GUIDELINES

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Surgical Approach to Head and Neck Cancer and Postoperative Complications: Are All Patients Eligible?

Sir:

A retrospective study of 100 consecutive patients undergoing immediate microsurgical reconstruction after resection of head and neck cancer was conducted, correlated to the economic impact in a heterogeneous group of elderly patients affected by different types of cancer, with different locations and different free flap donor sites.[1] Postoperative medical complications were statistically more important in pulmonary and alcohol patients in negatively affecting the outcomes and true costs of microsurgical procedures.

Suggestions on how to improve cost effectiveness included donor flap selection, synchronous harvesting of the flap, preoperative assessment, intraoperative fluid management, postoperative pulmonary therapy, early transfer of the patient to an intermediate care unit, tracheostomy decannulation, and speech and swallowing therapy. We would like to discuss the staging of the cancer.

Stage I and II diseases respond to single-modality therapy, either surgery or radiation, with a positive outcome rate of 60 to 90 percent.[2] The choice of therapy is related to potential morbidity and the possibility of recurrence, whereas oral cavity and oral pharyngeal cancers are usually treated with surgery and laryngeal tumors (whose excision leads to loss of speech) are treated with radiotherapy, with surgery reserved for failure and salvage.

Stage III and IV patients require multimodality treatment, including extensive surgery followed by radiation, although many patients die because of locally or regionally persistent or recurrent cancer, for a positive outcome rate of 30 to 60 percent.[2] Patients unfit for surgery undergo palliative radiation therapy and neoadjuvant chemotherapy for organ preservation in hypopharyngeal and laryngeal malignancy. Cervical node involvement reduces survival up to 50 percent for all sites, representing the real prognostic factor in head and neck cancer.[3]

We do not criticize microsurgical reconstruction after head and neck cancer as time consuming, since free flap reconstruction[4] was found to have, in our experience, lower complication rates, better aesthetic outcomes, less donor-site morbidity, a shorter hospital stay, and superior cost effectiveness compared with other procedures. We would underscore the main importance of accurate selection (staging) with respect to patient quality of life.

The cure rate in stages III and IV is low, and because of the severe influence on the main vegetative functions, respiration, and alimentation as a common consequence of surgery (often multistaged), many of these patients can experience comfortable, functional lives without the need for microsurgical reconstruction, especially if their general condition is already affected by other pathologies,[5] with, in addition, a better economic impact.

DOI: 10.1097/01.prs.0000298520.87892.74

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Maggot Therapy

Sir:

This letter is in response to the views expressed by Emsen.[1] I have not used maggot therapy as such, but I have vast experience in treating maggot-infested wounds of the extremities and nasal myiasis in leprosy-affected persons.[2,3]

Maggots were probably reintroduced to "biologically" debride badly infested wounds in cases in which surgical decisions were difficult to make at one go. I have noted that maggot-infested wounds granulate faster after maggot removal and formal cleaning. This has been attributed to the allantoin excreted by the maggots, which promotes granulations. The maggots of certain species of dipterans flies are merely scavengers and are at times beneficial. However, it is difficult to predict circumstances under which they act as scavengers or as serious parasites. The number of larvae to be applied to a particular wound is more or less empirical. Numbers reported in literature (200 to 600)[4] appear to be in excess. In clinical situations, while treating maggot-infested wounds in leprosy patients, I could not obtain more than 10 maggots. The young maggots perform better in comparison to those who are nearing maturation (total larval span is 7 to 10 days). Dressings should be changed every 72 hours, and several cycles can be repeated.[5]

Playing with maggots without understanding their biology can be dangerous, as reported.[1] Maggots (fly larvae) are voracious eaters. They consume tissue very fast, whether the tissue is necrotic or healthy, during the process of their development to the pupa stage. They have a tendency to migrate to the deeper tissues away from light. Hence, applying maggots to wounds near body cavities, such as the ear, umbilicus, abdominal wall, eyes, anus, genitals, eyes, and scalp, carries a risk of the larvae migrating deeper if treatment is not properly supervised or if an overzealous attempt is made to get rid of all the necrotic material at one go. Wounds of the extremities are better suited for maggot therapy compared with other body sites. It is probably for this reason that chronic osteomyelitis could be treated successfully with maggot therapy in mid-twentieth century.

DOI: 10.1097/01.prs.0000298521.37653.84

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Reply

Sir:

Dr. Malaviya emphasizes that young larvae are better than old larvae for treating wounds that are difficult to treat using a surgical approach. The author also states that the dressing should be changed every 72 hours and that several cycles can be repeated. These points are absolutely correct. In the case I presented, I mentioned that the patient's dressing had not been opened for 1 week and that there had been no changes with new forms. As a results, mortal and terrible complications can be seen in some or rare cases. This technique (whether medical or not medical) should be used by experienced physicians. As in the case I presented, uneducated or inexperienced persons can bring about the patient's death.[1,2] I advise our colleagues and patients who use any herbal or other extracts that we should have much more experience and/or data on

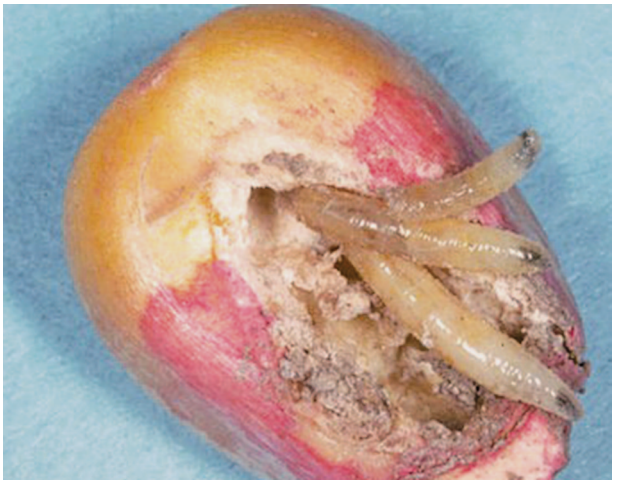

Fig. 1. Classic maggots.

these alternative therapies. Unforeseen, irreversible complications can occur (Fig. 1).
DOI: 10.1097/01.prs.0000298522.29165.bc

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Face Lift with Suspension Sutures

Sir:

It is a pleasure to read *Plastic and Reconstructive Surgery*. One always learns a lot. I was able to learn a lot from van der Lei et al.'s article in the June 2007 issue.[1]

It confirmed my view that sometimes you cannot believe that some journals do, in fact, good research of previous publications with regard to "new" techniques or procedures. It has already happened twice with my article "Ultrasound and Seromas."[2] Now, with the letter by van der Lei et al., entitled "Mini Face Lift with Suspension Sutures: Historical Analysis of Development and Morphic Resonance," I have learned more.

I have been working with facial suspension since 1990, and I must acknowledge that I did not know about the excellent article by Drs. Duminy and Hudson, even though I have the *Aesthetic Plastic Surgery* issue in my personal library at my clinic. I was astonished to read it.

The letter also taught me something about Dr. Saylan. I was progressively feeling that Dr. Ziya Saylan was becoming a forgotten author by Dr. Tonnard (he put Saylan's name in the first article and "forgot" him again and again), but indeed, Dr. Saylan was the first to omit the "creators of the technique," since his first article was published 2 years after the article by Duminy and Hudson, and both articles are absolutely similar. The journal that first published "The S-Lift: Less Is More" should have researched related techniques before printing the article.

As van der Lei et al. said in their letter, many people may have the same idea, but it is quite suspicious when the drawings look so much alike. Dr. Tonnard et al., in their reply, state that "in MEDLINE, the search term 'minimal incision AND face lift' alone delivers 32 references."[3] Well, I am clearly convinced that they know many other techniques involving suspension of the superficial musculoaponeurotic system, short scar face lift, face lift with suspension sutures, and so on, that they do not usually cite. I myself have an article published in *Plastic and Reconstructive Surgery* from June of 2001, entitled "The Roundblock SMAS Treatment,"[4,5] that describes a face lift with suspension sutures (in a very different way, fortunately).

Congratulations to van der Lei et al. for their excellent historical review on this topic and for the lesson they have taught to all of us.
DOI: 10.1097/01.prs.0000298545.00185.2b

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Reply

Sir:

The letter by Dr. Stocchero and our letter, entitled "Mini Face Lift with Suspension Sutures: Historical Analysis of Development and Morphic Resonance,"[1] clearly demonstrate what type of reactions may result from incomplete referral to key articles: feelings of misunderstanding, unpleasant feelings about authors who seem to claim to be the original inventors, and lack of acknowledgment. Of course, as stated before, it is known that similar ideas and thoughts arise in more places in the world seemingly independent of each other. On the other hand, besides the mysterious phenomenon of morphic resonance,[1,2] these similar ideas and thoughts may also be more closely associated because of the exchange of thoughts, however subtle, at congresses, conferences, workshops, and all kinds of other meetings.

We have experienced similar influences as mentioned with our bipolar coagulation-assisted orbital septoblepharoplasty technique, which is a fat-saving upper eyelid blepharoplasty.[3] We came up with this idea after having noticed discussions in the literature and during scientific meetings concerning the ongoing trend to

remove less fat from the eyelids. A close and critical search of the literature subsequently revealed that Cook, Derebery, and Harrah,[4] in the ophthalmologic literature, described in 1984 the use of electrocautery for the orbital septum of the lower eyelids, to shrink and tighten the orbital septum. Even though we were not aware of these important facts before this critical literature search, we credited all these ideas in our report.[3]

When an enormous body of literature exists, it may very well happen that some key references may be overlooked. Especially with new ideas, one should be very careful and try to be complete in acknowledging important key references. In our mind, this is primarily a responsibility of the authors and secondarily a responsibility of the journals, especially the reviewers.

In case of the historical development and influences of the mini face lift with suspension sutures, there seems to be some irony in the literature. Dr. Saylan[5] seems to be the first to forgot to mention the original work of Duminy and Hudson,[6] and Saylan was subsequently forgotten by Tonnard et al.[7–9] The latter authors have nicely replied to this item in their reply[10] to our letter.[1]

To Dr. Stocchero and all other contributors and readers of this excellent *Journal*, we just have to remember (and again, this is a repetition of what is in our opinion a very important comment by Thomas Biggs[11]) that we all stand on the shoulders of giants, and we should credit these giants. We, the authors of this reply, wish to credit all those who have contributed to the development of short-scar face lift techniques, including the important work of Duminy and Hudson,[6] Fulton et al.,[5] Tonnard et al.,[7–9] Baker,[12,13] Prado et al.,[14] and Dr. Stocchero.[15,16] Short-scar face lift techniques have earned a place in the armamentarium of the plastic surgeon's aesthetic surgical possibilities, although one should critically note that this technique may have also some shortcomings.[14,17]

DOI: 10.1097/01.prs.0000298524.57044.fa

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DISCLOSURE

There has been no financial support and there are no financial interests for any of the authors of this study.

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Reply

Sir:

We appreciate Dr. Stocchero's concern about the completeness of a reference list when publishing in scientific journals. The letter presents two major issues that can teach us more about what motivates someone to take up his or her pen (or computer) and write a letter to the editor.

First, Dr. Stocchero is happy that Dr. van der Lei et al. found an article in *Aesthetic Plastic Surgery* from Duminy and Hudson describing a technique similar to the one we finally described. Apparently, this article has never been cited by any other author, and Dr. Saylan is immediately accused of being the first to omit this reference. Moreover, we are also accused of "forgetting" to cite Saylan's name in further publications on minimal access cranial suspension lifting.

Second, Dr. Stocchero is unhappy that we did not cite his article published in 2001 in *Plastic and Reconstructive Surgery*, entitled "The Roundblock SMAS Treatment: A Face Lift with Suspension Sutures," and admits that his sutures are applied, "fortunately," in a different way.

We still believe that it is impossible, useless, and time consuming to try to obtain a comprehensive reference list. The articles cited in a reference list are the works that have influenced your way of thinking before your personal technique has developed. Unfortunately, we were not aware, like many others, of the work of Duminy and Hudson or the work of Stocchero. It was in 2005 at a meeting in São Paulo that we first met Dr. Stocchero, and we were impressed with his beautiful results. We do not think it is either fair or friendly to suggest that we intentionally did not mention his or others' publications. It adds nothing constructive to the whole discussion, and moreover, it is bad karma.

To illustrate the relativity of Dr. Stocchero's reaction, we would like to mention another reference that we came across while visiting Professor Ricardo Mazolla for a meeting he organized in Milan in April of 2007. As many of us know, Professor Mazolla has one of the most important historical libraries on plastic surgery in the entire world, and his Fondazione Sanvenero-Rosselli is taking care to maintain and protect this treasure. On a Sunday morning we had the opportunity to dive into the history of facial rejuvenation techniques, and among other articles, we came across a very remarkable article by a French plastic surgeon named Virenque entitled "Traitement Chirurgical des Rides de la Face et du Cou"

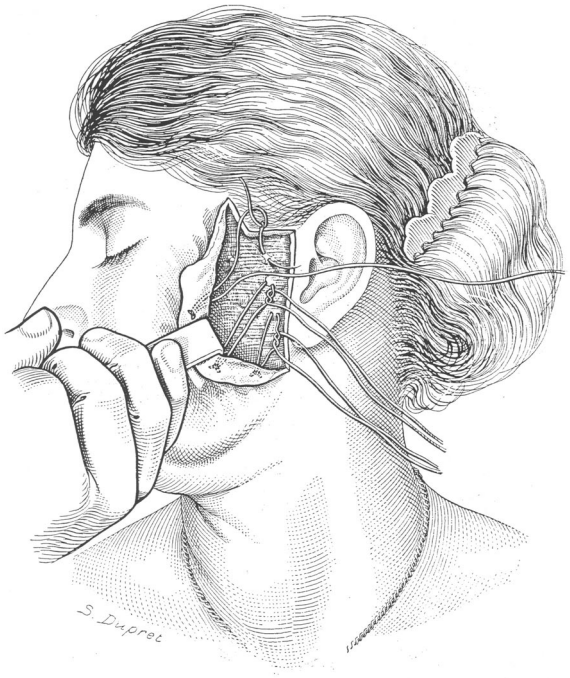

Fig. 1. Incision line and limited undermining. Placement of the three sutures, attached to the parotid fascia. From Virenque. Traitement chirurgical des rides de la face et du cou. In *La Pratique Chirurgicale Illustrée*. Paris, 1927.

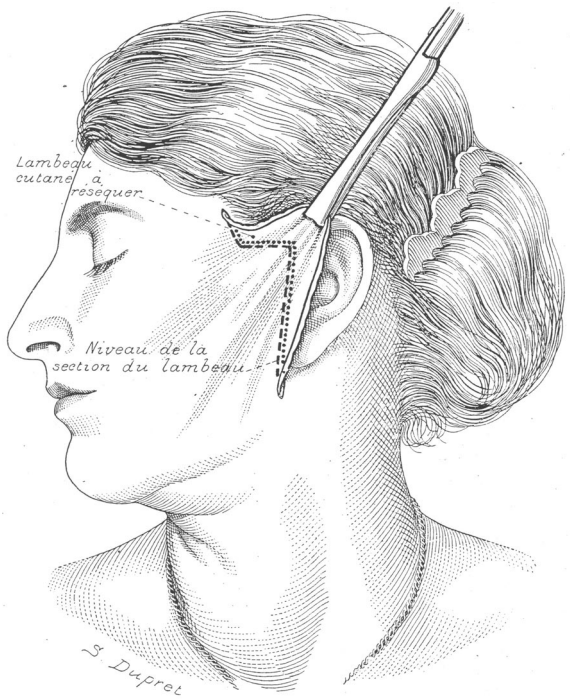

Fig. 2. Redraping of the skin flap before resection and placement of the final scar. From Virenque. Traitement chirurgical des rides de la face et du cou. In *La Pratique Chirurgicale Illustrée*. Paris, 1927.

("Surgical Treatment of Facial and Neck Rhytides"). The article was published in a book edited by Victor Pauchet, entitled *La Pratique Chirurgicale Illustréé,* in Paris in 1927! As you can see in Figures 1 and 2, the technique of incision, undermining (although very limited), and placement of three sutures is very similar to what we are doing today in our modern minimal access cranial suspension lift. Why was this technique not popularized before? Similarly, we know that open rhinoplasty incisions existed before but were only popularized in the 1980s. As said before, we do not pretend that we have invented anything. The only credit we could take is to have popularized a technique and a vision toward less invasive facial rejuvenation techniques, for which nowadays there is great public demand. Let us not forget, after all, and we are the first to admit this, as illustrated by this 1927 publication, that plastic surgery is and will ever stay a lesson in humility.

DOI: 10.1097/01.prs.0000304146.04824.17

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An Easy and Comfortable Way of Maintaining Dressings in Breast Surgery

Sir:

We read with interest the letter entitled "Adverse Reactions to Wound Dressings: A Financial Incendiary,"[1] and we fully agree with the authors regarding the high costs associated with the different types of bandages and dressings used in daily practice. Likewise, we often see adverse reactions secondary to the use of different types of sticking plaster, including the so-called hypoallergenic type.

Sharing our concern about these problems with the above authors, we report a new way of maintaining dressings during the postoperative period in breast surgery that allows easy access to the surgical wound with no discomfort to the patient.

We use an elastic "leotard" attached at the waist. We perform a small incision in the crotch of the garment (Fig. 1) that is wide enough to allow the patient's head to pass through. The arms are inserted through the leg holes. The dressings required in each individual case are kept in place beneath this "brassiere" (Fig. 2). During the patient's hospital stay, the garment is changed only if it is stained. Once the patient has been discharged, it can be washed in a washing machine and removed when the patient begins to take showers.

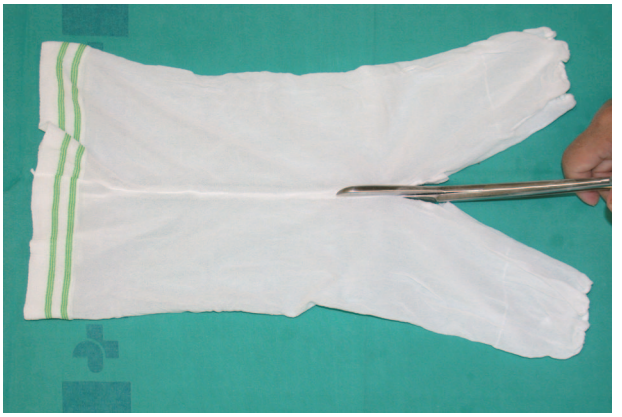

Fig. 1. Cutting the crotch of the leotard.

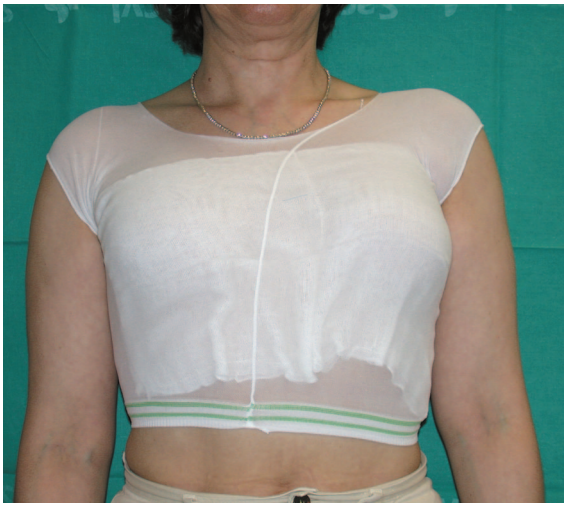

Fig. 2. Maintenance of the dressing with the garment.

The advantages we have found are as follows:

- The garment is easy to apply.
- The garment allows easy observation of the flaps and the nipple-areola region by simply lifting the leotard.
- The garment allows wound curing and dressing changes in the same way.
- The discomfort and skin lesions caused by sticking plaster and its removal are avoided.
- The elasticity of the leotard avoids the discomfort caused by elastic bandages.
- The patient may continue to wear the garment after discharge, which facilitates home cures.
- The garment is cheap.

We have not observed any adverse effects after 2 years of using the garment on patients who have undergone breast reduction, prosthesis implantation for breast enhancement, or reconstructive surgery involving flaps. Patient acceptance is highly satisfactory in all cases.

DOI: 10.1097/01.prs.0000298525.63082.74

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DISCLOSURE

The authors have no conflicts of interest to declare.

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Reply

Sir:

The wound care market in the United States was estimated to be worth $20 billion in 2006 and comprises two sectors, advanced and traditional.[1] Advanced wound care is one of the most lucrative and rapidly expanding market segments for both manufacturers and providers.[2] Traditional wound care products consist mainly of low-technology gauze-based dressings, such as woven and nonwoven sponges, conforming bandages, and nonadherent bandages. Although traditional wound care products are effective in many wound management environments, industry and commercial interests are focused on the wide range of new, advanced wound care products and treatments that are coming to market.[3]

At a time of scarce healthcare dollars, practicing proficient wound care is more important than ever. Proficient wound care means finding solutions that will not compromise the quality of care while achieving financially viable positive outcomes. Prescriptions for wound care products are not cheap, and costs are rising; it is therefore important that limited resources are used effectively and wisely.

Many challenges remain for healthcare professionals who are interested and involved in wound healing. Proficient wound care practices are fundamental to the management of surgical wounds and improving patient outcomes. Healthcare professionals must incorporate in their practice current professional standards, ensuring that each patient receives care that reflects quality and competence while being cognizant of effective resource management. Proficient wound care should be judged by its overall effectiveness, as measured by a series of positive events producing the desired optimal patient outcome.

The search for an ideal postsurgical breast dressing has led to the development of an impressive list of different materials and application techniques. Semipermeable adhesive membranes, gauze, and self-adherent elastic dressings may be among the most popular dressings for breast surgical wounds. Despite these advances, breast dressings may be cumbersome to apply and difficult to maintain in place, and if applied too tightly, they may cause vascular compromise and tissue necrosis. The ideal breast dressing should be easy to apply and remove, allowing any flaps and/or the nipple-areola complex to remain easily visible, but it should stay in place for the desired period of time. The dressing should provide evenly distributed pressure on the breast to decrease postoperative bleeding and limit edema and suture tension, conform to body contours, and keep the wound sterile. Benito et al. demonstrate proficient wound care with their simple, ingenious, and pragmatic technique of maintaining dressings after breast surgery.

Industry and commercial interests are focused on new, advanced wound care products and treatments, as these are the most financially profitable.[3] Healthcare professionals must demonstrate proficient wound care and remember that traditional wound care products are effective and efficient in many wound care environments.

DOI: 10.1097/01.prs.0000298544.24607.ca

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Free Flap Complications: Common Causes for Reexploration

Sir:

Bui and coworkers have published an excellent article on free flap reexploration based on a retrospective review of 1193 free flaps performed between 1991 and 2002 for reconstruction of surgical defects.[1] They carefully examined all vascular complications in suggesting how to face the most common causes for reexploration: pedicle thrombosis and hematoma/bleeding (also, an algorithm is provided for management of microvascular thromboses). They conclude that microvascular free tissue transfer has a low failure

rate, that different care is necessary for head and neck versus other free flaps (5 days of monitoring instead of 48 hours, because of the potential for pedicle kinking with neck movement), and that urgent reexploration is critical for salvage of compromised flaps.

The stated aim of their study was "to elucidate common *causes of* and methods for avoiding and managing" free flap reexploration. We would like to add to their discussion.

All the preoperative assessments in the article are grouped under the phrase "standard free flap tissue transfer principles were applied in all cases." No data are available on etiology (traumatic, oncologic, or malformative?), patient assessment, ischemia time, or the timing of reconstruction.

It is generally accepted that the ischemic time is irrelevant to flap survival up to 3 hours or to the point at which the no-reflow phenomenon occurs,[2] but what about the reexplored flaps reported? Besides, many studies have demonstrated how heparin offers protection from ischemia-reperfusion injury when it is administered during or before the ischemia time,[3] and it is not mentioned whether in reexplored flaps heparin with low-molecular-weight dextran (early part of the series) or with aspirin (after 1997) or heparin alone (after July of 2002) was administered.

In addition, the timing of free flap surgery (emergency, acute, or late for trauma reconstruction[4] and immediate or delayed for tumor reconstruction) is not quoted in the article. Some studies have reported high complication rates (up to 55 percent) for different flap types, with an increased rate in emergent lower extremity reconstruction and head and neck surgery compared with a low rate for breast reconstruction (when the patient is carefully assessed)[5,6] and if immediate soft-tissue coverage must always be considered in injuries where important structures are exposed.

Always interesting is the relationship between reexplored flaps and risk factors, among them mainly tobacco smoking, alcohol abuse, obesity, patient age, and preoperative irradiation, because of their influence on the results of microvascular free flap surgery.[7]

Surgeon experience should also be considered. A skilled surgeon—one who is able to suggest the most suitable flap in all conditions, plan the surgery properly, and perform the operation in an acceptable timeframe, with no technical errors—makes the difference in the final outcome.[8]

We hope that Bui and coworkers will add these data to their review, as it is true that "a comprehensive study examining the *causes* and methods of avoiding or treating [free flap] complications has not been performed." Their work could become an exhaustive reference point.

DOI: 10.1097/01.prs.0000298527.70283.06

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Reply

Sir:

I thank Dr. Annacontini and colleagues for their kind comments regarding the article entitled "Free Flap Reexploration: Indications, Treatment, and Outcomes in 1193 Free Flaps." My co-authors and I are glad that others have found our work helpful, and I hope that this letter answers the authors' questions.

Memorial Sloan-Kettering Cancer Center is a tertiary cancer center. Therefore, virtually all cases presented are for oncological reconstruction. This includes both immediate and delayed reconstruction of oncologic defects as well as defects resulting from sequelae of cancer treatment (i.e., radiation damage). The vast majority of cases in the article were for immediate reconstruction of cancer detects; however, some cases of delayed reconstruction were also included. Very few emergency cases were performed, since most cancer care at this hospital is scheduled electively. The general free flap population has previously been reported.[1]

I agree that, in general, ischemic time is irrelevant to free flap survival if it is kept below 4 to 5 hours. This was

the case in all of the flaps reported. It is difficult to keep track of ischemic lines in reexplored flaps, since it is not always clear exactly when the ischemic insult started. Therefore, I do not have any data available on this topic in the reexplored flaps.

As far as preoperative assessment, no addition screening, other than medical clearance, is generally performed. Rarely, hematologic consultation is obtained for evaluation of clotting abnormalities. Similarly, angiograms or magnetic resonance angiograms are obtained on patients with abnormal pulse examinations in whom a free fibula flap is planned.

The use of heparin is documented in the article; however, to clarify this point, I would like to add that heparin was administered systemically when anastomotic revision was performed. No heparin was given in negative explorations. Postoperative heparin was used at the discretion of the surgeon and was usually given if anastomotic revision was performed. My coauthors and I have recently reported the effect of our use of postoperative anticoagulation on the rate of microvascular thrombosis in a separate article.[2]

While I agree that it would be interesting to evaluate the relationship or potential relationship between comorbid conditions, this was not practically feasible in this study due to the low number of microvascular thromboses. In addition, similar analyses have previously been reported.[3]

Similarly, I agree that surgeon experience is a critical determinant of success in microvascular tissue transfer. However, an important additional factor that is often overlooked is the experience of our head and neck colleagues. I am fortunate to work with talented oncologists who can preserve critical structures necessary for complex reconstruction, and they are at least partially responsible for the relatively low rates of free frap loss.

I hope that these responses clarify some of the questions raised by Dr. Annacontini and colleagues.

DOI: 10.1097/01.prs.0000298528.79338.16

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Is the Dermis Graft a Reliable and Effective Option for Wound Coverage?

Sir:

Seung-Kyu Han and coworkers reported on the dermis graft for wound coverage.[1] They advocate (for small to medium-sized wounds on exposed areas, such as the face, neck, forearms, and hands) the dermis graft technique (applied to 53 wounds) as superior compared with the regular skin graft technique (applied to 33 wounds). Data were verified by the Vancouver Scar Scale and a visual analogue scale (for patient satisfaction). Better aesthetic outcomes for both the recipient and donor sites, a faster donor-site healing process, and reduced patient pain and discomfort were claimed to be the main advantages. By the way, we believe that many of the advantages mentioned in the article are due to the basic principles of plastic surgery.

It is well known that the amount of dermis included with the graft determines both the likelihood of survival and the amount of contracture. Also, the recovery and aesthetic outcome of the full-thickness graft are superior to those of the split-thickness graft.[2]

We usually harvest 0.015-inch split-thickness skin grafts applied principally on wounds with a well-vascularized bed, over periosteum, peritenon, or perineurium. However, use of both split- and full-thickness skin grafts follows strict indications (i.e., they are used for traumatic wounds that cannot be closed primarily, defects after oncologic resection, burns, scar releasing, congenital pathologies (i.e., syndactyly), and nipple-areola reconstruction). If the harvesting technique is correct, a proper dressing is applied, and good postoperative care is taken, morbidity is minimized.[3]

The dermis graft seems to have only a slight advantage in donor-site morbidity, particularly with regard to pain, because of the immediate covering of the exposed nerve fibers. On the other hand, as reported, for re-epithelialization of large defects, the healing process can be very long.

We never use skin grafts on the face and neck, except in oncologic cases, in the elderly, and when general conditions are compromised, thereby requiring a short operation time, where small to medium defects should be closed by means of local flaps. The main advantages of local flaps are that they have the same texture and pliability as the surrounding skin (improved cosmetic appearance), they have a reliable blood supply, there is minimal donor-site morbidity, they have the ability to restore sensation despite being technically more challenging, size is limited (two or more contemporary flaps may be necessary in large defects, if not skin expansion), and the arc of rotation can be limited.

In the end, skin substitutes (such as Hyalomatrix, Integra, AlloDerm, Apligraf, and so on) are today very common in clinical practice, principally for medium to large defects.[4–7] Skin substitutes or a combination of dermal substitutes and cultured epithelium may replace autografting. These procedures are still expensive and, in our experience, heal with a conspicuous

and unfavorable scar, but they do not require a donor site, and it is probable that in the near future, as the technology improves, skin substitutes will replace, in great measure, skin grafting.
DOI: 10.1097/01.prs.0000298529.93906.53

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Reply

Sir:

We appreciate the comments of Dr. Annacontini and colleagues concerning our article, "Dermis Graft for Wound Coverage" (*Plast. Reconstr. Surg.* 120: 166, 2007). With regard to their three major points (the advantage of the dermis graft in the recipient site, application of the dermis graft on the face and neck, and the use of skin substitutes), we present further clarification.

As described in our article, the dermis graft technique is definitely superior to the standard skin graft technique in terms of the aesthetic results of the recipient site as well as the donor site. Since the epidermis portion can be restored by epithelialization, which is induced by the migration and proliferation of adjacent epidermal cells, including melanocytes, the density and activity of the melanocytes as well as the precursor melanocytes of the epidermis of the graft develop to appear similar to those observed in the adjacent skin. Regarding the wound contraction of the recipient site after dermis grafting, the contraction of myofibroblasts can be inhibited by grafting with a greater dermal portion than a regular skin graft.[1,2] In addition, the overlapping fixation method in the dermis graft enables early suture removal, which results in inconspicuous stitch marks. This fixation technique also prevents scar widening and hypertrophy at the graft border, which is caused by the wound breaking strength after early suture removal, as the wound margin is sutured over the dermal tissue. The final results of the Vancouver Scar Scale were quite satisfactory and showed a statistical significance. According to the clinical survey, the patients were also satisfied with the results of the dermis graft.

Drs. Annacontini et al. also point out that donor-site morbidity can be minimized by using a correct harvesting technique and good postoperative care.[3] Despite the use of good harvesting techniques and dressings, however, hypertrophic scars are common along the donor sites in darker-skinned patients, including Asians. We can overcome this donor-site morbidity by using the dermis graft method.

We agree with Drs. Annacontini et al. that a local flap is the first and best option for wound coverage of the head and neck. In some cases, however, a local flap is not feasible, mainly because of the limitation of size and arc of rotation of a flap, particularly in young patients. Our dermis graft can be used as a replacement in these cases.

Skin or dermal substitutes and/or cultured cells, which are currently produced by advanced technology, may replace the regular skin graft.[4,5] However, these procedures usually heal with a significantly conspicuous and unfavorable scar. Further improvements are required in such technology to obtain aesthetic results as favorable as those obtained with the dermal skin graft method.

Although further investigation will be needed to determine the full value of the dermis graft technique, we believe that our method is safe and reliable and produces excellent results for wound coverage.
DOI: 10.1097/01.prs.0000298530.44884.2e

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The Results of Surgical Excision and Adjuvant Irradiation for Therapy-Resistant Keloids: A Prospective Clinical Outcome Study

Sir:

I find it quite disturbing—and perhaps it is a sign of the times—that the editorial decision was made to place the article entitled "The Results of Surgical Excision and Adjuvant Irradiation for Therapy-Resistant Keloids: A Prospective Clinical Outcome Study"[1] in the Cosmetic section of this *Journal*.

According to the definitions adopted in 1989 by the American Medical Association, "*cosmetic*" surgery is performed to reshape *normal* structures of the body to improve the patient's appearance and self-esteem," while "*reconstructive*" surgery is performed on *abnormal* structures of the body caused by congenital defects, developmental abnormalities, trauma, infection, tumors, or disease. It is generally performed to improve function, but may also be performed to approximate a normal appearance." Therefore, keloids, I believe all would agree, are certainly "abnormal" structures, as they are a type of scar tissue that progressively invades the surrounding normal skin. Indeed, some even consider keloids to be a type of "tumor." It is curious, therefore, that the Editor decided to place this article in the Cosmetic section.

It has been my own experience that surgery to treat keloids is now increasingly denied by managed care companies as being a type of cosmetic surgery. Even the initial consultations to discuss treatment options for keloids are being denied. Now comes this article, at perhaps no fault of the authors, which seemingly corroborates this misguided practice by insurance carriers. It is unfortunate that by categorizing the treatment of keloids as a type of cosmetic surgery, the Editor is now aiding and abetting managed care companies that feel likewise, with total disregard for the patient with symptoms of pain, tenderness, pruritus, infection, and disfigurement.

I do not know whether the decision to publish a keloid article in the Cosmetic surgery section of our *Journal* was that of the authors or the editors. In either case, I believe this is a disservice not only to those of us who still practice reconstructive surgery but also to our patients who might now find it ever increasingly difficult to secure third-party payor coverage for correction of these significant deformities.

DOI: 10.1097/01.prs.0000298531.02254.a4

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Reply

Sir:

I appreciate Dr. Arons' scrutiny in delineating which topic belongs in the Cosmetic or Reconstructive sections of the *Journal*. The Editorial Board decided more than 2 years ago when I became Editor-in-Chief that the way our readers read *Plastic and Reconstructive Surgery* is by topics of interest. We all agreed that would be great if we could do this and therefore we did it. The response from our readers has been overwhelmingly positive. The *Journal* is now much easier to read, and the sectional divisions also facilitate submission and review of manuscripts. With those objectives accomplished and those benefits obtained, however, we also know that in plastic surgery it is very difficult to separate almost anything into purely cosmetic versus purely reconstructive topics or subjects, as they go hand in hand: one always wants to achieve the optimal cosmetic result in every reconstructive as well as cosmetic case! The challenge of having to segregate articles into specific topics is thus occasionally artificial and awkward.

Having said all this, the Editor-in-Chief is responsible for delineating the placement of all articles, so I would not hold the esteemed authors of this excellent article in contempt. I would wish that this *Journal* and I as Editor-in-Chief would have all the prowess, powers, and influence Dr. Arons espouses and implies that we have: that one listing of an article in the Cosmetic section of our *Journal* will further impede the ability to obtain insurance coverage for keloids. Personally, I have not been able to procure insurance coverage for keloids and/or scar revision surgery for almost a decade, except for burn reconstructive patients. Perhaps his carrier still intermittently may cover this, which is wonderful. However, in today's reimbursement climate, unless a body malfunction, injury, or growth is truly functional in nature—such as constriction or a contracture of the elbow—it is considered cosmetic. Such designations are facts of our professional lives in this country. I apologize and would be taken aback if an insurance carrier uses this article to substantiate its

rationale for refusing to cover a keloid scar revision. That would be the exact opposite of any other rationale insurers have used in the past to refuse to authorize other valid procedures, such as breast reductions. As we have all encountered, insurance carriers rarely evoke science to make their decisions; if they did, they would not be able to refuse most of these reconstructive procedures performed in plastic surgery.

Again, I applaud Dr. Arons for bringing this matter to my attention and appreciate his reading the *Journal* with such intensity and resolve.

DOI: 10.1097/01.prs.0000298532.57936.75

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Modified Vertical Abdominoplasty in the Massive Weight Loss Patient

Sir:

I have some comments on the article by Borud and Warren in the May 2007 issue entitled "Modified Vertical Abdominoplasty in the Massive Weight Loss Patient."[1] The article highlights a valuable approach that is probably not performed often enough. The modified "fleur-de-lis" abdominoplasty with an inverted V vertical midline pattern of resection is a good option for many massive weight loss patients (Figs. 1 and 2). These patients accept the vertical scar well, and this procedure can eliminate the need for a circumferential body lift.

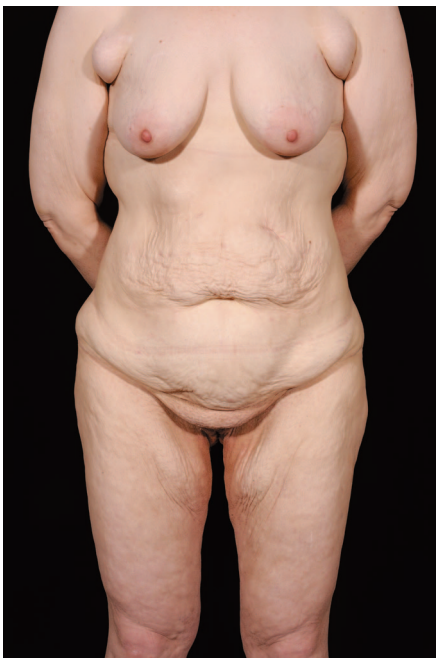

Fig. 1. A 40 year-old woman after a 180-pound weight loss.

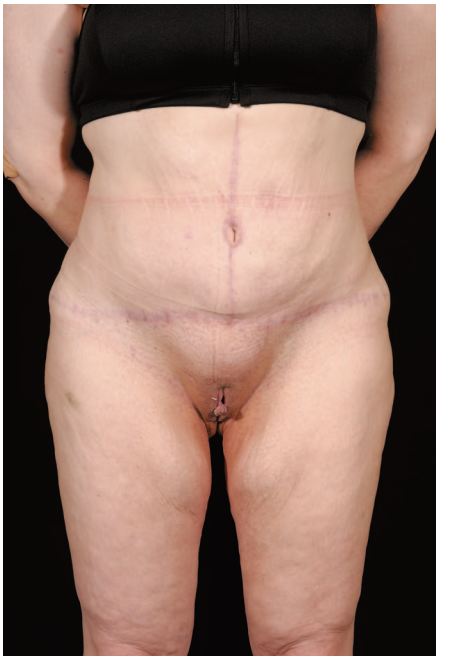

Fig. 2. View of the same patient shown in Figure 1, 2 months after vertical abdominoplasty.

I do take issue with the authors on a technical point. They perform extensive undermining of the flaps, with an associated 37 percent incidence of "wound dehiscence" (probably really flap necrosis). This operation can be performed in most patients with *minimal or no undermining*. The redundant tissue is resected "where it lies." The vertical resection provides adequate exposure for hernia repair and diastasis correction, with minimal elevation of the edges. Of course, this "no flap" approach markedly reduces the incidences of necrosis and seroma.

I also think, as do the authors, that making the superior incision first is an excellent idea that makes the operation safer and easier. Making the superior incision first can be a good option even in conventional (transverse) cosmetic abdominoplasty performed on patients of normal weight.[2]

DOI: 10.1097/01.prs.0000298546.23594.a6

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Reply

Sir:

I am indebted to Dr. Beraka for his thoughtful and insightful comments on "Modified Vertical Abdominoplasty in the Massive Weight Loss Patient" (*Plast. Reconstr. Surg.* 119: 1911, 2007). One of the primary considerations in the modified abdominoplasty is maintaining adequate vascularity to the entire flap to avoid T-junction necrosis or flap dehiscence in this area, also of ischemic origin. Dr. Beraka brings up a topic that my coauthors and I did not adequately explore in our discussion. We would like readers to note that the patient population in this study represented patients with the highest possible risk: 80 percent had incisional hernias from prior open gastric bypass that were simultaneously repaired at the time of modified vertical abdominoplasty. This high hernia incidence represents the consequences of the open gastric bypass era. In the study's 64 patients, the odds ratio for wound complications in hernia versus nonhernia patients was higher by a factor of 2.1:1. Within the nonhernia group, which was the typical population in most areas of the United States in 2007, the primary risk factor for wound complication was body mass index at the time of body contouring. At Beth Israel Deaconess Medical Center, physicians are constantly re-evaluating and revising the treatment algorithm based on their accumulated experience. Based on a review of the data on nonhernia patients, a body mass index greater than 32 is associated with a significantly higher risk of wound complications when modified vertical abdominoplasty is performed (1.8:1). In the most current algorithm (Fig. 1), the modified vertical abdominoplasty is offered only to nonhernia patients with a body mass index less than 32 who have bidimensional skin excess and who desire a front-only procedure. When this algorithm was applied retrospectively to the study group presented in the report, the risk of even a minor wound complication fell to 8 percent, which is arguably an acceptable rate in this selected group of massive weight loss patients.

It should be noted that in the current climate of laparoscopic gastric bypass or lap band procedures, the nonhernia patient with a body mass index less than 32 and significant transverse skin excess is commonplace. Many of these patients cannot afford or do not wish to have the aesthetically superior circumferential lower body lift procedure, yet desire the optimum outcome from a front-only procedure. For patients in this category willing to accept a vertical scar, we can provide a significantly improved aesthetic result with the modified vertical abdominoplasty versus standard abdominoplasty, with a low risk of complications.

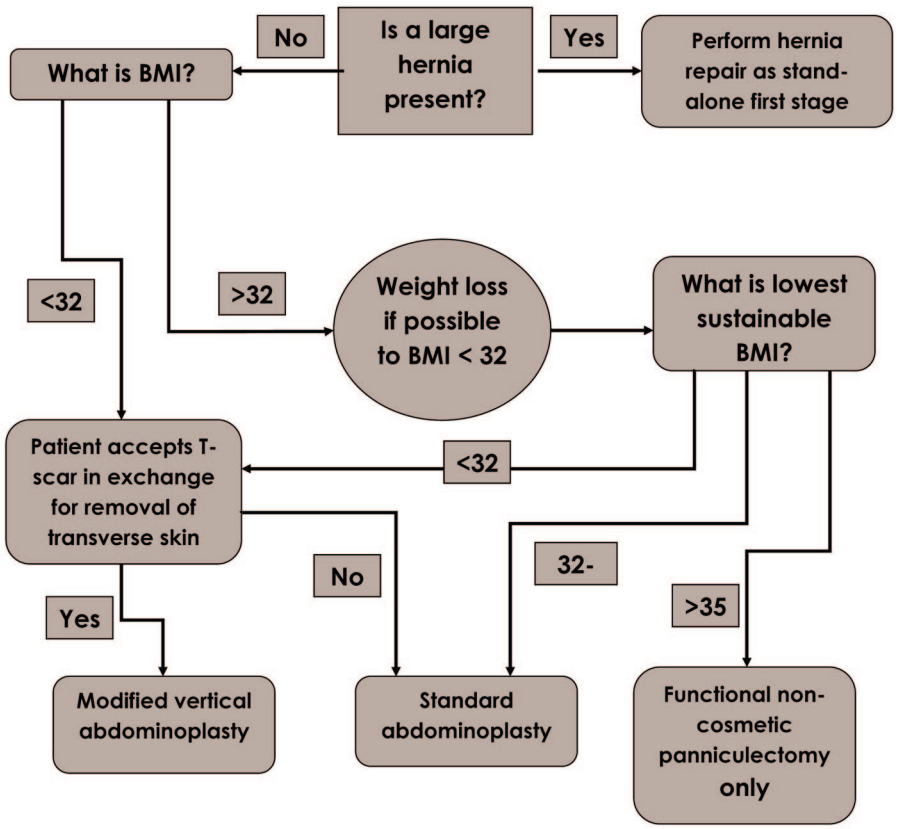

Fig. 1. Current Beth Israel Deaconess Medical Center algorithm for massive weight loss patients with significant transverse skin excess who desire front-only procedures for lower body contouring (*BMI*, body mass index).

Repair of large hernias requires substantial flap elevation to remove the hernia sac and identify healthy fascia for the repair. In cases of very large hernias, components separation combined with components separation and abdominal wall plication is a very effective technique. Since any approach to the large hernia inevitably results in sacrifice of significant lateral and medial row perforators, Beth Israel Deaconess Medical Center no longer offers concurrent modified vertical abdominoplasty in patients with abdominal wall hernias. The hernia is repaired first and then abdominal contouring is performed at a separate stage. It is hoped that the large-hernia massive weight loss patient will become a categorization relegated to historical interest with the popularization of minimally invasive bariatric procedures.

In the nonhernia patient, high residual body mass index at body contouring correlates with an increased risk of wound complications and flap necrosis due to the inherently poor vascular supply to the thick fat layer present in the abdominoplasty flap. Thus, at Beth Israel Deaconess Medical Center, a somewhat arbitrary body mass index cutoff of 32 has been established for offering the modified vertical approach. The procedure is also not offered to patients with extreme degrees of transverse skin excess in whom an adequate fleur-de-lis correction would require chasing the "dog-ear" up into the sternal area.

Despite these limitations and caveats, I believe the modified vertical abdominoplasty represents a safe, viable alternative for selected massive weight loss patients who are committed to front-only procedures. My coauthors and I stand by our assertion that in these patients with substantial skin excess above and below the umbilicus, the procedure frequently cannot be adequately performed with little or no undermining, as Dr. Beraka asserts. However, I fully agree with Dr. Beraka's excellent observation that, with care, many key lateral perforators can and should be preserved in the fleur-de-lis approach to minimize wound complications, even if significant flap elevation is required to adequately translate the loose tissue.

DOI: 10.1097/01.prs.0000298534.43364.17

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Wide-Awake Approach

Sir:

I read with interest the article entitled "Immediate Thumb Extension following Extensor Indicis Proprius–to–Extensor Pollicis Longus Tendon Transfer Using the Wide-Awake Approach" by Bezuhly et al.[1] I have been operating to correct the paralytic deformities of limbs (both hands and feet) of leprosy-affected persons for more than a decade using local infiltration of lignocaine 0.5% with adrenaline 1:200,000. I do not give any preoperative sedation and also do not use a tourniquet. To maintain a dry field, I keep the hand or foot on a 15-cm-high platform of towels. The operative field is almost dry but the tissues remain wet enough. Sometimes during the summer months, the wound is kept moist with wet saline sponges.

The technique has the added advantage that tension adjustments and balancing are easy. After fixation sutures have been inserted, digit position and movement can be tested and tension can be readjusted if required, before the skin wound is closed.

I test the opponens by asking the patient to move the donor finger, the ring finger for superficialis "Y" opponens, and the index for extensor indicis proprius transfers. For claw finger correction (either modified lasso or extensor to flexor four-tail transfers), a similar technique is used. Digit movement and deformity correction are assessed before the wound is closed.

I am not very convinced that integration of movement can occur immediately after the transfer, as suggested by the authors. In their cases, probably the same principal nerve is innervating both the muscles, which are extensors.

Other advantages, such as safety and time and cost savings, do exist. With appropriate adaptation, these corrective procedures can be used as day surgery procedures if not office procedures.

The references cited on page 1511, paragraph 2, line 12 (i.e., references 12 through 15) do not match the text.

DOI: 10.1097/01.prs.0000298535.99085.88

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Reply

Sir:

I am delighted to hear that Dr. Malaviya has also had very favorable experiences performing tendon transfers in unsedated wide-awake patients using pure local anesthesia with epinephrine hemostasis and no tourniquet. I agree that visibility with the wide-awake technique is excellent despite the lack of a tourniquet, and that the added safety of deleting the risks of general anesthesia is a distinct advantage.

His experience with tendon transfers in leprosy patients has also been favorable for proper tension ad-

justment of the tendon transfer, as the unsedated pain-free patient is able to move the transfer comfortably. This allows the surgeon to make transfer tension adjustments before the skin is closed. I have found that getting the tension right (not too tight and not too loose) is much easier when the patient is awake and cooperative than when general or regional anesthesia is used, which paralyzes muscles or makes the patient uncooperative. I also agree with Dr. Malaviya that this technique is much less expensive and faster, and can be performed more conveniently without the requirement of general anesthesia.

My main tendon transfer experience is with extensor indicis to extensor pollicis longus, but I have also used it for flexor digitorum superficialis to flexor pollicis longus. Unlike Dr. Malaviya, I have found that patients are able to easily move the transfer right in the operating room before the skin is closed without having to "learn the transfer" in every case. However, it may well be that Dr. Malaviya's experience has the benefit of a much larger number of patients in his leprosy hospital in India, and that I just have not treated enough cases yet to see patients who are not able to immediately "learn the transfer."

I am grateful that Dr. Malaviya has correctly noted that page 1511, paragraph 2, line 12 of our article[1] should cite reference 15 alone, not references 12 through 15, as is currently indicated. I apologize for this oversight and any confusion this may have caused readers.

The fourth dimension of hand surgery is active movement. Being able to watch comfortable, pain-free, patient-active finger movement and make surgical adjustments before the skin is closed in tendon transfers, flexor tendon repair, and many other hand operations has been a major bonus of the advent of the wide-awake approach to hand surgery.[2] Now that the safety of epinephrine in the finger has been established,[3,4] the tourniquet, which was the main obstacle to watching active movement, is no longer required for most of our hand operations. Surgeons can now test and observe freshly repaired flexor tendons actively gliding in their sheaths, and adjust the tension of the moving tendon transfers before closing the skin.

DOI: 10.1097/01.prs.0000298536.40257.bd

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The Distally Based Sural Flap

Sir:

We would like to make a critical comment on the article by Fodor and colleagues entitled "The Distally Based Sural Musculoneurocutaneous Flap for Treatment of Distal Tibial Osteomyelitis" (*Plast. Reconstr. Surg.* 119: 2127, 2007). Their opinion that "although there are reports of raising the flap and sparing the sural nerve, we consider this maneuver dangerous, especially for patients with comorbid conditions and scars because of previous orthopedic procedures" is based on little evidence and could potentially cause misunderstanding among the readers of the *Journal*.

In the results of our anatomical study (submitted data), we found by angiography using cadavers that the small extrinsic vessels around the sural nerve and lesser saphenous vein were important structures, especially in the distally based sural flap, and that those located around the sural nerve were particularly important for flap survival. However, compared with the numerous extrinsic vessels of the sural nerve and lesser saphenous vein, the sural nerve has relatively few intrinsic vessels.

Moreover, we have successfully harvested 28 distally based sural flaps that possessed the deep fascia and lesser saphenous vein but not the sural nerve as a means to prevent complications. In all of these cases, there was no instance of necrosis resulting from preservation of the sural nerve (submitted data). Thus, we consider that the sural nerve itself is not a critical factor for flap survival. The sural nerve can easily be detached from the flap by making a meticulous dissection from the deep fascia. This procedure causes less bleeding[1] and means that the sural nerve can be preserved without severely damaging its small extrinsic vessels.

Harvesting of the sural nerve is not without disadvantages. These include the development of a small sensory defect in the lateral aspect of the foot and the possibility of neuroma formation or reflex sympathetic dystrophy.[2] We do not deny the utility of reconstructions using ordinally[3] or modified distally based sural flaps[4,5] containing the sural nerve, because the purpose of these operations is different in each case. In cases where there is a large and distal lower leg soft-tissue defect that needs to be reconstructed without a free flap for some reason, the importance of preservation of the sural nerve should be reconsidered.

DOI: 10.1097/01.prs.0000298537.96722.9d

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Reply

Sir:

We thank Drs. Ogawa et al. for their interest in our article, "The Distally Based Sural Musculocutaneous Flap for Treatment of Distal Tibial Osteomyelitis" (*Plast. Reconstr. Surg.* 119: 2127, 2007).

They claim that the sural nerve can be preserved during flap harvesting. This is not new, and there are reports of using the sural flap with sparing of the nerve. This is an elegant procedure with good results for suitable patients. They distract the reader by comparing the sural fasciocutaneous flap (their experience) with the sural musculoneurocutaneous flap (our experience and report). In our article, we describe our technique in which we include a cuff of muscle surrounding the lesser saphenous vein and sural nerve. The pedicle of the flap has the following components: lesser saphenous vein with its two accompanying arteries, median sural nerve, and median superficial sural artery. There are many anastomotic networks between the vascular pedicle and the lateral and medial maleolar perforators. In the distal part of the flap, as described, there are important anastomoses between the gastrocnemius muscle and the vascular axis of the sural nerve.[1,2] It is very important to preserve them to keep the muscle cuff alive. There is no question that the blood supply of this muscle is delicate. We chose to use this composite flap and not the classic fasciocutaneous flap to fill the bone gap after bone debridement (osteomyelitis).

Taking into consideration that all our patients had osteomyelitis, which increases the rate of complications,[3] that half of our patients had maleolar scars (previous operations) that interrupted the perforators, and the fact that a cuff of muscle was included in the flap, sural nerve harvesting was mandatory for this type of flap. We could not find any publication about raising the sural musculoneurocutaneous flap without the nerve for patients with perimaleolar scars and osteomyelitis. It is certain that we would not perform this maneuver and would never teach a resident to do it when dealing with comorbid patients, as mentioned above.

DOI: 10.1097/01.prs.0000298538.64858.23

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Pressure Therapy with a Round Rod for Hypertrophic Scars

Sir:

Hypertrophic scars represent an abnormal, exaggerated healing response after skin injury such as trauma, surgical intervention, or burn, and usually cause major physical, psychological, and cosmetic problems. The pathogenesis for this deformity is the unusual and abnormal proliferation of fibroblasts and the overproduction of collagen. It is more important to prevent scar formation at the early stage after wound healing than to treat established scars.

There is no universally accepted treatment regimen for the management of hypertrophic scars. A combination of treatment methods should be applied to improve the lesions. These include pressure therapy, corticosteroid injection, silicone gel sheeting, hydrogel dressing therapy, laser therapy, radiotherapy, cryotherapy, and adhesive tape therapy. However, Mustoe et al. believed that silicone gel sheeting and intralesional corticosteroids were internationally applicable for the management of a wide variety of abnormal scars and

concluded that these were the only treatments for which sufficient evidence existed to make evidence-based recommendations.[1] Besides, pressure therapy was also considered to be one of the most widespread strategies.[2]

However, some hypertrophic scars are in special locations, such as near the articularis, and are smaller lesions (e.g., <100 mm^2). The silicone gel sheeting cannot be kept close to the lesions because of the movement of the joints, and the use of elastic bandages or pressure garments is obviously not suitable for scars that are too small. Moreover, high pressures can lead to treatment suspension, unpleasant side effects, and even deformity. Some patients do not accept intralesional drug injection. In such clinical situations, we have conducted pressure therapy using a small round rod.

The technique we suggest is the application of a small round rod, such as a pen or pencil. The rod is placed transversely on the surface of the scar and rolled; meanwhile, suitable pressure is exerted on the hypertrophic scar by the patient him- or herself for 4 to 5 minutes, five to six times a day, and should be great enough for the patient to able to tolerate it. Fourteen of these scars in 10 patients were managed with this technique. To compare the effects of treatment, six larger scars in six patients were selected and randomized into two groups: half of each scar comprised the control group and the other half of each scar was treated with pressure therapy. The treatment lasted for 3 to 6 months. Scar volume, height, erythema, and pliability were measured at months 0, 1, 3, 6, and 9. Desquamation appeared on the surfaces of the lesions while the therapy was progressing. The volume of treated segments decreased significantly after 1 month of treatment. Treated segments showed significantly greater improvement than control segments after 3 months of treatment. Elasticity of treated segments was significantly greater than that of the control segments after 3 months of treatment (Figs. 1 and 2). We have already obtained impressive results after treating all of

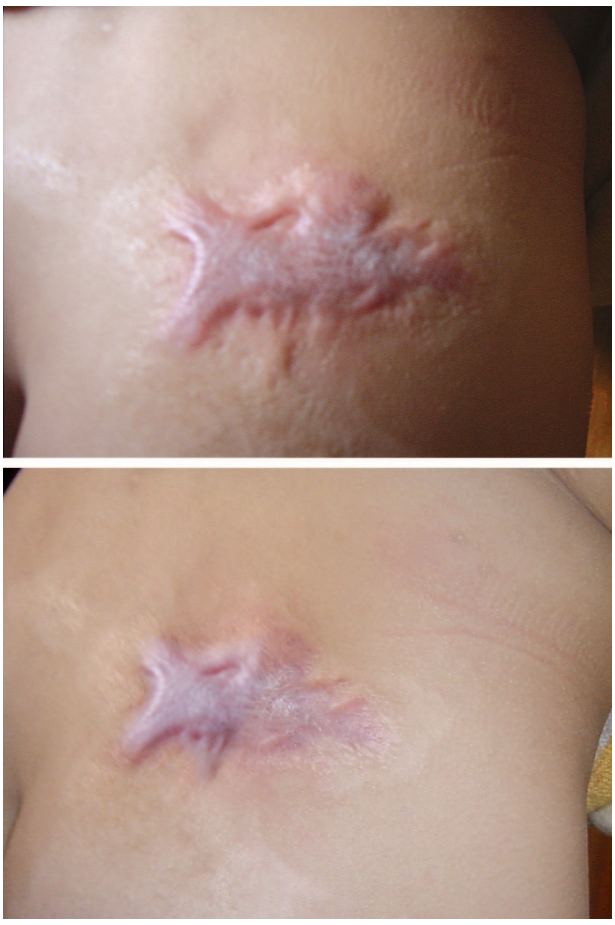

Fig. 1. (*Above*) Patient with a hypertrophic scar on the right buttock of 6 months' duration; the scar was erythematous and raised above skin level. (*Below*) The scar was divided into two halves, with the left half being the control and the right half receiving pressure therapy. After 6 months of treatment, the right half of scar was flatter and lighter compared with the left half.

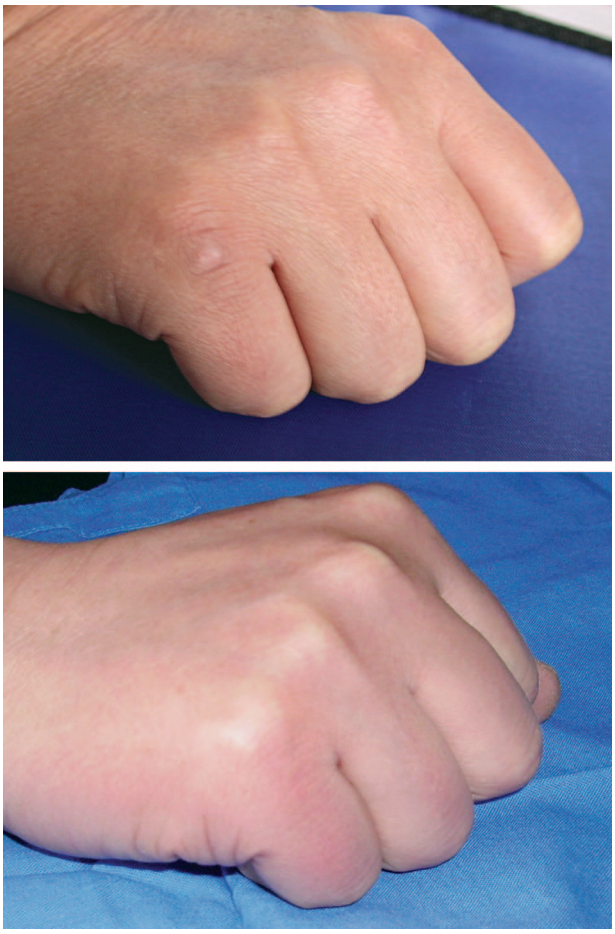

Fig. 2. (*Above*) A hypertrophic scar at the dorsum of the metacarpophalangeal joint of the right digitus minimus, before treatment. (*Below*) After 1 year of treatment, the scar regressed and pigmentation almost returned to normal.

our cases. Six to 12 months of follow-up results showed that no scar recurrence was seen and that pigmentation appeared in the treated regions in some cases. This technique seems to be a satisfactory method for managing hypertrophic scars.

DOI: 10.1097/01.prs.0000298539.78709.09

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Reply

Sir:

We read with interest the letter by Zhong and He on the use of pressure therapy for hypertrophic scars using a round rod, and we would like to make certain comments on it.

Keloids and hypertrophic scars are two discrete pathological, histological, and clinical entities. Scars within these categories will vary both in their clinical course and in their response to treatment.[1] The selection of the appropriate treatment, in each case, is based on the type, size, and location of the scar.

Pressure application is an unquestionable weapon in the hand of a doctor, whether dermatologist or plastic surgeon, who deals with wound healing. Pressure application is not an accidental affair as far as the amount of force used, the amount of time pressure is applied, and the orientation of the pressure.

Pressure therapy is a specific entity and has been established as an effective option that is known to reduce the volume of scar tissue. This is considered to occur as a result of localized hypoxia, resulting in fibroblast degeneration and cell detriment. Besides this, it is claimed that mechanical pressure changes glycosaminoglican levels and capillary permeability during the early phase of wound healing, causing shortening in the scar formation time. The pressure exerted should be at least 24 mmHg to exceed the inherent capillary pressure, but it must be below 30 mmHg; otherwise, it critically reduces peripheral blood circulation.[2]

Successful treatment can result after consideration of the following parameters: (a) the applied pressure should always be measured to achieve local hypoxia, without critically reducing local blood circulation; (b) the pressure should be applied 24 hours a day and for a period of 20 to 25 weeks[3]; (c) the use of custom-made pressure garments is preferable, because they apply isomorphic or controlled pressure to the entire surface of the scar only; and (d) the material that will come in contact with the skin should be hypoallergic and atraumatic. Finally, the patient should be evaluated to determine whether he or she is collaborative enough to follow the treatment.

Thus, knowing the way that pressure application affects the hypertrophic scar or keloid, we also understand the requirements this treatment has in terms of time, technique, and mainly patience from the patient. If "you get what you pay for" is a rule of the market in the domestic economy, in perfection of pressure therapy, the gnome that applies is "the more precise you are in terms of measurement and orientation, the more you approach the cure."

DOI: 10.1097/01.prs.0000298540.30613.52

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Tufted Angioma–Associated Kasabach-Merritt Syndrome Treated with Embolization and Vincristine

Sir:

This letter is in reference to our communication entitled "Tufted Angioma-Associated Kasabach-Merritt Syndrome Treated with Embolization and Vin-

cristine," published in the April 1, 2007, issue of the *Journal* (*Plast. Reconstr. Surg.* 119: 1392, 2007).

After publication of this communication, we received a comment on it from Professor John Mulliken, an expert in the field of vascular anomalies. Professor Mulliken questioned the diagnosis of tufted angioma in this case on the basis of the published views of the lesions. He felt that the clinical photograph and hematological studies pointed to the lesion being a rapidly involuting congenital hemangioma.

After receiving Professor Mulliken's comments, we reviewed the pathology in this case. Unfortunately, the pathologist who initially reviewed the case and gave a diagnosis of tufted angioma has since left the organization to work abroad. We believe, however, that the diagnosis of tufted angioma was made in good faith.

The pathology was hence reviewed by our senior pediatric pathologist. His comments tend to support Professor Mulliken's assertions in that histologically the lesion cannot be called a tufted angioma. A histological diagnosis of this lesion cannot be made owing to the small amount of tissue available. Although it is clear that this is a vascular anomaly, we do not have a firm diagnosis in this case, although we respect Professor Mulliken's view and it is quite possible that this is a rapidly involuting congenital hemangioma.

We apologize for this situation. As noted above, the diagnosis of tufted angioma was made clinically and histologically in good faith. Of course, we would be willing to accept any editorial decision that you feel appropriate to be made in this situation.

DOI: 10.1097/01.prs.0000298541.92722.5d

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American Association for Accreditation of Ambulatory Surgery Facilities

Sir:

I am writing in response to the communication by Donato A. Viggiano, M.D., entitled "Inspection Criteria for the American Association for Accreditation of Ambulatory Surgery Facilities" that was published in the May 2007 issue of the *Journal* (*Plast. Reconstr. Surg.* 119: 1983, 2007). The readers should be assured that the American Association for Accreditation of Ambulatory Surgery Facilities (AAAASF) considers its standards to be a "living document" and is constantly reevaluating and altering them as medical science and laws indicate. While the AAAASF always welcomes and reviews suggestions for standards modification from any source at any time, Dr. Viggiano has chosen an unusual forum to offer his comments and concerns, since the AAAASF and the American Society of Plastic Surgeons are two separate, distinct, and independent entities. However, the issues raised do warrant comment and clarification.

Standard 21-050, "The operating room(s) is adequately ventilated and temperature controlled between 68 and 72 degrees Fahrenheit." The original source for this standard was the 1997 edition of *Guidelines for Design and Construction of Hospital and Healthcare Facilities* (p. 58, Table 2), as published by the American Institute of Architects Academy of Architecture for Health with assistance from the U.S. Department of Health and Human Services. The actual temperature range recommended was "68 to 73 degrees Fahrenheit." Although the recently published 2006 version of that publication has been substantially expanded and modified in other areas, no change was made in the recommended temperature range. According to the American Institute of Architects Web site:

> "The Guidelines document is referenced by architects, engineers, and health care professionals throughout the United States and in other countries who are planning new or renovated health care facility construction. Authorities in 42 states, the Joint Commission for the Accreditation of Healthcare Organizations, and several federal agencies use the Guidelines as a reference, code, or standard when reviewing construction designs and plans and completed health care facilities."

The AAAASF certainly appreciates the need to maintain the patient's body temperature as near normothermic as possible, and indeed has another standard that requires the intraoperative monitoring of the patient's body temperature in appropriate cases. The lead article in the winter 2006 issue of the AAAASF's newsletter was, in fact, "Patient Safety: The Importance of Temperature Regulation before and during Anesthesia." Although various forms of "patient warmers" are widely available and recommended for both the operating and recovery rooms for both patient safety and comfort, operating room temperature can certainly be varied when they are not available or for any overriding concerns about patient safety. The AAAASF is also aware that maintenance of intraoperative normothermia is even a "pay for performance" measure that has been submitted by the American Medical Association consortium on perioperative care, with a goal of 36°C upon arrival in the postanesthesia care unit or designated recovery area. Dr. Viggiano's question, "Does the operating room have to be maintained between 68°F and 72°F 24 hours a day, 365 days a year?" is, it is hoped, offered "tongue-in-cheek," since that is clearly not the intent of the standard, which was designed to provide the most widely accepted guidelines for the operating

room when in use. While a less specific standard terminology of "adequate" could certainly be adopted, many more questions have arisen about clarification of what "adequate" means.

Standard 261-014, "Pneumatic boots or alternative devices for anti-embolic prophylaxis (such as TED stockings or ACE bandage wraps) are employed for all but local anesthesia cases of one (1) hour or longer and when medically indicated." With respect to this standard, let me offer the following comments regarding Dr. Viggiano's recommendations based on his own preference for the low-dose ketamine/benzodiazepine protocol.

First, from Jeffrey Apfelbaum, M.D., an AAAASF board member and professor and chair of anesthesia and critical care at University of Chicago:

> "While I'm not aware of any standards, guidelines, or policies that address this issue, I think it would be a mistake to consider the use of ketamine/benzodiazepine as having exercised adequate antiembolic prophylaxis. Though the author of this letter to the editor (Viggiano) cites one physician's personal experience (reference 5) and a reasonably well-done study (reference 4) that looks at platelet aggregation with ketamine use, I have not seen specific scientific evidence to suggest that ketamine could be utilized as antiembolic prophylaxis, especially in these high-risk cases."

From Felmont Eaves, III, M.D., chair of the American Society for Aesthetic Plastic Surgery Safety Committee:

> "On the issue of [venous thromboembolism] prophylaxis, I agree entirely with the standard and there are no data to back up his claim concerning ketamine/benzodiazepine. However, why wouldn't he use pneumatic compressions anyway? It is cheap [and] has essentially a zero risk of complications, and even if he is correct would at worst have no deleterious effect and at best might decrease the (low) risk even further."

From Phil Haeck, M.D., chair of the American Society of Plastic Surgeons Safety Committee:

> "I would point out that the purpose of the temperature guideline was also to make sure that the room temperature CAN be controlled at all by the personnel. It is not the intention to police the temperature around the country so much as to make sure there is the ability to adjust it at all times. . . . I think we should emphasize that the standard of care is to use compression stockings" and "is recognized by ALL accreditation agencies not just AAAASF as well as becoming a hospital standard .. . . Going without compression stockings in light of unproven evidence about ketamine and peripheral pooling is not supported by the (ASPS) Patient Safety Committee."

The AAAASF is committed to promoting the safest possible environment for patient care in its accredited facilities, and has developed Standards that are considered by most "stakeholders" involved in ambulatory surgery to be the industry's accepted standard. Why someone would balk at the use of patient temperature control devices for more precise maintenance of normothermia, or the use of any additional, risk-free, noninvasive modalities to minimize the potential risk of deep vein thrombosis is indeed perplexing.

DOI: 10.1097/01.prs.0000298542.76677.03

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Willing Victims of Baker's "Idol of the Theater"

Sir:

I read the editorial entitled "The Nature of Scientific Evidence" by Dr. Spear (*Plast. Recontr. Surg.* 119: 2310, 2007) and would like to comment on the other, not-so-rosy side of peer review.

Peer review is a fancy term for the historical Bacon's "Idols of the Theater," the contemporary version of which is "group think." David Wootton[1] documents the medical setbacks brought on by what he calls willing victims of Bacon's "Idols of the Theater" because of their prepossession by the existing theories, which is exactly why the intellectual origins of modern medicine remain a relatively unexplored field. Case in point: The neuropathic lesion of median neurodesis to flexor pollicis longus sheath in the proximal carpal tunnel that I found in 1999,[2] which undergirds my universal theory for cumulative trauma disorder and carpal tunnel syndrome on the basis of "traction neuritis," could, should, and would have been found in the early 1980s by Gelberman et al., particularly in view of their conclusion: "It must be concluded that factors other than increase in pressure in the carpal tunnel play a significant role in the causation of signs and symptoms of this condition."[3] But they did not find it because they were prepossessed by the existing theory of compressive neuropathy. And what about the group think responsible for the disingenuous histopathology of chronic flexor tenosynovitis in carpal tunnel syndrome?[4] The disingenuousness that I want to point out is in the misinterpretation of the histopathology and not the histopathology itself. At the heart of the debate over inflammation is this simple question: Can chronic flexor tenosynovitis occur without the histological preponderance of inflammatory cells? The answer is, "Heck, yes," because chronic inflammation is defined in standard textbooks of pathology as inflammation of prolonged duration in which active inflammation, cellular destruction, and attempts at repair are all proceeding simultaneously and the preponderance of one or the other is the histopathological determinant. Therefore, if the preponderant process is that of cellular destruction, as in rheumatoid tenosynovitis, the histopathology will be that of inflammatory cells, inflammatory cells, and more inflammatory cells. On the

other hand, if the preponderant process is that of repair, such as what I am describing in cumulative trauma disorder/carpal tunnel syndrome,[2] then the histopathology will be that of fibrosis, fibrosis, and more fibrosis. That is exactly what was found by Schuind et al.,[4] and yet this evidence is deliberately misconstrued by group think to fit their consensus of noninflammation. If, instead of jumping the gun on noninflammation, group think had probed the fibrosed tenosynovium of carpal tunnel syndrome with immunohistochemical staining for Cox-2, guess what? They would have found the "smoking gun" Cox-2 expression in the fibrosed tenosynovium of carpal tunnel syndrome.[5]

Finally, contrary to popular belief, the core of science is not a mathematical model but intellectual honesty, and I cannot imagine that a true scientist will ever put his or her passion above this core value.

DOI: 10.1097/01.prs.0000298547.99486.61

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