my benefit as well as for the benefit of my patients. All of us should expect this, as well as truth in representation of research. Yet, we are often provided with pamphlets for our patients that clearly contain false advertising, and we see false representation in most of our important periodicals and journals, in advertisements, as well as in research results.

What I am referring to are "before-and-after" photos of bleached, and other cosmetically changed, teeth. A prime example is found in a well-written, well-researched article in February JADA, "Light Augments Tooth Whitening With Peroxide," by Dr. J. Max Goodson and colleagues. Any

dentist or auxiliary familiar with cosmetics can see a major difference between the before-and-after pictures in Figures 4 and 5. Though I am sure there are differences due to the procedures, I object to the photographically enhanced differences.

Both "before" photos show gingiva that has muted, unnatural color, and both "after" photos show a more normal pink appearance to the gingiva. Different lighting is obvious. The before photos were taken from above, showing little reflection; the after photos were taken straight on to reflect more light, providing a lighter and whiter appearance. They should have been taken in the same light and at the same angle.

I have seen some bleaching advertisements that showed before and after photos that, through stereo imaging, are identical photos, yet one was digitally enhanced to look whiter. Awhile back, I received patient pamphlets from a company selling bleaching products that clearly showed false advertising. Some time after sending a letter stating that I could not hand out these misleading pamphlets, the company sent an apology. They stated that, after having received many complaints from doctors about the unrealistically white teeth advertised, they would reprint the pamphlets with realistic after results.

I am not belittling the extensive research, contesting the results or attacking the article mentioned above, but the photos are not up to par with the quality of the report. My issue? I would simply like to see all dental literature provide us with accurate before-and-after

photos so no one is misled.

Bleaching is the No. 1 requested cosmetic procedure. It would be very embarrassing to try to explain to every patient why his or her teeth did not bleach as white as the Kodak 110+ brightness white-paper-shaded teeth shown on our misleading pamphlets, brochures and educational tools. If any readers feel the same, I urge them to contact those companies or journals that continue to show us, and our patients, the big hamburger.

**Gary McCrummen,
D.D.S.
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Author's response: Dr. McCrummen has raised questions concerning the validity of the published images. First, I should emphasize that no part of the research depended on the images, and they were included only to illustrate the maximum responses seen.

In the manner of how the images were taken, I'm sure Dr. McCrummen is familiar with the old dental school trick of taking the pretherapy images on Kodachrome to emphasize the reds and the posttherapy images on Ektachrome to emphasize the blues. I am sure that Dr. McCrummen is also familiar with the difficulty in controlling wet chemistry variables. With this knowledge, we have tried to provide information that is as unbiased as possible.

As this is a serious scientific study, the images have been produced by what are accepted by the Food and Drug Administration and other regulatory agencies as the current scientific norm for image recording: unretouched digital images. The images were taken using a

Nikon 990 Coolpix (Nikon, Melville, N.Y.) using the dental light for illumination from the closest nonmacro distance for autofocus. As such, images taken may not conform to familiar norms of either Kodachrome or Ektachrome films but they are uniformly exposed under identical conditions and free from processing variables.

Dr. McCrummen points out an interesting observation that I had missed—the change in gingival color. In his critique, comment is made that gingival color is different in the after photos; to quote Dr. McCrummen, "both 'after' photos show a more normal pink appearance to the gingiva."

Actually, that is exactly what was reported in our data evaluation. The article clearly states that there was a significant reduction in Gingival Index (decreased redness) relative to baseline in all groups. I had not thought to verify this by considering the digital images. I would like to thank Dr. McCrummen for sharing this observation that seems to support our conclusions.

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MORE ABOUT TOOTH WHITENING

We are writing with comments about the February JADA cover story, "Light Augments Tooth Whitening With Peroxide," by Dr. J. Max Goodson and colleagues. We were very surprised that JADA would publish an article that is so devoid of scientific validation or explanation. We believe that the findings report-

ed in this study are highly suspect, and certainly are not as conclusive as the authors suggest. This is most unfortunate, because the results of this study already have been reported not only to the dental profession, but also to the general public through media, including television health segments.

We offer the following specific comments:

- The authors state that “power bleaching” is accomplished in a single office visit. In fact, most clinicians have found that multiple appointments are required to achieve significant whitening. Previous research¹ has demonstrated that the most important factor is the concentration of the bleach, not the use of a light. The Papathanasiou study reported that the addition of light is of no use in bleaching whatsoever.
- Although not a peer-reviewed publication, it is interesting to note that similar results were reported in the CRA Newsletter from April 2000.² The results of a split-arch study using light and bleach versus bleach alone revealed that light has no effect on the bleaching results. Even considering that 15 percent hydrogen peroxide clearly has the capacity to whiten teeth, the degree of whitening reported in the JADA article far exceeds that which can be reasonably expected from such a short-term treatment duration.
- Multiple examiners were involved in conducting the study, but apparently not in making the shade evaluations. The article notes that “the same investigator conducted all shade guide color evaluations...” In contrast, most clinical trials of bleaching involve two evaluators, both of whom have been

calibrated. Bleaching studies of this type should use two calibrated evaluators for increased reliability and validity, and calibration should be conducted pre- and post-study to ensure inter- and intra-rater reliability. When using two evaluators, a consensus is obtained for each case, which is far more reliable than the independent assessment of one evaluator, who can be inherently biased, whether intentionally or not. No mention of calibration, even for the one evaluator, was made in this article.

- There is no description of whether or how the colorimeter was calibrated.

- The authors state that reapplication of the “hydrogel” prevented teeth from drying out during treatment. However, the composition of this hydrogel is not described in the article whatsoever. Prevention of dehydration with any in-office bleaching procedure is virtually impossible, regardless of the composition of the whitener. And yet this article totally ignores the role of dehydration in the whitening process, even when it is widely known that dehydration effects result in considerable—but temporary—whitening effects that can exceed the immediate whitening effects of the bleach itself.

- It also is very puzzling that no recall assessments were made following rehydration of the teeth until three and six months after treatment. A one-week or one-month assessment would have been appropriate. Immediate posttreatment evaluations of shade changes have limited value because of the very important covariable of dehydration, which itself can account for substantial whitening

effects.

- Figures 4 and 5 report Vita shade guide changes of 13 and nine shade units, respectively. Admittedly, the color reproduction might be imperfect (although tissue tones appear equal), but neither case appears to reflect changes of these magnitudes.

- The subjective Vita shade data generally are far more impressive than the colorimeter data, which raises further questions about the accuracy of the Vita shades. For example, at six months, the light reportedly provided an improvement of four Vita shades. However, the colorimeter data indicate $\Delta L^* = 0.33$ and $\Delta b^* = -0.45$. These are very small changes, and cannot explain a true Vita shade improvement of four. Rather, they suggest the possibility of evaluator bias in the Vita shade selections.

- The statistical analysis appears quite impressive. However, if the data are inherently erroneous from evaluator bias or are otherwise flawed, no statistical analysis can render valid results. Moreover, it is unclear whether the statistical analysis considered each tooth or each subject (patient) as an individual case. Figure 2 certainly implies that individual teeth were used. Using teeth instead of subjects increases the sample size fourfold and clearly would impact the statistical analysis.

- Many of the results in this study are quite remarkable, yet little explanation is offered in the Discussion section. For example, the evaluator’s assessment that revealed lighter teeth at three and six months for the “light only” group is very remarkable, yet the author fails to

adequately address why—or more importantly how—long-term whitening from one treatment exposure to the light could have occurred. This observation is indeed quite remarkable and historically unprecedented. Surely some scientific explanation, theoretical or not, is warranted.

— Similarly remarkable results relate to the significantly improved gingival indices for all treatment groups at three and six months following only one treatment session. How can a short exposure to a light have any therapeutic benefit on gingival health? This observation, too, is astounding. Again, surely such an important finding deserves some attempt at a scientific explanation.

— The authors attempt to relate the improved gingival health noted in the groups exposed to the peroxide as being similar to those results noted in the literature (references 24 through 26). However, these references are totally inappropriate to explain the results observed in this study, because those studies dealt with carbamide peroxide whiteners, which contain urea, which has been shown to provide significant antiplaque effects.

— The gingival index results could be explained as either a statistical anomaly or a result of the Hawthorne effect (a distortion of research results caused by the response of subjects to the special attention they receive from researchers). The simple fact that these subjects participated in a clinical trial could have affected their oral hygiene habits.

— The entire Discussion section is, in our opinion, superficial and highly biased in its tenor,

and does not attempt to delve into the literature to explain why the noted results occurred. It provides little in the way of a credible scientific explanation for the observed results. Even the title of the article sounds like a news headline, rather than the title of a scientific investigation.

In conclusion, we find it unfortunate that JADA saw fit to publish, and highlight on the cover, an article with so many inherent problems and unsubstantiated claims, especially when the topic is of such obvious interest to the profession and the public. A journal that is supposed to be the flagship publication for dentistry has a responsibility to ensure that only articles of sufficient scientific merit and validity are published within its pages.

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1. Papathanasiou A, Kastali S, Perry RD, Kugel G. Clinical evaluation of a 35% hydro-

gen peroxide in-office whitening system. *Compend Contin Educ Dent* 2002;23(4):335-46.

2. CRA Newsletter. Vital tooth bleaching, in-office. April 2000;24(4):1-3.

Author's response: In the following, I will address comments made by Drs. Swift, Heymann, Kugel and Kanca that relate to our article. I will reference these dentists as "letter writers" in my response. Following are comments regarding Dr. Swift and colleagues' letter.

Whitening with light. The letter writers were surprised that light by itself whitened teeth. One possible explanation lies in the basic nature of colored pigments. Molecules are colored because they absorb certain segments, leaving the unabsorbed light to be reflected. Colored molecules possess conjugated double-bonds that absorb visible light. Irradiation with high-intensity light could rupture some of the double bonds, rendering the molecule less absorbent and therefore less colored. All museum curators know that light bleaches paintings.

Gingival health and tooth whitening. The letter writers were also surprised that the tooth-whitening treatments resulted in decreased signs of gingival inflammation. Although we do not know the reason, we do know that several pathogenic bacteria are "black-pigmented" by virtue of their having porphyrins (for example, *Porphyromonas gingivalis*). We also know that if we shine an intense light on these bacteria they die. It is also possible that individuals were taking better care of their teeth. Studies with carbamide peroxide usage and improved gingival health were cited to indicate that others

using tooth-whitening products have observed gingival effects.

Comparison with published results. The Papathanasiou study cited to suggest that light has little effect is not comparable to our study because the light source was a curing light. In our own studies, we have found that the curing light is not nearly as effective as the fixed halogen light.¹

Referring to a report from the April 2000 CRA Newsletter, the letter writers suggested that whitening at the level we reported is not within "reasonable expectation." First, all lights are not equal. The particular light used in this study as manufactured by BriteSmile produces an intense irradiance of 130-160 mw/cm² with negligible ultraviolet output over the entire one-hour period. The newsletter states that the CRA group has not evaluated the BriteSmile system. Reasonable in the mind of the letter writers or not, the level of whitening we measured was reported.

Experimental design. The letter writers indicate that the number of appointments needed for a desired degree of tooth whitening for a given subject is often variable. Although this is true, in our study, the number of applications was a controlled variable. The letter writers also state that measurements at one week and one month are important. I see no problem in the time scale we selected (three months and six months). Our data clearly show that the shade increased (became darker), the L* parameter decreased (became less light) and the b* parameter increased (became more yellow) between the immediate posttreatment period and

three months. I would expect the same would be true, but to a lesser extent, at one week and one month.

The letter writers suggest that a consensus approach in which one or more examiners are required to come to some level of agreement is somehow superior. This is one approach to measurement; it is not clear that it is superior. It does increase variability. It does not answer many critical questions: If the examiners differ, how are differences resolved? Are responses in some way "averaged" or "arbitrated"? If arbitrated, on what basis is the final arbitration made, or how is an average taken? Isn't arbitration a form of bias with the loudest and most authoritative advocate being the winner? Essentially we are asking that one number be dropped. What is the statistical validation for this procedure? We have chosen the alternative of a single calibrated measurer. There are proponents for both approaches. It is not clear which is superior.

Calibration. The letter writers were critical of the calibration methods used in the study. There are two aspects to calibration: precision and accuracy. Precision is virtually impossible to calibrate. This assumes a "gold standard" clinician, but no one tells us where to find one of these. We can and did measure reproducibility. This was evaluated using Vita shade tabs. The test is to obscure the tab identities, mix them up and have the evaluator attempt to arrange them in the correct order. This exercise, repeated many times, indicates that our color evaluator was reproducible 98 percent of the time.

The letter writers were mis-

takenly concerned that perceived calibration error would bias a study. Actually, in a blind experiment, evaluator bias in color would have a principal effect of decreasing sensitivity to detect an effect. Bias toward a particular treatment is controlled by the blinding. The statistical analysis in this study was conducted on a subject basis. The letter writers have deduced correctly that Figure 2 is an illustration of the distribution of shade of individual teeth.

Colorimeter. The colorimeter was calibrated according to the manufacturer's instructions each day. The procedure is to measure L*, a* and b* from a provided standard white plate. Analysis of these data indicates that the day-to-day instrument responses varied no more than 1 percent over the duration of the experiment.

The letter writers note that the shade guide measurements were greater than the colorimeter measurements. It has been our consistent observation that the visual discrimination by shade guide is a more statistically powerful change variable than the colorimeter. The colorimeter provides more objectivity but less sensitivity.

Dehydration. The letter writers state that "prevention of dehydration with any in-office bleaching procedure is virtually impossible..." How do the letter writers know it always occurs, and how do they measure it? The most direct method would be to take a standard volume of tooth enamel, measure the water content, whiten the tooth and take another sample and measure the water content. This is clearly a laboratory procedure, not a clinical procedure. We did report that the average

L* parameter in the light-treated control at baseline was 50.54, changed to 52.95 immediately after whitening and returned to 50.66 three months later. This could easily be due to dehydration. Not having measured the water content of enamel, we were hesitant to proclaim it so.

Images. The letter writers felt that the images did not illustrate the changes reported in the manuscript. Even in the published images it seems clear that there are differences in the before-and-after images. The

controversy over how much difference illustrates the problems I have discussed concerning calibration and multiple examiners.

I find it grievous that the letter writers feel they must refer to our work as “devoid of scientific validation,” “superficial” and “biased.” That aside, it is neither what we say nor what the letter writers say that counts—it is the data. The data speak for themselves. In this case, the experiment has been conducted with the scientific rigor required of an FDA sub-

mission. It has been audited by an external auditor, reviewed by peers and published in a reputable peer-reviewed journal. If the letter writers wish to refute the findings, there is only one way: let us see your data.

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1. Tavares M, Goodson JM, Stultz J, Newman MB. A comparison of four systems of tooth whitening (abstract 2440). *J Dent Res* 2002;81(special issue A [San Diego Abstracts]):A-308.