

displaced toward the cleft side as a result of the cleft. In addition, they postulated that the unilateral cleft affects development of the entire face and possibly the cranial base. In 1986, Delaire and I published a study of seven infants with unilateral cleft lip and palate who also had preoperative computed tomography scans.¹ We compared these scans to those of an age-matched group of five normal infants who had been scanned for other reasons. Kane et al. did not have age-matched controls, which they stated limited their findings. My coauthor and I found that there was asymmetry of the cranial base and, to an extent, the orbits. The cleft side was always more obtuse, with the petrous portion of the temporal bones more flared and asymmetrical. This effectively compresses the middle cranial fossa; thus, structures housed in the temporal bone, such as the eustachian tubes, are malpositioned. My coauthor and I concluded that there are effects on the cranial base and other parts of the skull in unilateral clefts, as previously suggested by Wardill.²

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The Role of Betadine Irrigation in Breast Augmentation

Sir:

We read with great interest the article entitled "The Role of Betadine Irrigation in Breast Augmentation" by Dr. Wiener.¹ We are in complete agreement with the author, and we would like to draw the readers' attention to our own work published in the *Journal* a few years ago.²

We were also frustrated with the U.S. Food and Drug Administration's labeling requirement, which affected saline-filled implants as well as silicone gel implants, similar to the ones commonly used in Europe. We therefore conducted an experimental in vitro study to assess the effect of povidone iodine on the physical properties of the implant shells. Solutions of povidone iodine with concentrations ranging from 0.01% to 10% (Betadine Solution neat) were used. Our study showed that there is no effect on the mechanical properties of the implant shells from the use of povidone iodine for irrigation of the pocket or the implant itself. Although our study was performed on shells from gel-filled implants, these shells are the same as those used for saline-filled implants, and our conclusions can also be applied to those.

The issue of fibroblast toxicity by povidone iodine raised by Dr. Wiener has been the object of several studies.³⁻⁶ We would like to submit that this effect could actually be beneficial in the battle against capsular contracture. Prantl et al.^{7,8} showed that fibroblasts were among the main cellular populations identified histologically in breast implant capsules, and this toxicity on fibroblasts could explain why povidone iodine has been found to be one of the most effective irrigants in decreasing capsular contracture.¹ Of course, further studies are warranted to identify the quantitative effect of povidone iodine on this cellular population, but we believe that this issue should be taken into consideration when discussing capsular contracture prevention.

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Reply**Sir:**

I thank Drs. Zambacos, Mandrekas, and Morris for their kind comments regarding my observations on this important subject. The studies conducted by Dr. Zambacos et al. have been corroborated by other studies, as cited in the article, confirming no deleterious effect on the implant shell by povidone iodine (Betadine).

The concern regarding tissue toxicity was primarily from the standpoint of wound-healing problems. This has clearly been shown, as cited in my article, to not be an issue. My personal use of Betadine is a dilution of 50%, a concentration that is significantly higher than that used by many other authors; as discussed, this dilution has not been shown to affect wound healing at the incision. With regard to the myriad possible reasons that Betadine is effective in decreasing capsular contracture by a significant rate, I believe basic research studies would be valuable, not only to plastic surgery but also potentially to other surgical specialties, for improving outcome. The possible effect on fibroblasts is certainly an intriguing idea and a possible additional explanation for the value of Betadine.

My attempts to reverse the policy of the U.S. Food and Drug Administration, through multiple communications with them and both Allergan/Inamed and Mentor, are at a standstill. Although the consensus among plastic surgeons is clear regarding the use of Betadine with breast augmentation, this issue is being ignored at other levels, and I strongly believe that allowing plastic surgeons to treat patients with the best possible care is our duty.

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The Use of Quantitative Abductor Pollicis Brevis Strength Testing in Patients with Carpal Tunnel Syndrome

Sir:

I enjoyed the well-documented and detailed article by Liu et al. entitled "The Use of Quantitative Abductor Pollicis Brevis Strength Testing in Patients with Carpal Tunnel Syndrome."¹ There is a particular difficulty in obtaining long-term follow-up of study patients, and the efforts to document a group of them and report their results 7 years later are commendable.

Having performed abductor pollicis brevis strength assessments in some 4000 carpal tunnel patients over the past 14 years, I am confident of its validity and consistency in objectively evaluating carpal tunnel syndrome.² There is a remarkable correlation between strength and clinical progress in both surgical and non-surgical patients. The more symptomatic hand is invariably weaker on abductor pollicis brevis testing. Objective assessments of abductor pollicis brevis strength

changes in patients with improvement or deterioration of symptoms is invaluable, whether they have undergone surgical or conservative treatment.

I also believe that correlation of electromyographic testing combined with abductor pollicis brevis strength testing is worthwhile. My colleagues and I have just completed a 50-patient study which we intend to publish shortly. We hope it will help establish recognition of the merits of abductor pollicis brevis strength testing as a simple, reliable, inexpensive, and objective examination.

The accuracy of the measuring devices for clinical use can be much less exacting than that of the laboratory-quality instruments often used in muscle measurement studies. There is such a profound loss in abductor pollicis brevis strength in the typical carpal tunnel patient (1 kg or so) that a 50-g accuracy is well within acceptable tolerances.³

The other anecdotal comment I should make is that patients with abductor pollicis brevis strength of less than 1 kg on presentation often do much less well in terms of symptom resolution. They almost invariably are relieved of their pain, but they may still be symptomatic in terms of numbness with repetitive activities or when sleeping or driving. Warning them ahead of time about this possibility avoids many prolonged discussions postoperatively.

Even when patients still have subjective symptoms, almost all will demonstrate some increase in abductor pollicis brevis strength. This may require several months to achieve.

There is a documented, objective preoperative and postoperative strength measurement. This is especially helpful in the unhappy patient with compensation and insurance claims, who complains postoperatively that he or she is really not better. The patient often implies some fault on the surgeon's part. This measurement, which a patient cannot fool, has saved many prolonged discussions and avoided what could potentially have become a somewhat confrontational situation.

In summary, I strongly support Liu et al.'s suggestion that this test offers a valid and objective measurement of the status of a patient's carpal tunnel syndrome. It correlates well with other clinical findings. Their article also outlines the steps we must all take to objectively determine the true role of the abductor pollicis brevis strength measurement in becoming a useful standard examination for evaluating carpal tunnel syndrome.

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