LETTERS AND VIEWPOINTS

GUIDELINES

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Letters

Human Face/Scalp Alloflap Harvesting Technique

Sir:

The human face/scalp alloflap is slowly being introduced in the reconstructive surgery armamentarium. Surgical, immunological, and ethical issues of face allograft transplantation are being assessed by different teams worldwide.[1–7]

Our aim is to discuss modifications to the human face/scalp alloflap recently reported by Siemionow et al.[8,9] We performed mock hemifacial and full facial flap donor harvests in five fresh human cadavers. Potential indications for full human face/scalp alloflap transplantation would be extensive disfigurement, most probably the lack of any functioning facial/expression muscles, and loss of the nose and/or ears, with complex defects in the periorbital and buccal regions.

In their recent anatomical and mock-surgical studies, Siemionow et al. suggested a skin-only facial alloflap.[8] This would make the final functional result depend on the absolute integrity of the recipient's facial muscles (a very unlikely situation in a severe burn or trauma patient) and the perfect adherence of the alloflap to the recipient muscular layer. It also seems unlikely that healing of the alloflap to the recipient muscles would duplicate the delicate cutaneous insertions of these muscles, which is necessary for nearly normal facial movement. Thus, it seems more reasonable to transfer the full-thickness human face/scalp alloflap, with the muscle layer, and to reinnervate these muscles through facial nerve repairs. The insertions of these muscles should be harvested with the alloflap and fixed to the recipient skeleton, as stated by Furnas[10] and defended by Siemionow et al.[9] The nerve repairs could be performed at the trunk level or at peripheral branches, depending on the individual injury.

Flap design in the periorbital, nasal, and perioral regions should be individualized to match the injury. In the periorbital region, full-thickness loss of the eyelids with a functional eyeball is a very unlikely, although possible, situation. In these patients, the full periorbital complex should be transplanted, including the eyelids, canthal ligaments, and tear-draining system. All of these structures, along with the insertion of the levator palpebrae superioris muscle, should be repaired. This is the most critical area of full human face/scalp alloflap transplantation, in terms of achieving nearly normal function and making possible alternative strategies should the transplant fail. Timely protection of the eyeball remains an unsolved problem if rejection leads to flap necrosis. In the nasal area, inclusion of all layers of the nasal pyramid seems the most likely scenario, and thus, the superficial dissection suggested by Siemionow et al.[8] seems pointless.

Scalp dissection should be carried out in the subgaleal plane from the occipital region to reach the frontal area. There is no reported case of frontal necrosis in face/scalp replantations, and therefore we would not consider repairing the supratrochlear or supraorbital vessels. We hesitate to include the occipital artery in the flap to reduce the risk of occipital necrosis. This was a major problem in 50 percent of scalp replantations reported in a recently published series[11] and in other articles,[12] although it was probably related to incomplete pressure relief in the postoperative period. Including the occipital artery in the full human

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face/scalp alloflap would make the dissection take longer and is probably not necessary. Necrosis of ears included in replanted scalps has also been reported. Including the posterior auricular artery could make sense, although there is no conclusive clinical or anatomical evidence to support this.[11]

Taking all of the above considerations into account, probably the most frequent clinical scenario for a full human face/scalp alloflap would be a subperiosteal composite functional alloflap. This design would be the fastest and safest, although concerns about lifeboat strategies continue to be a major topic.

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Endoscopic Forehead Rejuvenation: I. Limitations, Flaws, and Rewards

Sir:

In two important reviews, Dr. Guyuron shares his vast endoscopic brow lift experience.[1,2] In part I, he describes technique refinement, limitations, and flaws. He beautifully illustrates and discusses ways to minimize unfavorable results, such as inadequate muscle removal, dimpling, depressions, asymmetry, excessive eyebrow separation, brow elevation errors, and displeasing eyebrow arch. He correctly emphasizes that evaluation of animation photographs should now become the standard method of documentation of successful ablation of central brow depressors. After accumulating an enormous experience (372 cases), he has demonstrated effective and good long-term results.

Unfortunately, what has been omitted is a thorough discussion of his learning curve, which began 14 years ago, leading up to his present concepts and results. It would be interesting to have data on the number of revisions over this period, the number of patients requiring botulinum toxin type A postoperatively, and specific numbers on complications, such as hematoma/nerve injuries. It would also be interesting to know of the long-term survival of the glabellar fat graft Dr. Guyuron believes is so important. That suction drains are left in place for 2 days and that steroids are given suggest that there is significant postoperative edema and ecchymosis. In fact, the operative procedure described by Dr. Guyuron almost seems more invasive than the standard open technique and the limited procedures described by Knize.[3]

In part II, Dr. Guyuron addresses his long-term results and some complications.[2] A 7-year retrospective study of 100 of 202 post–endoscopic brow lift patients examined brow position and patient satisfaction. We must assume that those patients who were not enrolled in the study were omitted because they were slow to respond and not because of patient dissatisfaction. As the authors have pointed out, this is another limitation of a retrospective study. The results demonstrate statistically significant brow elevation. According to their reports, both patient and surgeon satisfaction were extremely high. In our study, 70 percent of the post–endoscopic brow lift patients were satisfied; however, only 50 percent of the plastic surgeons ($n = 21$) surveyed were satisfied.[4] The complications rates in this study are similar to those of previous reports. What is

most unsettling is the high percentage of patients with persistent paresthesias (50 percent) due to iatrogenic injury to the first division of CN III, despite well-described anatomical studies of this region.[5] Other known sequelae of sensory nerve injury include dysesthesia and neuroma formation.[6,7] Perhaps the endoscopic instruments avulse the corrugator muscles from the scalp incisions, or perhaps there is little regard for the supratrochlear nerves that run through that muscle, resulting in central forehead/scalp dysesthesia. Any patient considering endoscopic brow lift as a surgical procedure should be informed about this highly probable postoperative complication. Plastic surgeons (Drs. Glenn W. Jelks and David M. Knize) who routinely work in the periorbital region and remove glabellar muscles via transblepharoplasty incision report a much lower incidence of this complication (<5 percent).[8] Direct nerve visualization with loupe magnification and fine plastic surgical instruments utility in the glabella region is less traumatic. Jelks leaves the medial corrugator muscle through which these nerves run, and Knize dissects the nerves out to preserve them before removing the medial muscle.

In comparing the preoperative and postoperative photographs in part II of Dr. Guyuron's article,[2] one is initially impressed. However, on closer inspection there are some inherent flaws, as follows:

Figure 1: In the postoperative views, the patient's eyebrows are penciled (colored) and shaped, and she is wearing eye makeup.
Figure 2: In our opinion, the patient did not need a brow lift, and the postoperative result has a slight stare. There is also makeup on the patient's eyes.
Figure 3: In the postoperative views, the patient's eyebrows have been plucked and reshaped, and she is wearing eye makeup.
Figure 4: In the postoperative views, the patient appears to have had an upper blepharoplasty that caused hollowing, and her eyebrows have been plucked and reshaped.
Figure 5: In our opinion, this is another patient who did not need a brow lift. Simply plucking her eyebrows (which has been done) would have given a more natural result.

Finally, brow lifting enthusiasm has peaked and resulted in numerous cases of patients with an "unnatural, surprised look" from excessive brow elevation. As Dr. Val Lambros has stated, "Plastic surgeons tend to lift the eyebrows much more than they have fallen." Today, according to Dr. Michael J. Yaremchuk, the majority of these unsatisfied patients seeking or undergoing "brow reversal" had previous endoscopic brow lifts.[8]

Procedures such as the endoscopic brow lift can be presented to patients as "scarless" or "with minimal scarring," which has immediate appeal and significant marketing value over traditional surgical techniques. Since its description in 1992, the endoscopic brow lift has undergone growing pains, with numerous options for fixation. Many unanswered questions remain for the technique and the plastic surgeon. For the endoscopic brow lift, how long is the learning curve, how consistent are the results, and what is the frequency of revisions? For a plastic surgeon with a finite number of practicing years, how long should the learning curve be with a given procedure before abandoning it if many of the results are unsatisfactory? Is the learning curve worth the reward? With these new studies, surgeons will have greater knowledge to decide to either "rediscover" the endoscopic brow lift or opt for other brow lift techniques. Of great interest are the recent statistics from the American Society for Aesthetic Plastic Surgery comparing surgical cases from 2004 and 2005. The number of brow lifts performed decreased 25 percent in 1 year!

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Reply

Sir:

I am grateful to Drs. Chiu and Baker for their initial laudatory remarks regarding the two articles published

in the April 1, 2006, issue of *Plastic and Reconstructive Surgery*. The first of the two articles on endoscopic forehead rejuvenation reviews the technique and my refinements[1]; the second, written with Dr. Ramin A. Behmand, reports a scientific analysis of actual long-term follow-up results and survey data from 100 patients.[2] Drs. Chiu and Baker's positive introductory comments are followed by several criticisms and questions. I feel compelled to address these issues, the basis of which is a study that does not actually measure the effectiveness of endoscopic forehead rejuvenation by patient examination but rather reports the opinions of 21 New York surgeons about the procedure. The dissimilarities between Drs. Chiu and Baker's study and ours, with respect to the design and methods, forbid meaningful direct comparison of our disparate conclusions. I will now address each of their criticisms and questions.

First, Drs. Chiu and Baker request a detailed discussion of my learning curve, "which began 14 years ago." Endoscopic forehead rejuvenation has a 14-year published history. As I started performing the procedure when it was introduced, my learning curve mirrors, to some extent, the procedure's development. As such, my concepts and techniques are based on considerable clinical experience, mostly positive but some negative. The refinements that characterize my present technique, as published in the first of the two articles, represent the current culmination of that experience. Those refinements are my adaptation of the procedure as a response to my results and the results of others that dissatisfied me during my learning curve and during the course of the procedure's development. Thus, my first article is a *de facto* account of my "14-year" learning curve. As to the learning curve for the endoscopic forehead procedure in general, this is an individual matter. Now that other authors and I have elaborated on the techniques that have produced consistent and pleasing results, the learning curve is far shorter than "14 years" and is reasonable, considering the effectiveness of the technique.

Drs. Chiu and Baker state their interest in the number of complications, the need for postoperative botulinum toxin type A injections, and the rate of hematoma/nerve injury in my series. There were no hematomas, and the rate of nerve-related morbidity is well described in the article. Regarding complications, the cases included in the first article for demonstration of the endoscopic forehead rejuvenation flaws were selected after careful review of numerous patient photographs. These were included to give a candid portrayal of imperfections. These suboptimal results were generally unnoticed by the patient but, when carefully scrutinized by the surgeon, served as the impetus for the development of modifications and refinements. None of these patients elected to undergo revision surgery, nor did these patients request injection of botulinum toxin type A. Only the one patient who experienced detachment of fixation on one side, as mentioned in the article, underwent revision surgery.

Drs. Chiu and Baker also inquire about the survival of the glabellar fat grafts. As mentioned above, we have had only the rarest occasion to reoperate on endoscopic forehead rejuvenation patients. When reoperation was required, it was in the short term, and thus, we cannot directly comment on the long-term survival of endoscopically placed fat grafts. However, long-term external examination suggests adequate survival to maintain normal glabellar contours. This is well documented in the second article. It should be noted that we learned that overcorrection of fat graft placement in anticipation of subsequent resorption is not necessary. Furthermore, secondary surgery to correct age-related changes many years subsequent to primary open forehead surgery has revealed survival of the fat graft in every patient. The actual percentage of fat survival could not be judged.

As to the use of corticosteroids and drains, since a dead space is created by pulling the scalp laterally due to skin redundancy, we felt it justifiable to use the drain, regardless of whether it is an open or closed technique. Also, whenever the periosteum is elevated, there is a somewhat higher chance of facial edema, which can be mitigated by corticosteroids. Drs. Chiu and Baker's deduction that employing means to reduce seroma and edema implies an overly invasive procedure is unwarranted. Endoscopic forehead rejuvenation includes precise dissection, tissue release, and reorientation to produce a pleasing aesthetic result without a bicoronal incision. As the endoscopic procedure significantly reduces the length of the access incision, it is inherently less invasive than some of the alternatives.

Whether or not the patients who did not return for a follow-up were satisfied is unknown to us, due to the retrospective nature of our study. Some enrollment difficulties, such as relocation with no forwarding address, rendered re-examination impossible or impractical for some patients. Thus, I appreciate Drs. Chiu and Baker graciously providing me the benefit of the doubt by assuming that the nearly 50 percent enrollment was the result of intrinsic difficulties in retrospective studies that require a long-term call back solely for the purpose of research and "not because of patient dissatisfaction."

As mentioned in the introduction, Drs. Chiu and Baker offer several opinions about the effectiveness and overall quality of endoscopic forehead rejuvenation in their letter based on their previously published study.[3] However, by their own admission, Drs. Chiu and Baker's study is not designed or implemented to actually gauge the effectiveness of the procedure. Their study reports that the frequency of this procedure in one hospital in New York City decreased in recent years, and it uses surgeon survey results to insinuate that the major reasons for the diminution are surgeon dissatisfaction and a narrowing of indications. In our retrospective study, we brought the patients back, measured the changes, and asked them to complete a questionnaire. Some of the long-term follow-up examinations occurred nearly 8 years after surgery. Meanwhile, the 70 percent patient and 50 percent physician satisfaction

scores reported in Drs. Chiu and Baker's article are speculative. The recall memory and perception of 21 surgeons summarized in a brief questionnaire with no linkage to chart reviews for results, patient surveys, and patient examinations has little bearing on the findings and conclusion of our objective, evidence-based study demonstrating the long-term effectiveness of endoscopic forehead rejuvenation.

Also, the answers to questions such as "What complications have you observed after the endoscopic brow lift procedure?", "Have you administered botulinum toxin type A injections within 6 months of the endoscopic brow lift procedure?", and "What percentage of these patients are injected with botulinum toxin type A?" are highly unreliable, because no patients were examined and no charts were reviewed. These and other questions are leading in nature, especially when considered in composite. They are not balanced by another important, yet notably absent, line of questions that would have permitted respondents to provide comments regarding their positive experience with endoscopic forehead rejuvenation.

The incidence of hypoesthesia and paresthesia has been similar in our practice, regardless of the technique. This, in all likelihood, has to do with the magnitude of muscle removal and manipulation of the nerves rather than the choice of exposure. The reality is that our report was carefully conducted with an examination of the patient, which is very different from anecdotal reports of a lower incidence of nerve-related complications. Moreover, the incidence of permanent paresthesia was extremely low in our study and even lower than that reported by other surgeons. We were unable to find a study that documents the degree of hyposthesia or anesthesia based on careful patient examination and documentation similar to that of our study. Furthermore, any minor complication or flaw should be viewed in the context of a favorable 89 percent overall patient satisfaction rating.

Regarding the role of eyebrow elevations brought up by Drs. Chiu and Baker, while I do agree with Dr. Val Lambros that deflation plays a role in the eyebrow position, it is not the sole reason for senescent changes.

I am sure that most of the patients who are undergoing reversal of endoscopic forehead rejuvenation had that procedure performed many years ago, at a time when surgeons were performing it with overzealous enthusiasm and trying to prove that the technique works. Today, most surgeons who have sufficient experience produce gratifying results after endoscopic forehead rejuvenation.

With regard to Drs. Chiu and Baker's comments regarding the association between a staring look and endoscopic forehead rejuvenation, it is not unlike the open technique, which has resulted in the same look for decades and, unfortunately, can still be seen today. Many of the results in published reports cannot be justifiably assessed, since essentially none include the animated views. Moreover, in most reports, the forehead is covered with hair, concealing highly undesirable elongation of the forehead. It is this displeasing outcome of coronal forehead lifts that prompted me to design and publish our experiences with the pretrichial hairline incision, which today I reserve for patients with a long forehead.[4]

In their article, Drs. Chiu and Baker demonstrate a reduction in the rate of endoscopic forehead rejuvenation procedures over a 7-year period. They provide a statistical context for that decrease by showing that the rate of coronal brow lifts and other plastic surgery procedures did not change significantly over the same period. While there is overlap, the indications for coronal brow lift and endoscopic forehead procedures also have differences. Thus, potential alternatives to these procedures, such as botulinum toxin type A injection, may affect the rates of implementation of each in different ways. The use of botulinum toxin to treat dynamic glabellar rhytides and raise the lateral brow gained popularity over the same period that endoscopic forehead rejuvenation experienced a decline in use at Manhattan Eye, Ear, and Throat Hospital. It may be the fact that the less invasive nature of botulinum toxin attracted patients and surgeons who would otherwise have chosen the endoscopic procedure when indicated. If this population shift did in fact occur, it does not demonstrate that the endoscopic procedure is regarded as ineffective or flawed, only that a subset of patients and surgeons opted for something else and likely for a variety of reasons.

Finally, Drs. Chiu and Baker found fault with our results. They were critical of the fact that the patient in Figure 1 of our second article reinforced her eyebrow and used eye makeup in the postoperative photograph. Those who report long-term results realize how difficult it is to persuade patients to return for a follow-up. Insisting that they remove their makeup is not realistic. Even more unrealistic would be to ask patients not to pluck or reinforce their eyebrows. The patient in Figure 1 has eyebrows that have been reinforced, not redrawn. Regarding the patient in Figure 2, one has to note that the asymmetry has been corrected, frown lines have been eliminated, and there is essentially no change in the lid crease. I fail to see the "stare" that they have referred to, as the iris is still overlapped by the upper eyelid by at least 1 and perhaps 2 mm (optimal). With regard to the patient in Figure 3, the elimination of frown lines and the improvement in the eyebrow position have no relevance to the plucking of eyebrows. Drs. Chiu and Baker's assumption about the patient in Figure 4 is incorrect. This patient did not have a blepharoplasty. What they have perceived to be a hollowing of the upper eyelid is merely an increase in tarsal show as a result of eyelid ptosis that this patient gradually developed over the long follow-up period. This was not pointed out in the article because it seemed obvious. For the patient in Figure 5, Drs. Chiu and Baker, again, failed to note that there was significant hooding over the lateral eyebrow preoperatively that rendered the supratarsal crease suboptimal; this was eliminated postoperatively. Plucking the eyebrow would not have changed this. In addition,

they failed to detect the eyelid ptosis that compelled the patient to elevate the eyebrows as compensation and resulted in the appearance of overcorrection, as evidenced by a horizontal forehead crease.

On the one hand, Drs. Chiu and Baker find fault with my results and state that the eyebrows are overcorrected, while on the other hand, they are of the opinion that this operation does not work. Is this not a contradictory statement?

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The Importance of the Retaining Ligamentous Attachments of the Forehead for Selective Eyebrow Reshaping and Forehead Rejuvenation

Sir:

I read with great interest the article by Sullivan et al. entitled "The Importance of the Retaining Ligamentous Attachments of the Forehead for Selective Eyebrow Reshaping and Forehead Rejuvenation," published in the January 2006 issue of *Plastic and Reconstructive Surgery*.[1] I very much appreciate this contribution to our knowledge of forehead anatomy, but I must disagree with the authors' conclusion that the ligaments they describe actually retain the eyebrows. Hence, I must disagree with authors' clinical application of their anatomic findings.

The authors nicely demonstrated three specific soft-tissue structures that attach bone just above or at the superior orbital rims to the overlying periosteum near the level of the medial end of the eyebrow on each side. Two superior attachments were found approximately 1 cm above the orbital rim, and one inferior attachment was found near the orbital rim. They further demonstrated these attachments extending through the periosteum to the adjacent layer of the multilayered deep galea plane (Fig. 3 of the authors' article). These attachments were described as extending into the "area of the frontalis muscle," but this was not demonstrated anatomically. Unequivocal evidence of the existence of the entire retaining ligamentous system is crucial to the support of their conclusions. In fact, these described attachments would not be expected to extend to the frontalis muscle in this area, because they are separated from this muscle by the glide plane space (Fig. 1). The glide plane space is a cleft in the deep galea plane that overlies the lower 2.0 cm of the

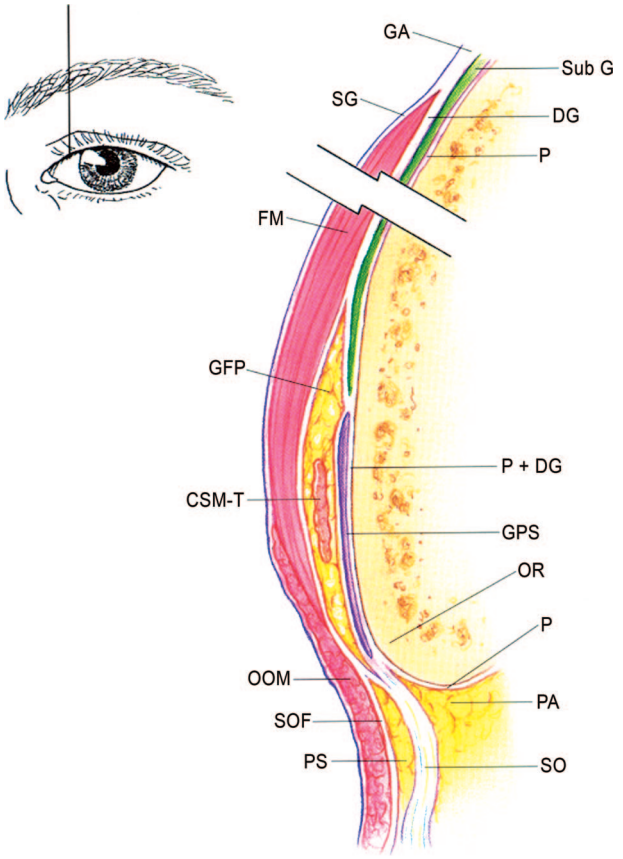

Fig. 1. Over the calvaria, the galea aproneurotica (*GA*) lies on the subgalea fascia plane (*Sub G*), which separates the galea aproneurotica from periosteum (*P*). As the galea aproneurotica approaches the upper forehead, it splits into the superficial galea plane (*SG*) and the deep galea plane (*DG*) to envelop the frontalis muscle (*FM*). The deep galea plane splits again to envelop the galea fat pad (*GFP*), which, at the level indicated in this illustration, contains the transverse head of the corrugator supercilii muscle (*CSM-T*). Over the lower forehead, the deep galea plane splits a third time to form the glide plane space (*GPS*), a space deep to the corrugator supercilii muscle that contains only loose areolar tissue. Under the glide plane space "floor," the subgalea fascia plane, which separated the deep galea plane from periosteum over the upper forehead, is obliterated. The floor of the glide plane space is the deepest layer of the deep galea plane fused with periosteum (*P + DG*) and is fixed to the underlying frontal bone. The multiple layers of the deep galea plane rejoin and fuse to the orbital rim (*OR*) before they enter the orbit to form the suborbicularis oculi muscle fascia (*SOF*) and the septum orbitale (*SO*). The preseptal fat pad (*PS*) lies superficial to the septum orbitale, and the preaponeurotic fat pad (*PA*) lies deep to it. The superficial galea plane that covers the surface of the frontalis muscle continues over the surface of the orbicularis oculi muscle (*OOM*). (Modified from: Knize, D. M. *The Forehead and Temporal Fossa: Anatomy and Technique*. Philadelphia: Williams & Wilkins, 2001. P. 46, Fig. 4.1.)

forehead and extends from over the origin of the corrugator muscle at the superior-medial orbital rim to approximately the level of the junction of the middle and lateral thirds of the width of the frontal bone on each side. The floor of the glide plane space is formed by the deepest layer of the galea, which is fused with periosteum and firmly fixed to bone the over this area. This space exists to allow the overlying corrugator supercilii muscle enveloped in the galea fat pad to freely move the eyebrow medially. The soft tissues superficial to the roof of this space, including the frontalis muscle, move freely, because there are no soft-tissue attachments to bone that traverse this space. Each frontalis muscle slides freely over the galeal fat pad that overlies the glide plane space, and each lower frontalis muscle has an active and passive range of motion of at least 1 cm at the level of the medial end of the eyebrow. For this reason, I question the authors' suggestion that their described attachments extend up into frontalis muscle or through the orbicularis oculi muscle to the dermis under the eyebrow. A simple observation supports my position. While looking in a mirror, push your medial eyebrow cephalad. You will see that it easily moves to a level of aesthetically exaggerated elevation. If there are retaining ligaments in this area, preserving them would not appear to prevent the medial eyebrow from excessive elevation, as suggested in this article. The multiple planes of the deep galea do refuse along the superior orbital rim to form the septum orbitale, and thus, there are some fascial connections to the layer of the deep galea under the frontalis muscle as shown in the figure, but these fascial connections do not clinically tether the medial frontalis muscle, the orbicularis oculi muscle, or the medial eyebrow.

In Figure 5 of the article, cephalad traction was placed on a forehead flap that had been elevated at the "supraperiosteal" or subgaleal level with release of all the galeal attachments to bone. The medial eyebrows were shown to move cephalad, and this was attributed to release of the described "ligamentous" attachments. It is true that the medial eyebrows did move cephalad, but they did not move cephalad as a direct consequence of the release of these attachments. These described attachments do tether the deepest layers of the deep galea plane to bone, but there is no evidence that they tether the dermis under the eyebrows to bone. The soft-tissue release shown in Figure 5 allowed the entire galea plane to be moved cephalad. Since the frontalis muscle originates from the galea, the frontalis muscle moved cephalad. Because the eyebrows are suspended from the lower edge of the frontalis muscle, the medial eyebrows moved cephalad along with the forehead flap. I believe that release of the authors' described attachments simply freed the galea to move and the attached frontalis muscle pulled the medial eyebrows up with it.

I very much appreciate the authors' efforts to clarify the soft-tissue anatomy along the superior orbital rim, but I disagree with their clinical application of their work based on their conclusion that the described attachments act as retaining ligaments for the medial eyebrows. I am convinced from my own anatomic studies that their described attachments are a part of a "retaining" mechanism for adhering galea to bone, while the frontalis muscle and the overlying medial eyebrow are not "retained" by these attachments. Frontalis muscle tone determines the medial eyebrow level. Clinically, the medial eyebrow becomes overelevated in some patients after they have undergone foreheadplasty, not because the authors' retaining ligaments are transected but simply because the surgeon elected to transpose the central part of the forehead flap and its contained galea cephalad, pulling the frontalis muscle and the medial eyebrow up with it. I continue to believe that the most important measure a surgeon should take to avoid the "startled look" of an overelevated medial eyebrow is to refrain from transposing the medial portion of the forehead flap in all cases except those with marked medial eyebrow ptosis. I have totally released galea from bone in hundreds of cases now using the limited incision foreheadplasty procedure.[2] That release would have necessarily included the authors' retaining ligaments. I have yet to see the medial eyebrow rise as a result of it, because only that part of the galea plane over the lateral frontal bone on each side is transposed cephalad with that procedure. Neither the galea above the medial eyebrows nor the attached medial frontalis muscle is moved.

This is a case of two observers looking at the same anatomy and visualizing quite different systems for the living subject. Since future surgeons may base the design of their procedures on published anatomy, it is important for the sake of their patients how these anatomic details are clarified. If there are retaining ligaments that extend to and tether the frontalis muscle and/or the dermis under the medial eyebrow, this must be clearly demonstrated anatomically before any procedure is designed based on this concept.

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Is the Silicone Implant Suspender Superior to or the Equivalent of the Autogenous Fascia Graft?

Sir:

We recently read an article in the *Journal* by Friedhofer et al. entitled "Correction of Blepharophimosis with Silicone Implant Suspensor" (*Plast. Reconstr. Surg.* 117: 1428, 2006). The authors advocated the superiority of the silicone implant suspender versus the autogenous fascia graft for correction of upper eyelid ptosis in blepharophimosis. We wondered why they

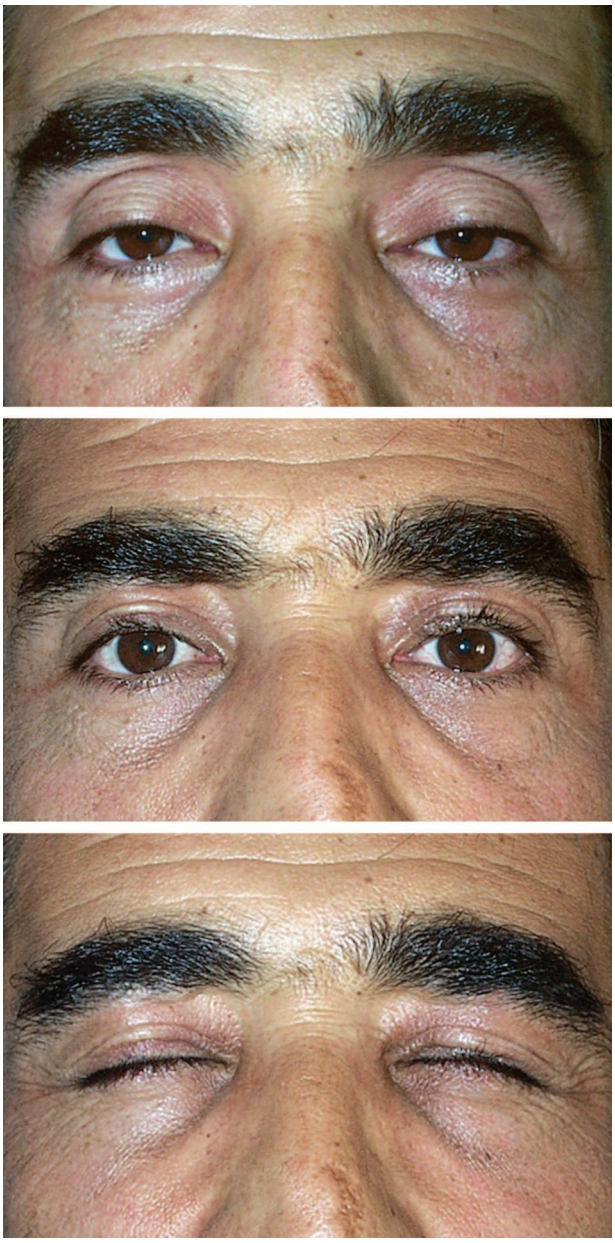

Fig. 1. (*Above*) Bilateral congenital ptosis. (*Center*) Postoperative view. (*Below*) Natural closure with no force on the orbicularis muscle.

preferred the silicone implant suspender rather than the autogenous fascia graft. Is it because the silicone implant suspender is superior to or the equivalent of the autogenous fascia graft? The answer should be no. They stated that they preferred silicone implant suspender because the technique is simpler, the operative time is shorter, and there is no potential donor-site morbidity, such as a second operative field, scarring, a long recovery period, muscular hernia, or potential infection. In addition, they claimed that fascia grafts do not have sufficient elasticity and allow palpedral occlusion. We will discuss all of the reasons they used to support the use of the silicone implant in the repair of blepharoptosis. Their technique is not a simpler, conventional fascia sling technique.[1] In contrast to the authors' suggestions, serious complications, such as exposure and migration of the silicone implant into the eyelid, have been observed.[2] In addition, high infection rates have been with the use of silicone for ptosis repair.[3] The deep temporal fascia has several advantages over the other treatment methods. Its location allows for easy exposure, and it can be harvested while protecting surrounding structures.[4] The entire fascia can be harvested over the muscle. It is thin and pliable, so it is easily handled and placed at the upper eyelid. Furthermore, it is a durable material. In addition, the deep temporal fascia can be harvested from the same operative field in ptosis surgery. The scar, which is a significant problem for alternative autogenous materials, can be easily hidden by hair. However, their figures are not focused or clear enough, and their technique leads to remarkable scars which can easily be observed above the eyebrows in Figures 5 and 6. The effectiveness of their technique is also doubtful, because blepharoptosis is still evident in Figures 6 and 7. We do not agree with the authors' statement that the autogenous fascia lacks sufficient elasticity and doesn't allow palpedral occlusion. The deep temporal fascia has sufficient elasticity, and the patient can close his or her eyelids without a problem (Fig. 1).

Finally, surgeons who use the silicone implant for blepharoptosis should consider the growth potential of children. Adjustment of the silicone implant cannot adapt to the growth potential of children. In contrast to silicone, the autogenous tissue graft has growth potential.

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Reply

Sir:

We appreciate the opportunity to answer the comments of our colleagues. The silicone suspender developed by us in 1989 and published only in 1999[1] simplifies the techniques that use an autologous fascia graft from either the lateral thigh or the temporal region. Even though we consider the frontalis suspension with autologous fascia an excellent method, we disagree with Dr. Tellioglu et al. on several points.

Avoiding a second incision to harvest the fascial graft shortens the operative time and the morbidity of the procedure. The implant is sterile and ready for use, and the simplicity and quickness of its insertion, allowing adjustments at any time, make this method simpler than those that use autologous fascia.

Serious complications can occur if the surgeon does not take the minimal care to perform a safe operation using silicone, which is normally well tolerated by the human body.

Exposure and migration of the implant occurred when the silicone was used in a strip pattern, because the adhesion to the tarsus was weak, leading to reappearance of the ptosis and separation from the tarsal plate. Moreover, the capsule created around it was very thin. All these factors led us to develop the silicone implant, which provides greater safety because of the horizontal, Dacron-reinforced silicone plate. The plate measures 13 × 3.5 mm and has three 1-mm holes that provide adequate fixation to the tarsal plate.

This Dacron-silicone plate leads to a more extensive capsule formation, which facilitates its adhesion. The silicone should be inserted in the submuscular plane to be protected. This detail was enough to avoid extrusion and infection.[2] We did not have any cases of infection in our series.

Our article was not about cases of pure severe ptosis but about treatment of the complex blepharophimosis syndrome, which is frequently accompanied by difficult anomalies such as telecanthus, epicanthus inversus, and shortening of the palpebral fissure, among others. These anatomical alterations make the correction difficult and provide limited aesthetic results, according to the intensity of the syndrome. Consequently, the correction of a pure ptosis cannot be compared with the correction of the ptosis of a patient with blepharophimosis.

Our follow-up was of more than 12 years, and until now, no child has needed reoperation because of growth. Only two patients were reoperated on, but that was because of hypocorrection due to an intense expression of the syndrome. In children with less dense eyebrows, the scar can be temporarily visible, but as the child grows, the scar becomes hidden, as shown in our Figure 7. These incisions are the same as those used in the autologous fascia techniques.

In the case exemplified by Dr. Tellioglu et al., the patient had moderate, not severe, ptosis. The presence of an elevated palpebral fold leads to the suspicion of a congenital disinsertion of the levator aponeurosis, which is usually treated by reinsertion, without the need for an autologous fascia graft.

In conclusion, we understand that medicine is a science of ephemeral truths. Universally accepted concepts should be regularly reviewed and discussed in an impartial way until we can reach a conclusion that will certainly be temporary.

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Le Fort III Osteotomy or Distraction Osteogenesis Imperfecta

Sir:

The Le Fort III osteotomy is an important procedure for craniofacial surgeons; it is used to improve obstructive sleep apnea and eliminate tracheostomies, and it can significantly normalize the facial appearance in children with craniofacial dysostosis. Since the introduction of distraction techniques, surgeons are now faced with a choice in advancing the midface. Dr. Phillips et al. recently reported their series of 14 traditional Le Fort III advancements.[1] Based on an analysis of their patients and a comparison to previously published studies, they concluded there was "no significant improvement in results using the Le Fort III distraction osteogenesis when compared with traditional methods."[1] I have considerable admiration for the authors' well-respected unit in Toronto, but I believe that their conclusions were not supported by the data that they presented. The authors did not prospectively randomize their patients into two treatment groups, but instead retrospectively compared their 14 patients to earlier published studies, including my report of halo distraction Le Fort III osteotomy, which described my initial experience with 10 patients.[2]

The authors performed 30 Le Fort III osteotomies over a 7-year period at their busy unit, but only 14 cases had reliable postoperative follow-up. It is well known that patients who are unsatisfied with their outcomes may choose not return to the doctor who performed their procedure. When patients do not have a good result, surgeons may choose not to order follow-up evaluations, recognizing that no measurable gain was achieved. I do not know whether either of the aforementioned scenarios occurred in the authors' series, but when more than 50 percent of the patients do not have appropriate follow-up, the issue of potential bias must be addressed. In my opinion, the only result that is shown photographically in their article did not have sufficient advancement of the maxilla: the face is shown in repose preoperatively and smiling postoperatively, which raises the cheek mass, giving the spurious impression of a greater advancement. I also believe that if the authors wanted to compare their series of traditional Le Fort III osteotomies (with which their senior author has considerable experience) to my initial series of halo distraction patients, who had undergone a nascent technique that was still in the process of development, they should have commented on the bias introduced by this experience differential.

I am not sure whether Dr. Phillips and his co-authors have any personal familiarity with halo distraction, but I currently perform both procedures (traditional and halo distraction Le Fort III osteotomy). In September of 2005, at the International Society of Craniofacial Surgeons biannual meeting in Coolum, Australia, I had the opportunity to present an expanded long-term outcome assessment of my own series of Le Fort III halo distraction patients, previously published in May of 2005.[3] This review included 77 Le Fort III osteotomies performed over a 7-year period, 56 of which were by halo distraction. In the second half of this series, the average skin-to-skin surgical time was 3.5 hours, and the average hospital length of stay was just over 3 days. I believe that if the authors could have compared their Toronto experience (average surgical time, 8 hours; average hospital length of stay, 11 days) to these data, they might have reached a different conclusion. I recognize that it is possible to obtain significant advances in older patients with a traditional Le Fort III; however, I am not able to achieve the degree of overcorrection that I believe is important in younger children using a traditional method. With distraction, not only do surgeons have the freedom to position the maxilla as far forward as desired, but they can also adjust positioning after leaving the operating room. With a traditional technique, the advancement is limited not only by the skin envelope but also by the stability achievable through the use of plating systems and/or intermaxillary fixation. I recognize that I ask a lot of my patients when I apply an external halo, but those who have previously undergone a traditional Le Fort III that included intermaxillary fixation (as reported in the Toronto experience) have unanimously preferred the external halo to having their jaws wired together. Although I have been fortunate to not have any significant pin-related complications, I do agree with the authors that the use of a pin-based halo system can present potential complications. However, halo distraction does not require the use of bone grafts, thus eliminating a craniotomy (or other bone graft harvest) and not only shortening the length of surgery but also reducing the potential risks of surgery. While removal of the halo requires a brief 15-minute anesthetic, many surgeons also use a second anesthetic to remove intermaxillary fixation.

I believe that the authors' use of the term "distraction osteogenesis imperfecta" may well be appropriate; it is not a perfect technique, but the derogatory "imperfecta" is equally applicable to the standard Le Fort III. On the basis of my personal experience in growing children, I choose the halo distraction technique because I am convinced that it provides results that are clearly superior to those of the traditional technique. The operation is smaller and simpler and has a very high success rate and a lower complication rate. While I do not think that the authors' data supported their conclusion, I do commend them for reviewing their significant series, which exemplified the good results achievable with any procedure at an experienced center.

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Anatomical Localization of the Umbilicus: A Statistical Analysis

Sir:

We have read the article[1] by Abhyankar et al. entitled "Anatomical Localization of the Umbilicus: An Indian Study" (*Plast. Reconstr. Surg.* 117: 1153, 2006). The authors describe the anatomical position of the umbilicus in 75 young healthy females, assessing the position using four measures: the distance between the xiphisternum and the umbilicus, the distance between the umbilicus and the pubis symphysis, the distance between the anterior superior iliac spine and the umbilicus, and the inter–anterior superior iliac spine distance. The authors observed that the umbilicus is situated around the midline plane such that the ratio of the distance between the xiphisternum and the umbilicus and the distance between the pubic symphysis and the umbilicus is 1.6:1; also, the ratio of the distance between the umbilicus and the anterior superior iliac spine and the inter–anterior superior iliac spine is approximately 0.6:1. Furthermore, the

authors kindly provide the values of all 75 patients involved in the study.

We analyzed the data provided by the authors with more advanced statistical methods (multiple linear regression with likelihood ratio test[2] for data reduction and simplicity of the equation; STATA 9.0, Statasoft Corp., College Station, Texas) and found that the distance between the xiphisternum and the umbilicus can be very accurately predicted by the following equation:

$$X_u = -2.32 + 0.91X_p - 0.07H$$

($p < 0.0001$; $R^2 = 0.9320$; Pearson's correlation coefficient, 0.9654) where X_u is the distance between the xiphisternum and the umbilicus, X_p is the distance between the xiphisternum and the pubic symphysis, and H is the height, all measured in centimeters (Fig. 1).

We congratulate Dr. Abhyankar and colleagues for their simple and useful work. We believe that by adding this statistical analysis, the surgeon can be more confident in where to perform umbilical reimplantation.

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Immunohistochemical Differentiation and Localization Analysis of Sweat Glands in the Adult Human Axilla

Sir:

The May 2006 article entitled "Immunohistochemical Differentiation and Localization Analysis of Sweat Glands in the Adult Human Axilla" (*Plast. Reconstr. Surg.* 117: 2043, 2006) by Beer et al. contained important histological data about the localization and distribution of sweat glands in the axillary region and possible clinical consequences.[1] From our long-standing experience with surgical procedures for the treatment of focal axillary hyperhidrosis, we would like to complement this study with our own experience.

In their introduction, the authors distinguish among three classic surgical methods for the treatment of axillary hyperhidrosis. This classification of Bisbal et al., dating back to 1987, distinguishes the three types of surgery. Type I describes a subcutaneous tissue resection without skin excision; type II, the en bloc resection of subcutaneous fat and overlying skin; and type III, small skin excision of central axillary skin combined with subsequent open curettage of adjacent skin. Due to the introduction of a variety of novel minimally invasive therapy options, we believe that this classification is imprecise and insufficient. We propose dividing type I into types IA and IB, as techniques such as superficial liposuction and suction-curettage should be distinguished. Whereas superficial liposuction, introduced by Lillis and Coleman in 1990,[2] solely removes superficial subcutaneous fat without affecting dermal parts, the principle of suction-curettage combines removal of subcutaneous fat and deep dermal tissues. Hence type IA should be reserved for procedures that solely eliminate subcutaneous fat, and type IB for the combination of fat removal and additional curettage of skin parts—more precisely, deep dermal parts. This is supported by a recent gravimetrically controlled study showing that both methods differ significantly with regard to their

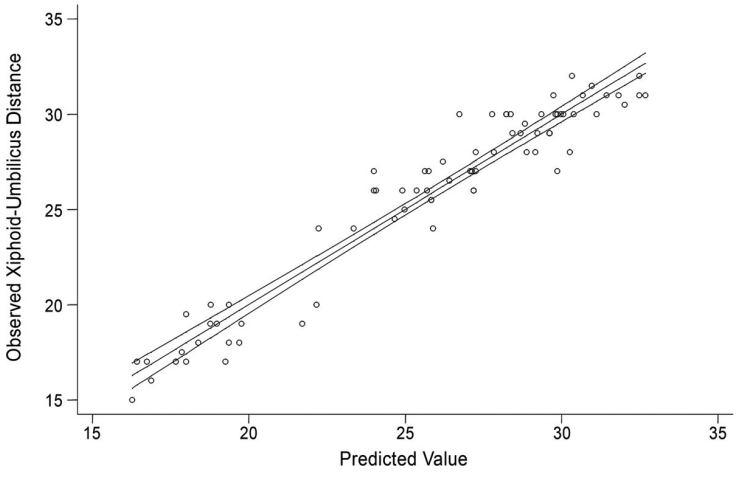

Fig. 1. Observed versus predicted xiphoid-umbilicus distance.

efficacy. Suction-curettage showed a significantly higher reduction of sweat rates.[3]

We agree entirely with the statement by Beer et al. that the results of their study favor the use of minimally invasive strategies, assuming these techniques are aggressively performed.[4] In our opinion, however, an exception for open surgery still remains and should be noted. It is a well-known problem that a nonresponse to minimally invasive strategies occurs in up to 19.3 percent of cases.[5] Recurrent sweating is often clinically visible as spot-like areas in the iodine starch test. In these cases, the small areas can easily be excised with subsequent primary closure with less effort than a complete secondary minimally invasive surgical procedure.

To summarize, the study by Beer et al. provides essential data for understanding the anatomy and distribution of axillary sweat glands in normhydrotic patients. Additional future data on sweat gland density and distribution and a comparison of preoperative and postoperative histological findings in patients with hyperhidrosis and bromhidrosis would be desirable.

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DISCLOSURE

All authors hereby disclose any commercial associations that might pose or create a conflict of interest with information presented in this communication.

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Reply

Sir:

I thank the Drs. Bechara et al.[1] for their valuable contribution to my recent study, "Immunohistochemical Differentiation and Localization Analysis of Sweat Glands in the Adult Human Axilla."[2] This study revealed that in Caucasians either all or the preponderant majority of eccrine, apocrine, and apoeccrine sweat glands are localized in the superficial subcutaneous tissue at the interface of the dermis/subcutaneous tissue and not in the dermis itself.

On this basis of this knowledge, we suggested optimizing Bisbal et al.'s[3] type I skin-sparing, minimally invasive surgery in hyperhydrotic/osmidrotic patients and abandoning the more radical type II and type III sweat gland excision including the overlying skin, thus avoiding a higher rate of complications and broad, unsightly scars.

Also, given this knowledge, it seems logical that all type I techniques must aim at efficiently removing the superficial subcutaneous tissue, including the sweat glands, up to the dermal border. It seems reasonable that the dermis itself can be left untouched.

With respect to efficacy, it has been shown repeatedly that by superficial blunt liposuction alone it is impossible to eradicate the sweat glands, as considerable force is needed to disrupt the glands from their ducts.[4] Even with sharper instruments, such as grinded liposuction cannulas or curettes,[5] and other tools, such as ultrasound,[6] it has been fastidious to remove the glands efficiently. It would prove even more difficult to remove parts of the dermis in the axilla using a minimally invasive technique without exerting undue force on the remaining skin, irrespective of which mechanical instrument is used. In contrast, Bechara et al.[1] state that with suction–curettage it is possible to remove sweat glands containing parts of the dermis in the axilla; therefore, they propose a division of type I surgery into two separate types, IA and IB. Type IA should be reserved for procedures eliminating only subcutaneous fat, and type IB should be reserved for the additional curettage of deep dermal parts. According to the sporadic occurrence of sweat glands in the dermis and the virtual infeasibility of removing dermal tissue mechanically using one or more tiny stab incisions, I do not believe that such an extended classification of type I surgery is warranted.

What we need, and are still lacking, are efficient tools to remove the sweat glands in their subdermal location. How this can be best achieved is explained in a report by Yoshikata et al.,[7] who claim that the subcutaneous tissue and the glands, including the subdermal vascular plexus and the hair follicles, have to be removed in the same manner that defatting is accomplished in preparing a full-thickness skin graft. Similar statements were made in 1977 by Rigg,[8] who postulated a true flap-to-graft conversion of the axillary skin.

Only the future will show which tool is best suited for the technique of converting the axillary skin from a flap into a full-thickness skin graft and thus removing all sweat glands without the extensive trauma to the skin of conventional mechanical instruments.[9]

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Bilateral Lumbar Hip Dermal Fat Rotation Flaps: A Novel Technique for Autologous Augmentation Gluteoplasty

Sir:

We read with interest the article by Raposo-Amaral et al. entitled "Bilateral Lumbar Hip Dermal Fat Rotation Flaps: A Novel Technique for Autologous Augmentation Gluteoplasty" (*Plast. Reconstr. Surg.* 117: 1781, 2006). The authors describe an innovative use of a dermal fat flap for buttock augmentation.

We agree with the authors that buttock implants have been described with various rates of complications and difficulties,[1] thereby hindering wide acceptance, and that lipografting is an effective means of moderately increasing buttock volume but does not directly address ptosis.[2] This makes a local dermal fat flap a good option. The ideal flap, in our opinion, should be versatile, not vascularly compromised, and give the maximum projection at the midlevel of the buttocks.

We had previously presented and published our experience with an alternative autologous buttock augmentation with a dermal fat flap in 20 patients.[3] We described a perforator-based dermal fat flap originating within the regularly excised supragluteal tissue rotated caudally to reach the inferior gluteal fold. By turning onto itself and extending into the inferior gluteal crease, it gives the buttocks maximal projection at the midlevel, allows for an ample and more caudal volumetric filling, and results in a natural and stable enhancement of this area (Fig. 1).

This flap has reliable circulation and requires minimal additional operating time. There have been no

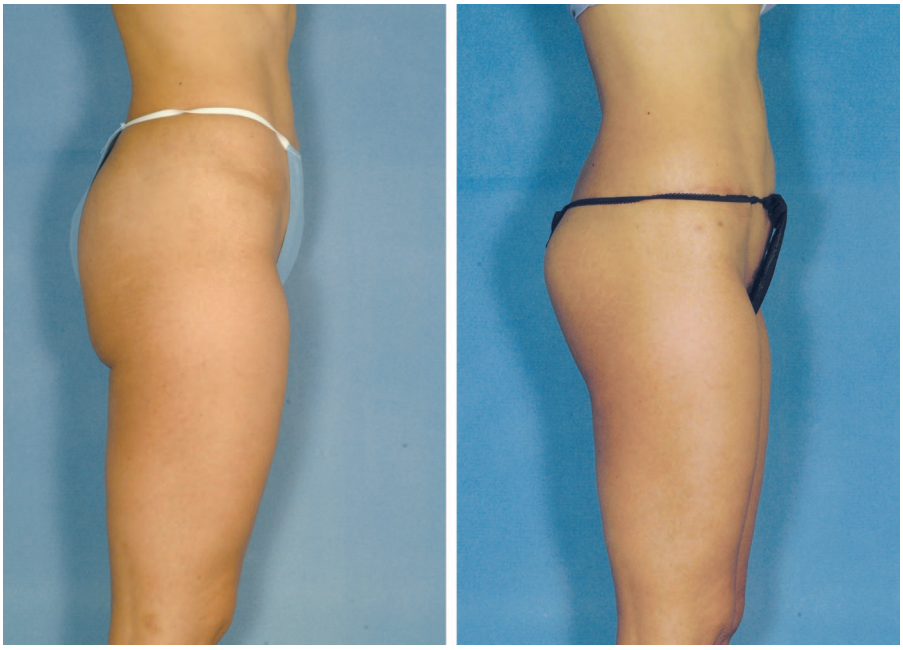

Fig. 1. Preoperative and postoperative photographs after autologous buttock augmentation with a dermal fat flap.

major complications, and the most common minor complication was delayed wound healing. It can be custom-designed for each patient, and more than one flap can be created if necessary.

We do not instruct patients to avoid the supine position or direct weight bearing with the buttocks. We have had only one patient who developed minimal induration, probably due to minimal fatty necrosis; the necrosis resolved over time, without compromising the augmentation or contour.

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Reply

VIDEO+

Sir:

We thank the authors for their interest in our approach to gluteal augmentation. We share their enthusiasm for autologous augmentation gluteoplasty, as well as their opinion regarding patient selection for the dermal fat flap for gluteal augmentation.

We favor superior pole gluteal augmentation rather than midlevel gluteal augmentation (Fig. 1). Our concern about the vascular supply of the dermal fat flap has driven our choice. Kankaya et al.[1] classified the gluteal region into three vascular zones based on an anatomic study. This group found that the superior gluteal zone combined 48.5 percent of perforators, whereas the central gluteal zone was the most poorly vascularized region. Because of these findings, we were initially concerned that the relative lack of gluteal perforators in the medial zone might jeopardize a dermal fat flap based on a pedicle from this zone, but it appears others have had success with this approach.[2,3]

The postoperative prone position is uncomfortable and can cause breathing problems. Early in our series, we maintained our patients in the prone position in order to avoid compression of the dermal fat flaps. However, due to respiratory risk and to the discomfort that this position produces, we have modified our approach. Whenever possible, we now place the patient in the lateral and supine position. To date, we have not observed additional postoperative complications due to this modification.

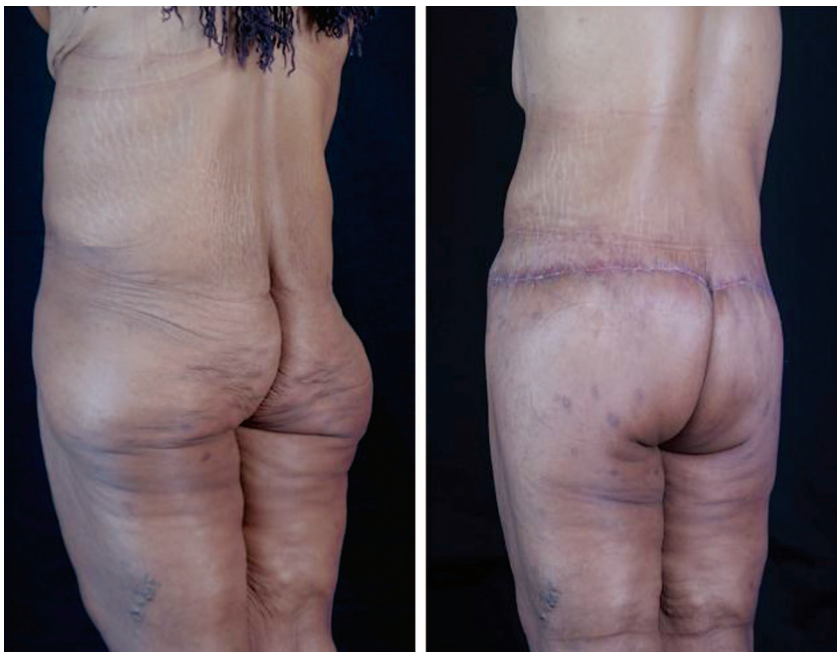

Fig. 1. (*Left*) Preoperative oblique view of a 52-year-old woman after weight loss of 170 kg. Note the enormous amount of lax skin in the gluteal region. (*Right*) The 3-month postoperative results exhibit the superior pole gluteal projection achieved by the operation.

We do believe that standardized photographs without underwear are important to objectively analyze surgical outcomes. In summary, a number of different techniques for dermal fat rotation flap gluteoplasty are available that allow us to achieve reliable gluteal projection in selected patients.

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Patient Safety in the Office-Based Setting

Sir:

We read with interest the CME article entitled "Patient Safety in the Office-Based Setting" (*Plast. Reconstr. Surg.* 117: 61e, 2006). The authors do a good job of discussing many aspects of patient safety in the outpatient setting. This is a topic about which everyone in medicine should be aware.

We would like to bring to the attention of the authors and others with an interest in patient safety that the misrepresentation of data on this issue has made it into our prestigious *Journal*, when previously it had remained in the journals of other specialties and in the media with sensationalized reporting. The quote "Office-based procedures comprise a 10-fold increase in risk for serious injury or death as compared with an ambulatory surgical facility" is a false claim. The article mentions that *U.S. News & World Report* in 2003[1] made this claim. These data originated from an *Archives of Surgery* study[2] that was flawed in many ways and has been shown to have used poor methodology with results of questionable value. The study compared adverse incidents and deaths in the office setting to those in ambulatory surgery centers in the state of Florida.[2] Florida collects data on adverse events occurring in both the office and the ambulatory surgical setting, but these data have limitations and there are differences in adverse event reporting and the requirements between these facilities, as shown by Venkat et al.[3] The Vila study also makes the claim, without evidence for support, that office surgery may be safer when an anesthesiologist is present.[2] This statement upset the society of nurse anesthetists, and a discussion on this issue is beyond the scope of this commentary.

Vila et al.,[2] in their *Archives of Surgery* study, comment about possible reasons for differences in the outcomes between different facilities. This must be regarded as pure speculation, because the data analyzed are inadequate to address these issues, as the researchers themselves acknowledge in their own article. At the time of the study, the databases of procedures performed in ambulatory surgical centers and physicians' offices differed so substantially that an accurate comparison of the facilities was nearly impossible. Accurate data on the results of surgical procedures performed in the office setting are extremely difficult to obtain and, when available, are not directly comparable with publicly available information from hospitals and ambulatory surgical centers. The definition of an adverse event used for an ambulatory surgical center differs from that used for physicians' offices. The time frames encompassed by the ambulatory surgical center and office databases, used in the Vila study, are markedly different. The actual number of procedures performed in the office setting that would qualify for inclusion in the Vila study is unknown, although they make a poor estimate. The denominator of patients in the Vila study biased the results, as the numbers used were markedly different for the ambulatory surgical center, with all procedures being performed included in the ambulatory surgical center numbers while only high-level anesthesia cases (levels II and III) were used in the office facility numbers. Also, only registered office facilities were used in the denominator, but adverse events and deaths occurred under level I anesthesia and under levels II and III anesthesia in nonregistered offices and should have been included in the numerator of the study, which shows an overestimation of the risks of office procedures in the Vila study.

In 2004, Venkat and colleagues[3] reviewed the *Archives of Surgery* study with the goal of reassessing the risk of mortality from ambulatory surgical center and office-based facility procedures using improved estimates of the numbers of cases performed. Although exact numbers are still unknown (as this information is not collected by the Board of Medicine), a reassessment of the data allowed the authors to conclude that available evidence speaks to the safety of outpatient surgery in all settings, with even a lower adverse event and mortality rate in the physician's office when compared with other facilities. These findings are consistent with other studies supporting the safety of accredited office facilities and outpatient surgery in general. Morello and associates[4]

initially showed comparative safety among accredited offices, surgery centers, and hospitals with more than 400,000 procedures in the outpatient setting, and these data are updated in currently unpublished data from the American Association for Accreditation of Ambulatory Surgery Facilities, with more than 900,000 procedures showing continued safety in accredited facilities.

When flawed studies are reported to the public or quoted in the literature, such as in *U.S. News & World Report*,[1] the *Archives of Surgery* study,[2] and the *PRS* article[5] on patient safety, the results tend to become gospel and are almost impossible to dislodge from the forum of public discussion. The citing of these flawed reports makes it difficult to protect patients and the public from this type of destructive pseudo-information. The suggestion would be that physicians continue to critically read any medical literature that may be read by the public or touted in the media. Do your own research, and draw your own conclusions based on your own experiences and facts and then set the record straight, as many have done in our specialty. It is unfortunate when poorly used statistics and discredited studies grow in the media and incorrect conclusions become accepted as fact, when all they really qualify as is junk science and they misinform the public (as in the silicone controversy).

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Reply

Sir:

The comments of Clayman and Seagle are important. The increasingly voluminous amount of research makes the physicians' ability to filter discredited work progressively more difficult. The *U.S. News & World Report* is reputable and reflects lay opinion; its article cites a respected peer-reviewed journal. The efforts and diligent reply of Clayman and Seagle are what our community needs to stay vigilant and remain abreast of the seemingly mountainous research, especially when such research derives from ostensibly sound sources. Their reply, however, not only questions an individual article but also broadly argues that office-based surgery is indeed safe—an argument that is not only controversial but belies the very need for patient safety improvement. The *U.S. News & World Report* article is only one of several that my coauthors and I cite to demonstrate the urgent need for such improvement. These sources echo the general sentiment sounded in the *U.S. News & World Report* article: that we in the medical profession must urgently improve patient safety, or others will force us to improve it via regulation. Unfortunately, no patient safety research can be flawless. Self-reporting and tracking mistakes or errors are innately "questionable," and given the low incidence of complications in any setting, prospective studies are cost prohibitive. Under these circumstances, "safe" becomes a relative term. We as physicians must view safety conservatively—from the patient's perspective. Our obligation is to pursue changes above what we as physicians feel is safe and to rise to the level that the public demands. Clayman and Seagle's reply is important, articulate, and insightful. It is greatly appreciated, and we hope that one day, through venerable efforts such as theirs, patient care will become indisputably safe.

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Viewpoints

The Bottleneck Theory: Accounting for Clinical and Experimental Variability in the Outcome of Functional Muscle Transfer

Sir:

Functional muscle transfer with cross-facial nerve grafting is the accepted standard treatment in facial reanimation surgery. Variable results have been reported, ranging from poor postoperative movement[1] to pleasing symmetry to overactivity or "tightness."[2] Several observations have suggested that such "tightness" is dynamic rather than fibrotic. For instance, tightness only seems to affect those grafts with pleasing initial motion, and transfers undertaken in the periphery appear unaffected by the problem.

It is known that muscles reinnervated by a reduced axonal number form greatly enlarged motor units.[3] Enlarged motor units maximize force generation in the

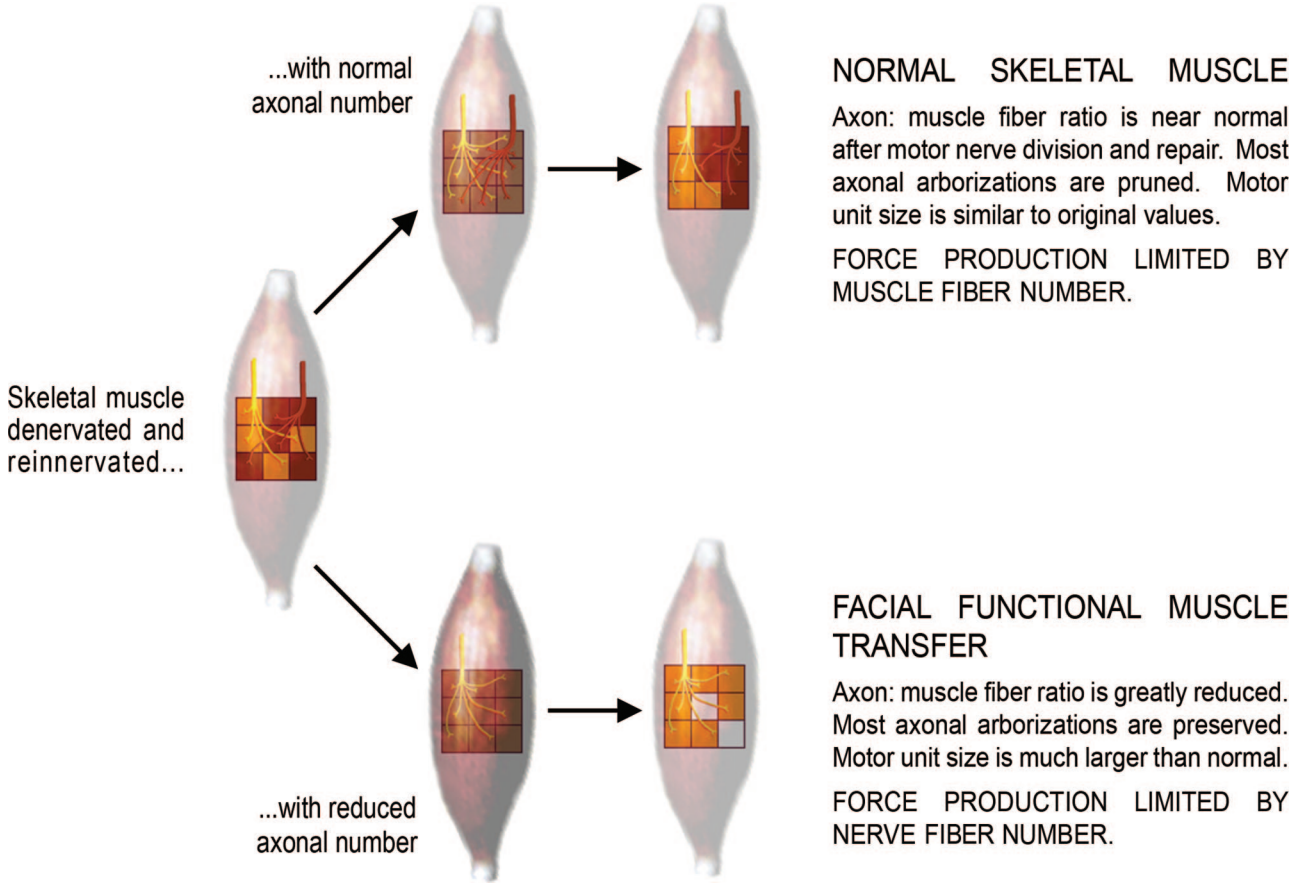

Fig. 1. The effect of axon number on motor unit size formation following muscle reinnervation.

face of a small axonal input, but increase sensitivity to small changes in reinnervating axonal load. This results in highly variable physiological outcomes, as a small number of axons may be responsible for the function of a large number of myocytes. Thus, the addition or subtraction of even a few of such high-innervation ratio axons can change force output markedly.

The (re)innervation ratio depends on nerve:muscle fiber availability. When this ratio is high, initial polyinnervation of muscle fibers by arborization is likely, but pruning of polyinnervating sprouts restores a normal reinnervation ratio.[4] Conversely, when the nerve:muscle fiber ratio is low, polyinnervation is unlikely, resulting in minimal axon pruning and persistently enlarged innervation ratios[4] (Fig. 1).

Force production requires muscle fibers to be both present and innervated. Therefore, muscle fiber availability or motor input may limit force output by muscle transfers. If the reinnervating capacity of the motor axons exceeds muscle fiber availability, then the latter limits force generation (muscle-bottlenecked muscle). If there are more muscle fibers than can be reinnervated by motor axons despite maximal innervation ratio, then the nerve supply limits maximum force production (nerve-bottlenecked muscle). Normal muscle is muscle bottlenecked, as evidenced by the capacity for an increased innervation ratio after partial denervation.[3]

The usual donor units for facial reanimation are large muscles reinnervated by the contralateral facial nerve via a graft. The reinnervating axonal number may be insufficient to fully reinnervate the transfer, producing nerve-bottlenecked muscle. These are highly sensitive to variations in axonal load, since the addition or removal of individual axons changes innervated muscle fiber availability by a number equivalent to the nerve's maximum motor unit size. This contrasts with normal facial muscle, where the loss of even high percentages of motor axons can be compensated for without detriment through motor unit expansion[3] (increasing axonal input has no effect on force production, as all myocytes are already innervated).

Unsatisfactory muscle transfer outcomes could possibly be avoided by reproducing a muscle-bottlenecked system. This would require a donor muscle possessing the "correct" number of muscle fibers for optimal force production and sufficient donor motor input to reinnervate it fully (with as near to normal innervation ratio as possible). The facial nerve appears, from experimental work, to be able to double its innervation ratio,[5] suggesting that a single facial nerve could innervate both sides of the face while just retaining bilateral muscle-bottlenecked systems.

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DISCLOSURE

None of the authors have any financial interest or commercial conflict of interest in the publication of this communication. The preparation of this communication did not require the use of any medical products, devices, drugs, or the like.

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The Mathematics of Breast Reduction Surgery

Sir:

In the United Kingdom, reduction mammaplasty can be performed in the National Health Service if the patient has breast asymmetry, back pain, or a psychological condition directly attributable to breast size. Some centers limit cases based on the estimated weight of breast tissue needed to be removed to give symptomatic improvement.

We challenge this concept that the reduction in breast weight alone will directly improve the symptoms related to posture. During surgery, two factors are altered: breast weight and the location of the nipple-areola complex.

We propose that relocation of the nipple-areola complex contributes as much to the resolution of symptoms as the weight of tissue removed. By lifting the breast on the chest wall, better support is afforded by the shoulder girdle. The breast produces a mechanical force (moment) around the clavicle and counterbalance is required, proportional to the weight of the breast and the distance at which it acts.

The following mathematical model describes how the counterbalance can be reduced significantly using a standard breast reduction technique.

The model is a seesaw in balance (Fig. 1). The breast weight ($w1$) acts at the nipple, which is a known distance ($y2$) from the clavicle (or sternum). This is counterbalanced by a factor of body weight ($w2$), a constant, which acts via the pulling back of the shoulders ($y2$). As $y1$ and $w1$ decrease after reduction mammaplasty, and $w2$ remains constant, $y2$ has a compensatory drop, thereby improving posture.

Calculating the mechanics of the force produces the following formula for the single breast:

$$y2 = \frac{w1 \times y1}{w2}$$

The weight of the breast is said to act at the nipple, although in real life it will act from the center of gravity

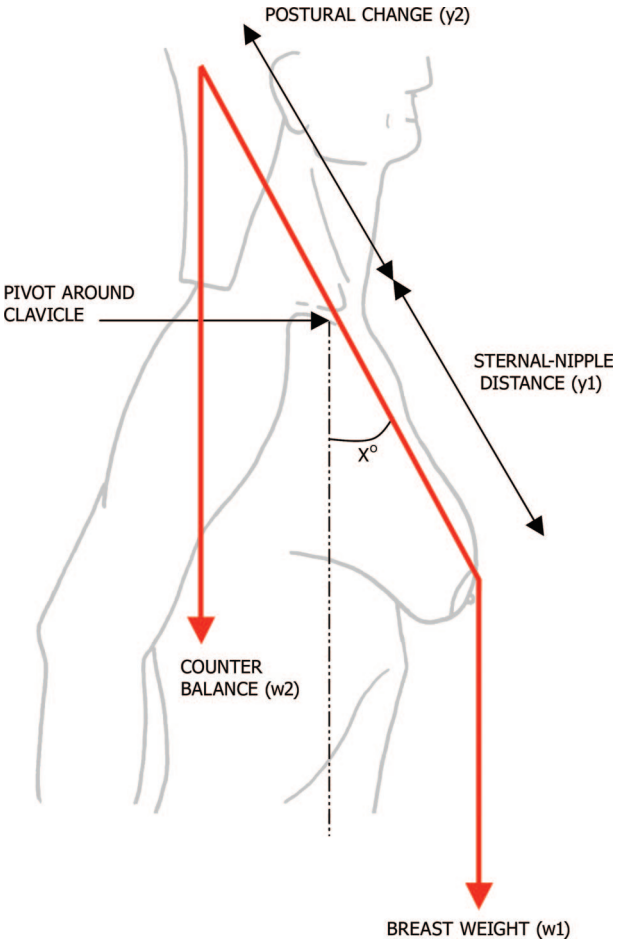

Fig. 1. The breast acts as a seesaw in balance based on the values of breast weight ($w1$) and sternal-nipple distance ($y1$).

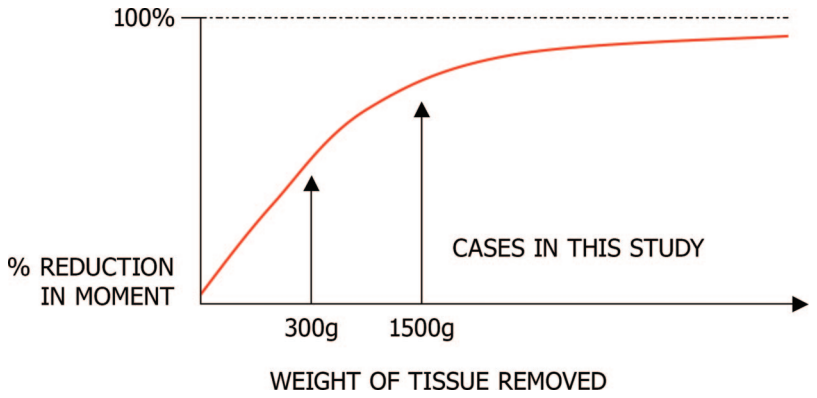

Fig. 2. Although a five-fold increase in weight is removed, the benefit to posture only increases by 25 percent.

of the breast, a value that cannot be measured accurately in vivo. Also, the protrusion of the breast (x) remains unchanged.

We want to know the change in $y2$ after surgery. The three known values postoperatively are as follows: the sternum to nipple distance before (preoperatively), Yb; the sternum to nipple distance after (postoperatively) Ya; and the weight of the tissue removed, E.

The $\Delta y2$ value is the change in posture. It is proportional to changes in the above values.

The formula for percentage reduction in moment (ΔY) is as follows:

$$\% \text{ reduction} \cong 1 - \left[\frac{Ya}{Yb \times (1 + E)} \right] \times 100$$

From our data, we have compiled a set of data relating to reduction mammaplasty of various magnitudes with a range of sternal-nipple distances.

If the percentage of reduction is plotted against the weight of the tissue removed for a fixed sternal-nipple distance, a logarithmic curve results (Fig. 2).

Reduction of this moment based on weight and sternal-nipple distance will improve posture and symptoms. Using this argument, it is difficult to judge when a mastopexy becomes a breast reduction. In cases of very large breasts, there is no argument that the mass of breast tissue is the cause of discomfort, regardless of the sternal-nipple distance. Smaller breast reductions (<300 g) can still achieve improvement, but the effect is mainly due to the relocation of the nipple-areola complex. This improvement is going to be more noticeable in the smaller-framed individual.

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An Extended Latissimus Dorsi Musculocutaneous Flap Raised on the Serratus Branch and Supercharged with the Circumflex Scapular Pedicle

Sir:

The dominant vascular pedicle of the musculocutaneous latissimus dorsi flap is the thoracodorsal vessel, a branch of the subscapular vessel.[1] Studies have proven that in cases in which the main thoracodorsal pedicle has been ligated (e.g., previous mastectomy, free flap breast reconstruction), flow to the latissimus dorsi is rapidly re-established through the serratus branch of the thoracodorsal vessel by means of reverse flow.[2,3]

There are, however, cases in which the vascularity of the skin island to the musculocutaneous flap has been compromised by this nondominant pedicle.[2] We present a case report of an *extended* latissimus dorsi musculocutaneous flap, raised on the serratus anterior branch with microvascular supercharging, in the setting of a ligated thoracodorsal vascular pedicle.

A 41-year-old woman had previously undergone a bilateral mastectomy for breast pain. She had initially been reconstructed using Becker tissue expanders, and following six revision procedures for either capsular pain or low-grade infections, we proceeded to autologous breast reconstruction. A deep inferior

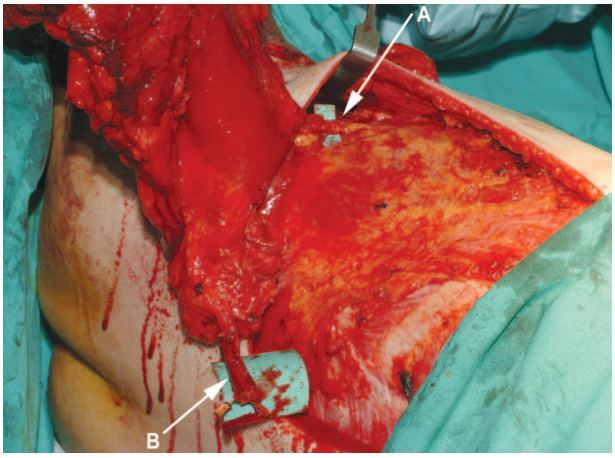

Fig. 1. *A*, serratus branch pedicle; *B*, divided circumflex scapular vessels.

epigastric perforator flap was raised and anastomosed to the thoracodorsal vessels. Unfortunately, the flap failed to establish an adequate flow.

A year later, a decision was made to raise a pedicled *extended* latissimus dorsi flap based on the serratus branch, although there was concern that backflow via the serratus branch might not be adequate for flap perfusion. Therefore, at operation, the extended latissimus dorsi was raised on the serratus, incorporating the circumflex scapular vessels at the apex of the upper extremity of the flap (Fig. 1). To facilitate transfer of the flap, the insertion of latissimus dorsi onto the humerus and the circumflex scapular pedicle were divided and the flap was transferred for insetting. After division of this pedicle, there was a noticeable change in the perfusion of the flap.

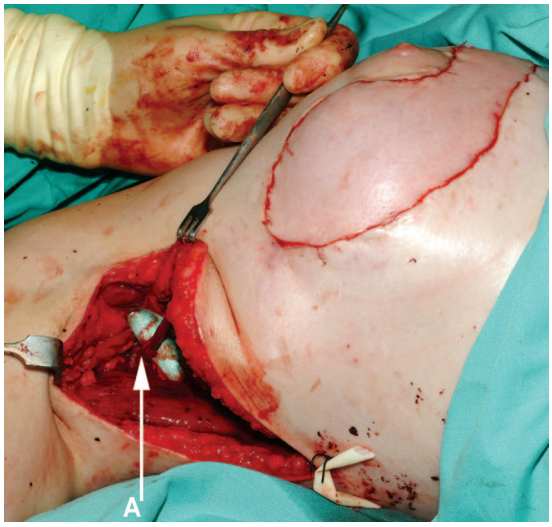

Fig. 2. Re-anastomosed circumflex scapular vessels.

A decision was made to augment the blood supply by re-anastomosing the artery and one vein of the circumflex scapular pedicle, *anterior* to the teres major. Perfusion of the flap improved immediately (Fig. 2). Postoperative recovery was unremarkable.

In situations in which the main thoracodorsal pedicle to the latissimus dorsi flap is divided, blood flow is re-established in the serratus branch of the thoracodorsal artery by means of reverse flow, in addition to increased collateralization from the scapular plexus.[4]

Despite the literature supporting survival of the latissimus dorsi flap based on the serratus branch, there may be concern about raising an *extended* musculocutaneous flap based on a nondominant pedicle. As we had concern about this, the extended latissimus dorsi flap was raised incorporating the additional circumflex scapular pedicle. The teres major was an obstacle to the movement of the flap, but rather than divide the muscle, we divided and subsequently re-anastomosed the circumflex scapular pedicle, thereby preserving the function of teres major.

When raising the extended latissimus dorsi flap, this secondary pedicle is divided to allow the flap to move and to preserve the function of the teres major.

If there is subsequent compromise to the vascularity of the flap after division of the circumflex scapular vessels, then simple re-anastomosis of the pedicle is performed *anterior* to the teres major.

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The Rhomboid Flap for Immediate Breast Reconstruction after Quadrantectomy and Axillary Dissection

Sir:

Plastic surgery has become a fundamental part of breast cancer treatment. An interdisciplinary approach provides the patient undergoing breast reconstruction or repair with an enhanced quality of life.[1]

Quadrantectomy, understood as the resection of a breast quadrant, followed by axillary dissection and postoperative radiotherapy, has shown a prognosis similar to that for mastectomies. Its main objective is maximum local control, with minimum mutilation. It was widely promoted by Veronesi et al.[2] in the 1980s, and in many cases, its use is clearly indicated.

There are frequently three types of breast deformities secondary to quadrantectomies, classified by Berrino as types I to III: deviation or distortion of the nipple-areola complex, tissue deficiency (gland and/or skin), and breast retraction.[1] Various techniques have been recommended for immediate or late repair, from primary suture to skin glandular or myocutaneous flaps.[3–5]

We present our experience with immediate breast repair with a rhomboid skin flap, compare the latter with previously used methods, and describe the extent of the corrections obtained.

A total of 250 patients with an average age of 52 years were submitted to immediate breast repair after quadrantectomy and axillary dissection, from June of 1997 to June of 2005. A rhomboid, cutaneous-glandular or cutaneous-fat projected flap, similar to a Limberg flap, was used.

The surgical procedure consists of identifying the projection of the tumor on the skin, then marking a diamond around the lesion, the margins of which are determined by the specialist. Afterward, the rhomboid flap to be used is also marked (Fig. 1, *above*). Skin and subcutaneous tissue are incised to the muscular plane, and detachment is performed to allow complete rotation (Fig. 1, *center*). Synthesis consists of fixing the flap base to the subcutaneous tissue of the upper margin of the area to be repaired to better support the flap and skin suture (Fig. 1, *below*). All cases were drained with a continuous aspiration drain, which is left in place for an average period of 8 days. Patients generally began radiotherapy in the third postoperative week.

The rhomboid flap allows the breast contour to be maintained in practically all cases, thereby attenuating or even avoiding secondary deformities. The patient's subjective assessment of the aesthetic aspect is always positive.

Tissue redundancy is commonly observed at the anterior axillary line for two reasons: delivery of the latissimus dorsi from the thoracic wall, during surgery, and late lymphatic edema caused by axillary emptying. The alteration can be attenuated with one of the segments of the rhomboid flap.

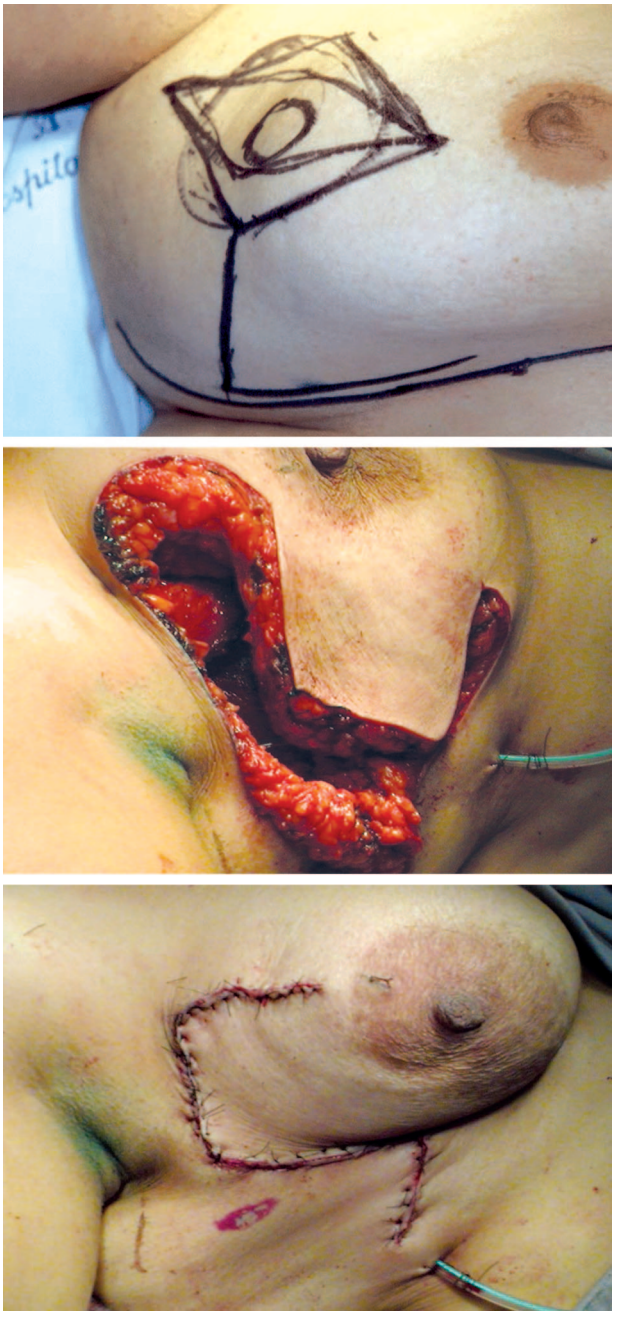

Fig. 1. (*Above*) Delimitation of the projection of the tumor on the skin and diamond-shaped marking around the lesion and the rhomboid flap, with a breast-oriented pedicle. (*Center*) Resected tumor and the resulting postquadrantectomy area, with the flap incised and undermined at the peitoral fascia. (*Below*) Positioned and sutured flap.

Although the scar is large, it is a good quality scar in the long term, possibly due to postoperative radiotherapy. The mammary groove is not distorted in the external quadrants, although there is a slight constriction of the groove in relation to the internal inferior quadrant. It can be corrected later, with no technical difficulties.

We prefer to have a contralateral balance at a second stage, as the degree of postradiotherapy mammary reduction is quite variable.

In conclusion, conservative surgery has been increasingly employed and the methods used are aesthetically oriented. The rhomboid flap has been demonstrated to be very versatile because the procedure is easily performed, the flap preserves the breast contour, and the early and long-term results are favorable. It has been the option for immediate repairs.

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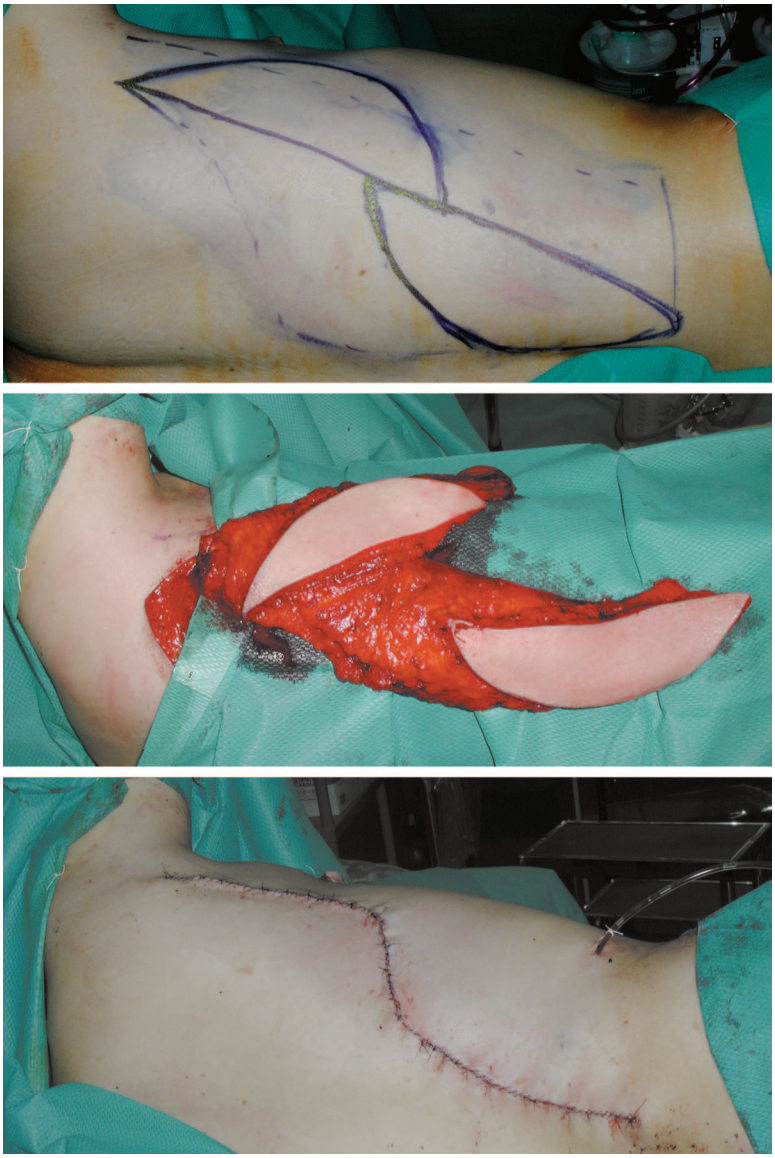

Fig. 1. A divided latissimus dorsi flap was elevated for breast reconstruction after mastectomy, and the donor site was directly closed.

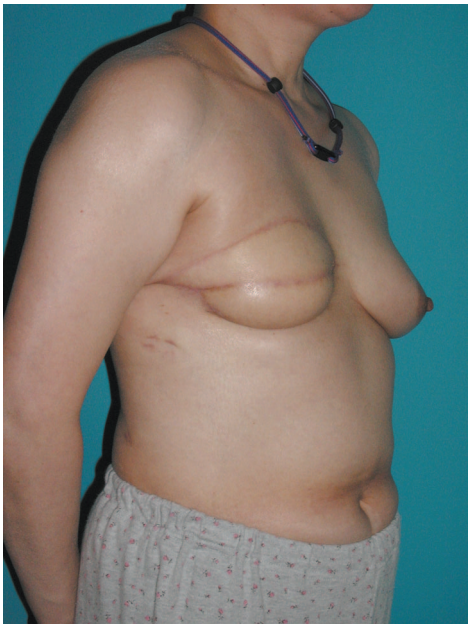

Fig. 2. Postoperative view of the reconstructed breast.

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Divided Latissimus Dorsi Musculocutaneous Flap for Breast Reconstruction

Sir:

The latissimus dorsi musculocutaneous flap can be divided as two skin islands on a pedicle and sometimes used for chest wall reconstruction.[1,2] This dividing method is so advantageous that primary closure without a skin graft is possible at the donor site, along with harvest of a wide flap[3] (Figs. 1 and 2).

We consider this method also to be versatile for breast mound reconstruction, because of the ability to harvest wide skin and large volume without a skin graft. Although this method may not be the first choice when the aesthetic demand is very high, it can be used in selected cases as an alternative for breast reconstruction.

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Augmentation Mammaplasty: Postoperative Cephalosporin-Induced Hepatitis

Sir:

In the past 2 years, three patients developed postoperative hepatitis 2 to 3 weeks after augmentation mammaplasty. In one case, hepatitis was traced to the patient eating raw oysters. The other two patients tested negative and were thought to have preoperative sensitization to cephalosporin and the administration of a single intraoperative dose of Kefzol. One of the patients recalled two separate 7-day treatments of cephalosporin 1 month apart, 9 months before her surgery. The other patient had exposure to antibiotics months before the onset of hepatitis, but we were unable to get further information regarding the type of antibiotic.

All three patients became quite ill after their surgery, with jaundice and abnormal liver function studies. The two with the allergic response requested removal of their implants, though this was not done. All had a complete recovery.

An estimated 1.25 million people in the United States have chronic hepatitis B virus infection, and more than 4 million have chronic hepatitis C virus, formerly known as non-A, non-B hepatitis. Immunization is currently available for hepatitis B virus but not for HBC.[1]

A search of the literature revealed few articles dealing with allergic hepatitis.[2–5]

The American Society of Plastic Surgeons discusses the use of prophylactic antibiotics on their Web site under "Practice Parameter: Treatment Principles of Silicone Breast Implants." They indicated that the "intraoperative prophylactic use of antibiotics is a time-honored surgical dictum and most surgeons administer antibiotics just before the time of surgery (most commonly cephalosporin-type antibiotics when not contraindicated due to an allergy)."

The implications of using prophylactic antibiotics affect both the patient and the physician. The potential benefit of prophylactic antibiotics may be offset by the more significant problems of pseudomembranous colitis, yeast infections, and allergic reactions, including hepatitis as reported here. The development of resistant organisms and costs are also to be considered.[6]

For the physician, there are implications for the patient's welfare and there may also be potential legal issues. Have preoperative prophylactic antibiotics become "standard of care"? If not, should the American Society of Plastic Surgeons adjust their stance? Are we vulnerable when a patient develops a severe reaction to prophylactic antibiotics? Do we need to address these issues in our informed consent?

In my own practice, I used no prophylactic antibiotics from 1970 to the mid-1980s unless indicated, and had no infections in augmentation mammaplasty. In the mid-1980s, I succumbed to the use of antibiotics because of legal concerns.

These two cases of allergic hepatitis are presented because I believe that it is important for physicians to understand that there lingers one more potential complication from a form of therapy that is pervasive and still poorly understood.

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Mesotherapy: Can It Be a Cause of Perforator Flap Failure?

Sir:

We would like to present a case of failed breast reconstruction with the free perforator flap that was most likely related to the adverse effects of previous mesotherapy procedures.

A 41-year-old woman with right intraductal breast carcinoma was referred for immediate reconstruction. As there were abdominal scars, a free superior gluteal artery perforator flap was planned. Preoperative color Doppler ultrasonography identified two major perforators in the supra-

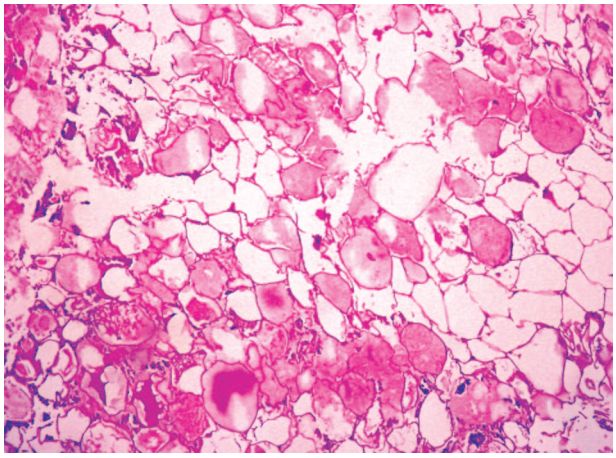

Fig. 1. Light microscopic evaluation of the subcutaneous nodules in the gluteal region. Adipocytes underwent fat necrosis throughout the field, and thickening of vessel lumens is evident (*hematoxylin and eosin staining, 100×*)

piriform area. A review of the patient's medical history revealed that she had undergone nine applications of mesotherapy to the bilateral gluteal regions for contour correction 15 months before her admission.

During the operation, a 23 × 12-cm superior gluteal artery perforator flap based on both perforators was raised. During flap dissection, numerous superficial subcutaneous nodules along the flap edges measuring about 1 cm in diameter were noticed and removed for histopathologic examination. When the perforator dissection ended, the better one was kept and the other was ligated due to the pedicle configuration. However, arterial perfusion immediately diminished all over the flap in a nonhomogenous fashion. No dissection trauma was observed along the perforator under the operating microscope, which was physically intact and pulsating. Topical lidocaine and papaverine were ap-

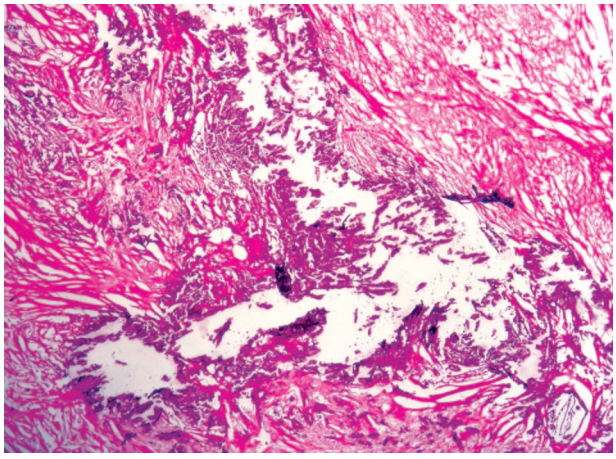

Fig. 2. Light microscopic evaluation of the subcutaneous nodules in the gluteal region. A calcified nidus (*center*) surrounded by fibrosis (*hematoxylin and eosin staining, 100×*).

plied with warming, but no recovery was observed in 40 minutes. The pedicle was then divided and anastomosed to the internal mammary vessels to create a sympathectomy effect. However, after microanastomosis, no circulation could be detected in the flap, despite a working anastomosis and bleeding proximal side branches. The flap underwent total necrosis and débrided on the fourth postoperative day. Histopathologic examination of the subcutaneous nodules detected calcified niduses surrounded by fibrosis and fat necrosis (Figs. 1 and 2). The vessel lumens in small to medium-sized vessels were hypertrophied.

Today, there is an increased trend all over the world toward mesotherapy, which is offered as a surgery-free fat reduction modality.[1] However, there is little evidence of its efficacy but inversely proportional, well-documented literature on its adverse effects.[2–5] These effects may be divided into two groups: those related to the multiple-injection procedure itself (hematomas, hypertrophic scars, pigmentations, abscesses and fistulas, and mycobacterial infections) and those related to the injected solutions (allergic hypersensitivity reactions, urticaria, skin and fat necrosis, subcutaneous calcifications, and adenopathy).[3–5] In the presented case, it may not be possible to establish a definitive link between the flap failure and the previous mesotherapy sessions; however, as the other causes of free flap failure were ruled out, mesotherapy damage to the flap donor site might have been a cause. In a case published by Lee and Chang,[4] similar subcutaneous nodules were found and histopathological examination revealed fat necrosis with calcifications and panniculitis, as well as thrombosis in small to medium-sized vessels. Rose and Morgan[5] detected a more active septal and lobular panniculitis by lymphocytes and macrophages in the earlier periods. As perforator flaps are relatively less vascular flaps that are dependent on the arborizing branches of the perforator artery in the subcutaneous fat, the subdermal plexus itself, and the choke vessels, they may be more liable to the damage created by mesotherapy, which is performed just at these levels.

Therefore, it may be wise to take the patient's mesotherapy history into account and to test the perfusion capacity of individual perforators before ligating the others, or to use more than one perforator in those cases.

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Aesthetic Correction of Tuberous Breast Deformity in a Male-to-Female Transsexual Patient

Sir:

Tuberous breast deformity was first described by Rees and Aston.[1] It is characterized by a constricting ring at the base of the breast. The breast base is absent or deficient in the vertical as well as horizontal dimension. With breast development, the breast projects forward but lacks the characteristic fullness. The nipple-areola complex is overdeveloped, with apparent herniation of the breast tissue into the areola complex. While the tuberous breast deformity in the female breast is a common and well-described condition, the tuberous male breast is a very unusual variant of gynecomastia.[2]

We describe a tuberous breast deformity in a male-to-female transsexual patient. To our knowledge, there is no report in the literature of tuberous breast deformity in these patients.

A 24-year-old male-to-female transsexual patient presented to our department requesting an augmentation mammaplasty. She had already been treated with hormonal therapy for 2 years and had undergone gender-confirming surgery three times elsewhere. On physical examination, only a slightly developed left breast with palpable glandular tissue was observed; the right breast was better developed. The breasts showed apparent herniation of the breast tissue into the areola, hypoplasia of the lower medial and lateral quadrants, and insufficient skin in the subareolar region (Fig. 1). The tuberous breast deformity could be classified as type III using the classification of von Heimburg et al.[3] We performed an augmentation mammaplasty on this patient using a modification of the technique first described by Madrekas et al.[4] Through a periareolar, doughnut-shaped incision, the overdeveloped areolar tissue was removed. The breast tissue was mobilized, and the constricting ring was completely disrupted through a vertical incision. The pectoralis muscle was incised, and a subpectoral pocket was created to place the silicone gel prosthesis behind the muscle (Mentor High Profile; left side, 300 ml; right side, 250 ml). The medial and lateral breast tissue flaps were then attached in the cranial region using 3 × 0 polydioxanone mattress sutures. The breast tissue was placed to cover the prosthesis at its inferior pole, and periareolar skin closure was performed.

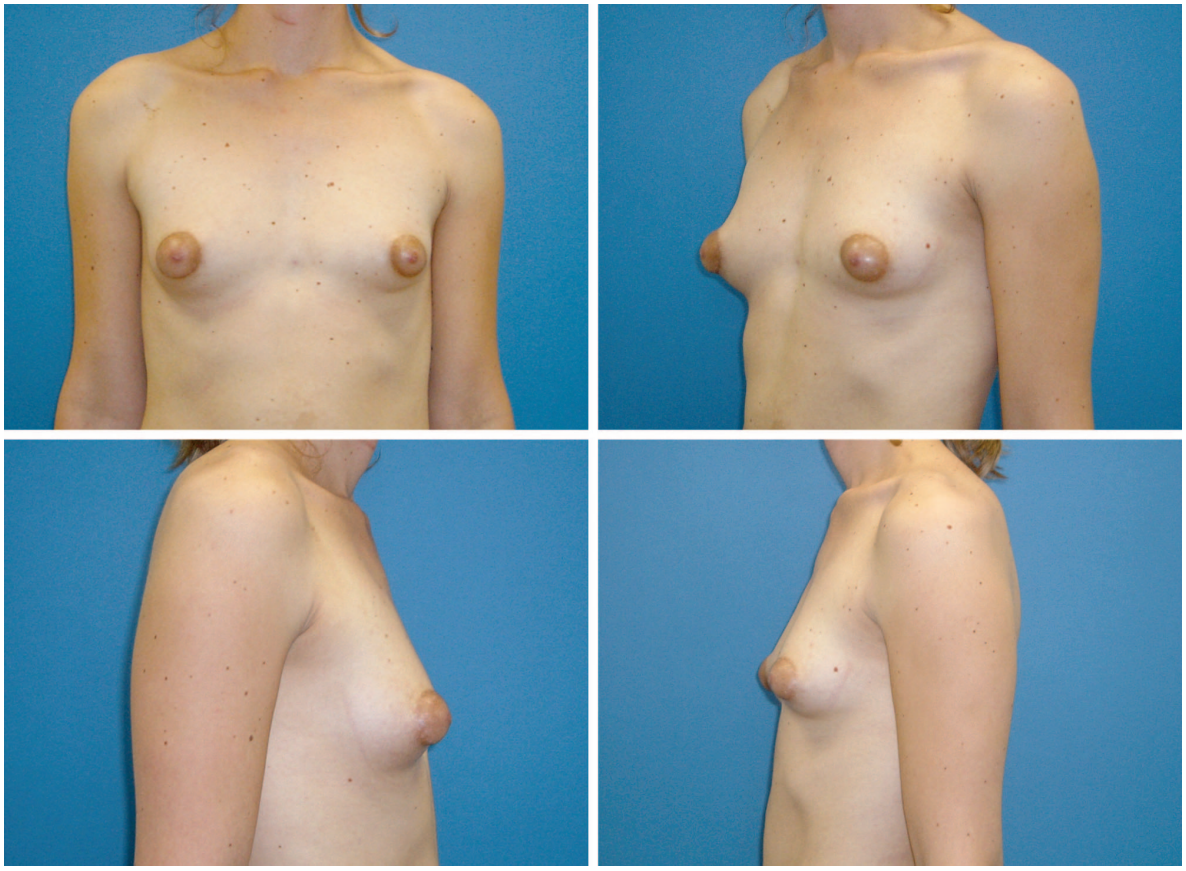

Fig. 1. The patient presented with a bilateral tuberous breast deformity type III, using the classification of von Heimburg et al. There was apparent herniation of the breast tissue into the nipple-areola complex, significant hypoplasia of the lower medial and lateral quadrants and insufficient skin in the subareolar region.

Postoperatively, the patient received oral antibiotics for 5 days and analgesic and antiphlogistic medication. The patient was advised to wear a special brassiere for at least 6 weeks. No postoperative complications occurred, and the patient was very satisfied with the operative result. After postoperative month 6, a nice aesthetic and symmetrical breast shape was achieved (Fig. 2).

Here we describe for the first time the development of a tuberous breast deformity in a male-to-female transsexual patient. The deformity was corrected using a modification of the technique first described by Madrekas et al.[4] Because of the underdevelopment of the breast itself, silicone gel prostheses were used for augmentation mammaplasty, and in contrast to the method described by Mandrekas et al., the implants were placed behind the pectoralis muscle and not subglandularly. We believe that, according to literature,[5] this will reduce the risk for onset of capsular contracture and give better coverage of the prostheses, especially in the case of tuberous breast deformity in combination with hypoplastic breast tissue.

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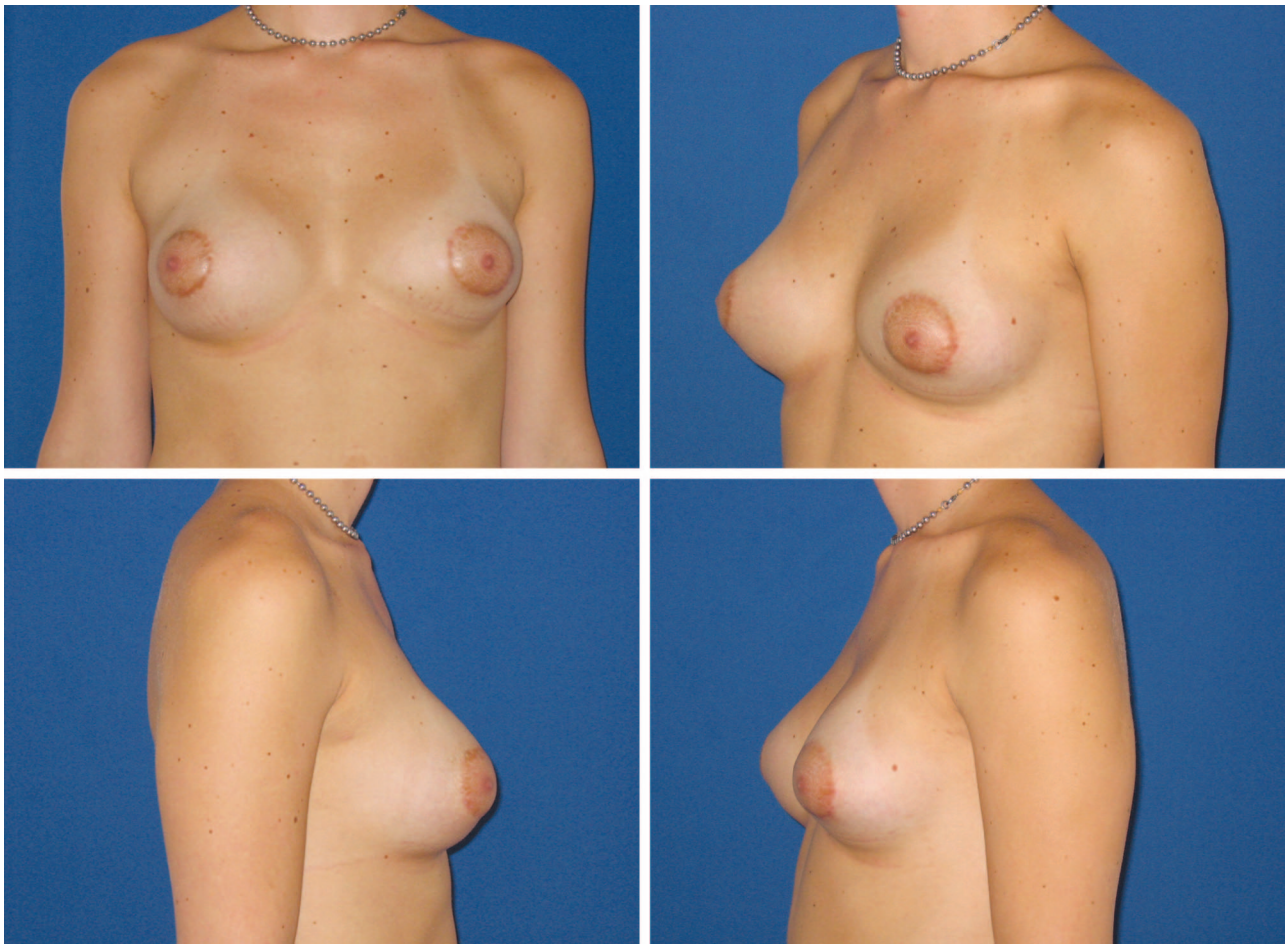

Fig. 2. Six-month postoperative views, after complete correction of the tuberous breast deformity. A nice aesthetic and symmetrical breast shape with good projection was achieved.

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Stewart-Treves Syndrome in a Congenitally Lymphedematous Upper Limb

Sir:

A 64-year-old man was referred to our plastic surgery unit with a 3-month history of severe forearm pain radiating to the axilla and an increase in limb swelling in a congenitally lymphedematous left arm. There were multiple nodules and blisters varying in color from blue to pink, with no cellulitis or axillary masses (Fig. 1).

His case was initially treated as an infection. An incisional biopsy was reported as benign, but a second biopsy revealed a spindle cell lesion with vascular spaces lined by malignant hyperchromatic cells. Immunocytochemical analysis confirmed angiosarcoma. A magnetic resonance imaging scan demonstrated no mass lesion. He proceeded to forequarter amputation with no adjuvant treatment. Irresectable recurrence was apparent at 1 year, and palliative radiotherapy for pain relief was commenced (Fig. 2).

Stewart and Treves described angiosarcoma arising from chronically obstructed lymph vessels in six postmastectomy patients in 1948.[1] The classic description of Stewart-Treves syndrome is associated

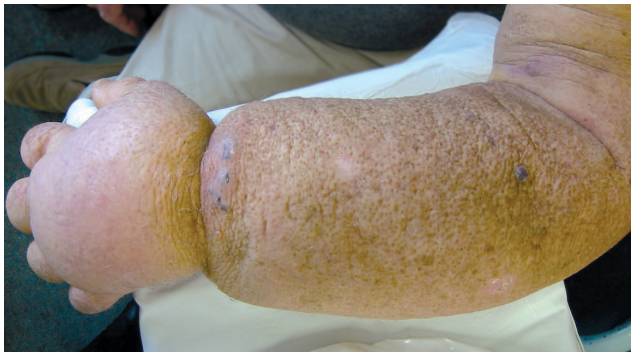

Fig. 1. Lymphedematous left upper limb with multiple blue nodules and blisters at the wrist and forearm.

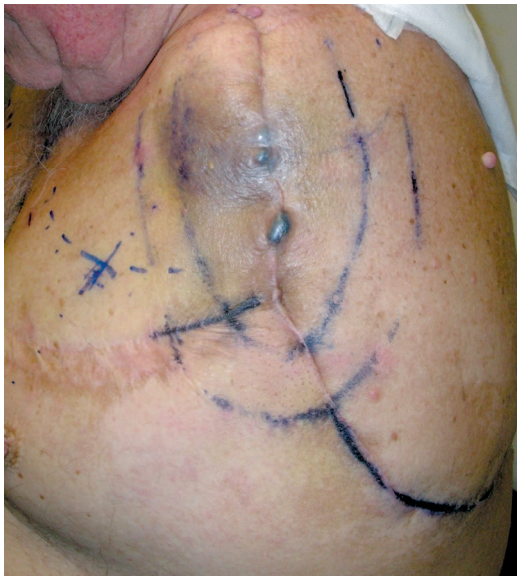

Fig. 2. Recurrence of lymphangiosarcoma in the forequarter amputation scar at 1 year.

with breast cancer but occurs in other forms of chronic lymphatic obstruction. The incidence is rare, with approximately 220 cases published[2] and only a few congenital lymphedema cases. Angiosarcomas are highly malignant tumors of endothelial origin. Angiosarcomas occurring without lymphedema are classically designated hemangiosarcoma and occur commonly in the head and neck region in adults after the fifth decade. The clinical course is aggressive, with a tendency toward local recurrence and widespread metastatic disease; the 5-year survival rate is less than 20 percent.[3] Angiosarcoma occurring in chronic lymphedema is known as lymphangiosarcoma. Its classic description is in survivors of breast cancer treatment, usually 10 years after mastectomy and axillary node dissection. Congenital lymphedema cases have an average latency period of 38 years.[2,4] The prognosis is poor, with early metastatic disease via lymphatic and hematogenous routes. The exact pathogenesis is still under debate. Chronic lymphedema predisposes to atypical vascular proliferation with resultant mutations of the endothelium and subsequent angiosarcoma.[2]

In addition, a relative immunosuppression protects these neoplastic cells from regulatory mechanisms. Future research may show a link between DNA damage and impaired detection and repair. Radiotherapy may act by creating a lymphatic obstruction and edema, or it may damage cellular DNA directly. The diagnosis is often delayed, as initial presentation may not be alarming.

The differential diagnosis includes benign pathology, such as infective disorders (*Mycobacterium marinum*, fungi), granulomatous conditions, vasculitis, traumatic ecchymoses, primary malignancies (Kaposi's sarcoma, melanoma), mycosis fungoides, the rare malignant hemangiopericytoma, and cutaneous metastatic deposits (e.g., breast, lung).

Histological confirmation (immunohistochemistry, CD-31), radiological determination of local disease, and staging with multidisciplinary treatment planning are recommended. Definitive treatment is radical ablative surgery, such as forequarter amputation. Wide excision with limb-preserving techniques has a high incidence of recurrence.[5] The mean survival time is 8 to 31 months, and less than 10 percent survive 5 years.[4] There is no convincing evidence for primary treatment with either radiotherapy or chemotherapy.

The dismal prognosis is compounded by the usual delay in diagnosis. This argues strongly for close monitoring of patients with chronic lymphedema and a high suspicion of skin changes, however benign they may appear.

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DISCLOSURE

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A Simple Template to Improve Preoperative Marking in Abdominoplasty

Sir:

Preoperative marking of cutaneous incisions in abdominoplasty procedures, including transverse rectus abdominis myocutaneous (TRAM) flap breast reconstruction (donor area), can be easily performed using a template made from the packaging for sterile gloves, allowing us to obtain adequate symmetry between the two sides of the excess flap to be resected (or to be raised in TRAM flap cases).

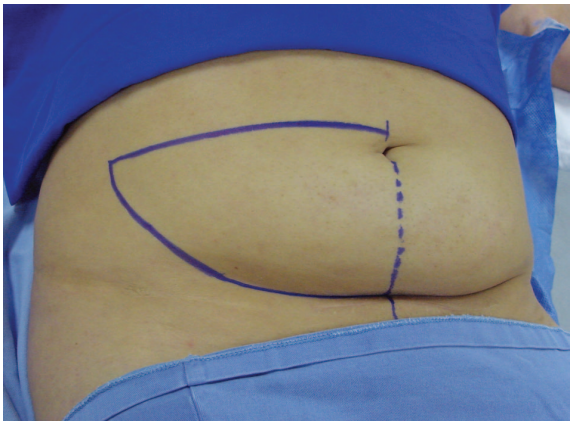

Fig. 1. Markings for the right side of the flap.

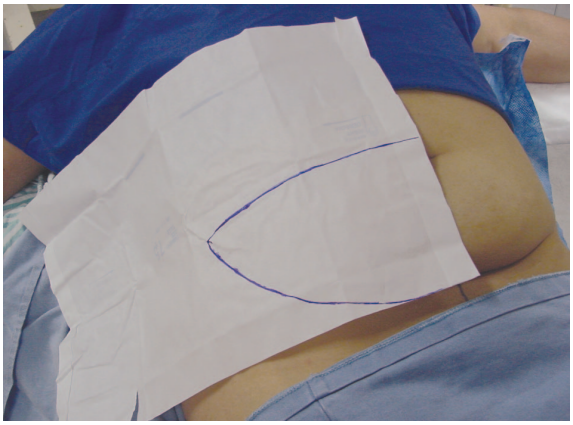

Fig. 2. The packaging for sterile gloves is used to make a template.

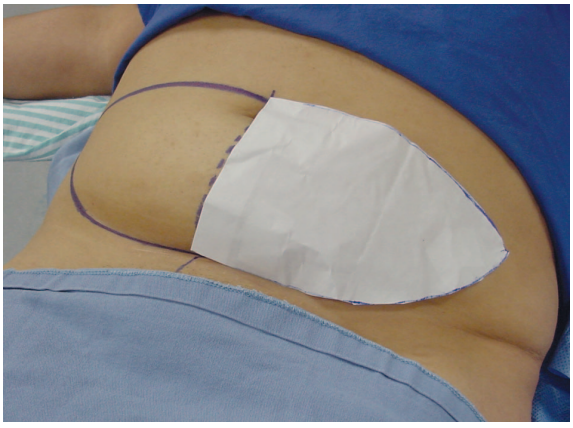

Fig. 3. The template is used to mark the left side of the flap.

We begin by marking the right side of the flap with methylene blue dye, following the area defined superiorly by a line from the anterosuperior iliac spine and the umbilicus and inferiorly by the groin and the pubic creases (Fig. 1).

We then cut a template from the packaging for sterile gloves, place it over the previous markings (Fig. 2), and then rotate the template 180 degrees to make the same markings on the left side (Fig. 3).

This is a simple, feasible method, and we recommend it for achieving a straight line for the abdominal scar.

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Cable Ties: A Simple Device with Multiple Applications in Plastic Surgery

Sir:

High-tech devices or techniques are becoming increasingly integrated into the practice of plastic surgery, either as diagnostic or therapeutic procedures. However, good craftsmanship remains as one of the bases of fine plastic surgical practice. This keeps open a wide field for useful innovations not necessarily based on sophisticated technology. The pioneers of our specialty have resorted to modifications of hardware tools or simple household devices to add to the surgical armamentarium. From time to time, new applications have been published for simple devices.[1,2]

The purpose of this communication is to bring up the subject of the multiple applications of cable ties. These self-retaining plastic straps allow gradual tightening through a ratchet effect, while making it impossible to release the grip. We have used cable ties for different procedures, as follows:

- In breast reduction, as a tourniquet to make Schwartzman's maneuver easier (a cable tie is secured at the base of the breast)
- For serial closure of complex wounds, based on the viscoelastic properties of skin[3,4] (Fig. 1)
- For giant nevus resections, applying the presuturing method[5,6] (Fig. 2)
- For upward traction of redundant soft tissues of the abdominal wall, in cases of dermolipectomies in morbidly obese patients (the large soft-tissue apron

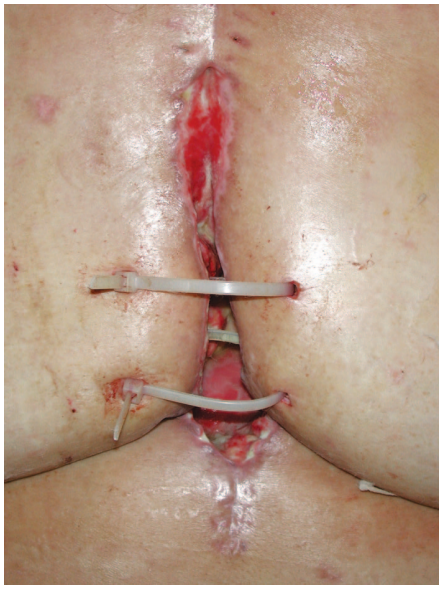

Fig. 1. Sternotomy osteitis has been repaired using pectoralis major muscle flaps, with resulting dehiscence of skin closure. Serial closure of skin dehiscence is achieved with cable ties.

is transfixed with cable ties, which in turn are suspended from an overhanging bar)

Specific advantages of the cable ties are their low price, easy availability in different sizes, and relatively simple sterilization and disposal after use. We expect that other colleagues will find further applications for this device.

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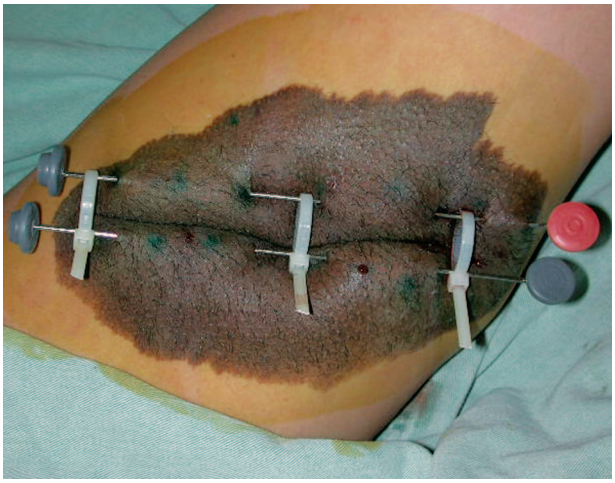

Fig. 2. For a giant nevus measuring 18 × 15 cm, cable ties have been placed for a presuturing effect.

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Removal of Radio-Opaque Foreign Body with Pinpoint Accuracy

Sir:

Finding a foreign body embedded in soft tissue can prove a difficult task, as many surgeons who routinely face this challenge know.[1–4] In facing a similar problem, I have developed a technique that works well for me. Over the years it has produced consistently good results and allowed easy detection and removal of the radio-opaque foreign body.

The foreign body is identified by means of an image intensifier x-ray device during surgery. The entrance point, if it is identifiable on the skin, is marked with methylene blue dye. When the entrance point is not visible, a small artery forceps is moved over the surface under the x-ray machine until it is placed directly over the foreign body. Two 22-gauge hypodermic needles are inserted through the skin at an approximately 90-degree angle to each other, with the tips aiming at the estimated position of the foreign body (Fig. 1). The image intensifier is then used to change the position of the needles until both tips touch each other and both are placed on the radio-opaque foreign body (Fig. 2). The position of the needles in relationship to the foreign body is verified at two 90-degree angles using the image intensifier. The needles are then used as a guide for accurate dissection into the precise location of the foreign body in the tissue, thereby minimizing the dissection required to find it. The incision is usually made at the entrance point, but if the

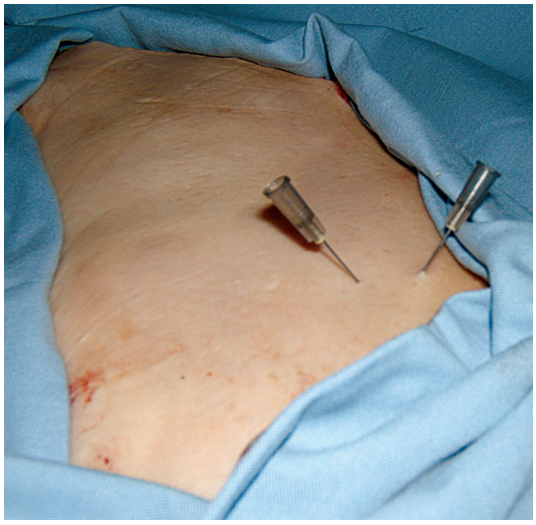

Fig. 1. Intraoperative view of two hypodermic needles pointing to the foreign body.

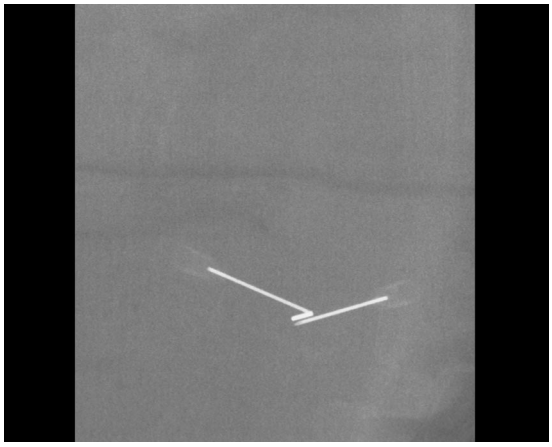

Fig. 2. Radiograph shows the position of the needles relative to the foreign body before dissection is commenced.

foreign body is remote from that point, then the incision is made in the skin directly over the foreign body. Small artery forceps is used to remove the foreign body through this small incision. The wound is closed in a routine manner. A tourniquet can be used if necessary.

The technique allows a fast approach to a foreign body, limited dissection, and consistently good results.

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Simple Trick in Tying a Knot in Microsurgery, without Crossing Your Hands

Sir:

Microsurgical suturing for vessel anastomosis and nerve repair can be tedious for many reasons. Working in a small space and under a magnifying field can also make things awkward. One normally needs to cross one's hands to make square knots when placing the suture in a transverse position (i.e., from right to left or vice versa). Sometimes when crossing the hands, structures or instruments obstruct the path, making it difficult to maneuver, or in some circumstances an important structure or instrument can be knocked out of position. Therefore, one may spend more time suturing than necessary.

Many techniques have been described in the literature to aid in suturing. New sutureless techniques were described around the turn of new millennium in an effort to speed up microanastomosis. These techniques included the use of lasers, tissue adhesive, sutureless sleeves, and vessel clips, among other things.[1–5]

We would like to share a simple technique for tying a knot during microsurgery that allows one to avoid crossing the hands to make a square knot. This simple trick is achieved just by alternately tying knots using a micro-forceps and the micro-needle holder.

After placing the suture through both ends of the vessel, the surgeon will normally use the needle holder to tie a knot. The first knot is tied by making a loop around the needle holder, picking up the short end, and crossing the hand to achieve a square knot. We suggest that the loop be made around the forceps (held by the left hand) to tie the first knot. The next knot is tied using the needle holder as one usually does. By alternating these two actions, you can avoid crossing the hand and make tying knots in microsurgery look easy and simple (Fig. 1).

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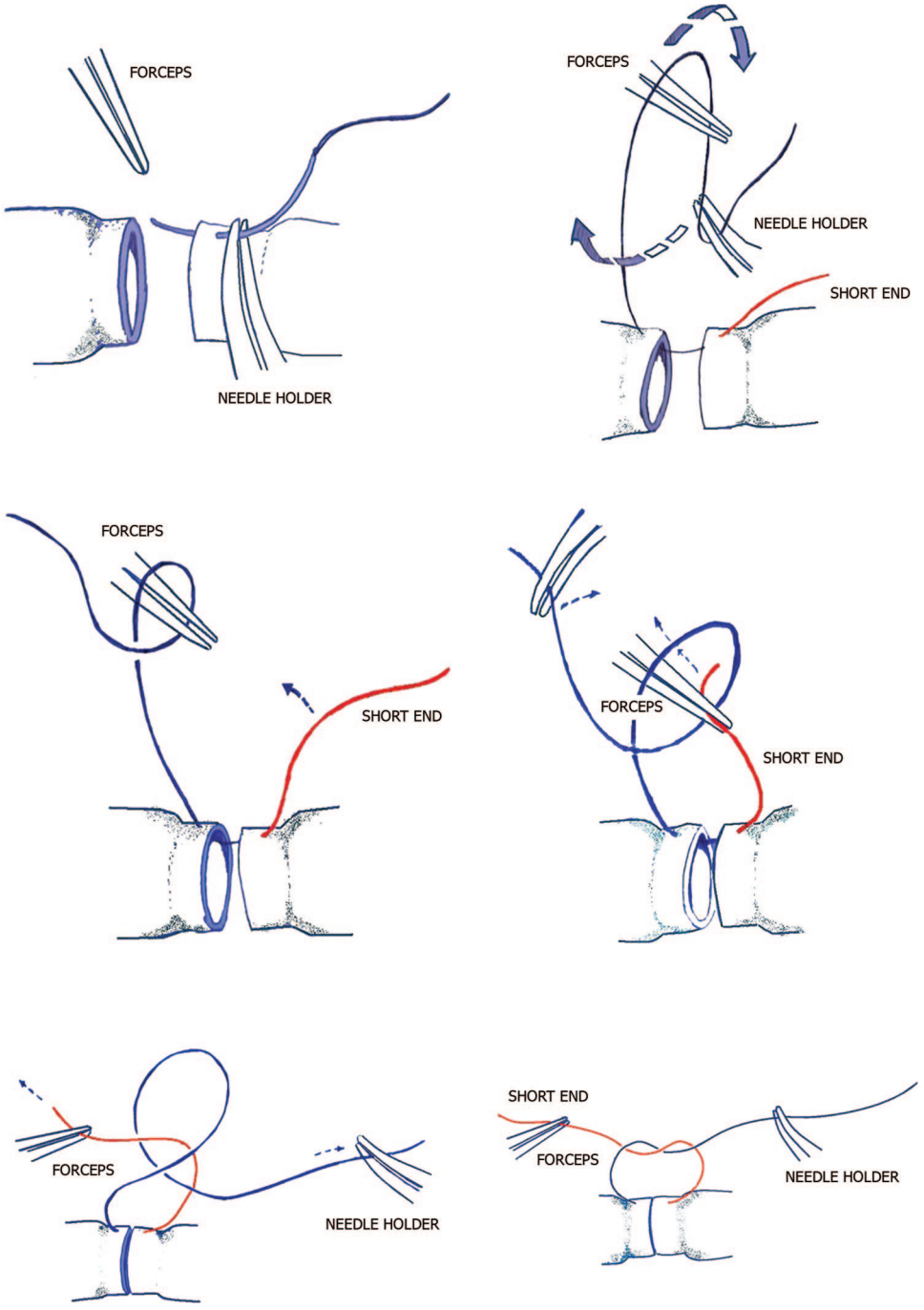

Fig. 1. Step-by-step illustrations of suture-tying techniques (the surgeon is right-handed). (*Above, left*) The suture is placed in a normal fashion, from right to left. (*Above, right*) For the first knot, a loop is made around the forceps. (*Center, left*) The short end of the suture is picked up using the forceps. (*Center, right*) The short end is pulled through the loop to make a square knot (*below, left and right*). These steps can be repeated for the third knot if required, after the surgeon has tied the second knot using the needle holder as one usually does.

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Gli-1 Oncogene: The Key to Keloidogenesis?

Sir:

Keloids are the result of a dysregulated wound-healing process and are characterized by the formation of excess scar tissue that proliferates beyond the boundaries of the inciting wound.[1] The pathophysiology and signal transduction of keloids are not completely understood.[2]

Previous studies have focused largely on the role of the keloid fibroblast, because it is primarily responsible for collagen and extracellular matrix production, which form the bulk of keloid tissue. In recent years, an increasing body of evidence has shown that autocrine, paracrine, and endocrine epithelial–mesenchymal interactions play a major role in normal skin homeostasis, growth, and differentiation. The secretory role of keratinocytes is now established and is known not only to influence the adjacent mesenchyme but also to have far-reaching systemic effects by modulation of the immune system.[3] More recent studies have found that keloids are formed by intrinsically normal fibroblasts that responded to an abnormal extracellular signal,[4] and that normal fibroblasts' behavior simulates that of keloid fibroblasts when they are cocultured with keloid-derived keratinocytes, with increased proliferation and collagen production.[3]

Sections of formalin-fixed, paraffin-embedded keloid tissue (2 to 3 μm) from 30 patients that were obtained from the pathology archives of Airlangga University, Surabaya, Indonesia, were tested for the presence of gli-1 by means of gli-1–specific antibodies (Santa Cruz Biotechnology, Santa Cruz, Calif.; sc-6152) (1:20) using an avidin biotin complex technique and steam heat–induced antigen retrieval. An avidin biotinylated enzyme complex kit (Dako LSAB2; Dako Corp., Carpinteria, Calif.) was used in combination with the automated Dako Autostainer. Hematoxylin was used as a counterstain, and diaminobenzidine tetrahydrochloride (Sigma D-5637) was used as the stain for gli-1.

All keloids demonstrated high expression of fibroblasts, fibrocytes, and epidermal keratinocytes surrounding the keloids (*stratum basale* and *spinosum*). Diaminobenzidine tetrahydrochloride is the dark orange stain (Fig. 1).

It is obvious that the changing behavior of normal fibroblasts to keloid fibroblasts is brought about mainly by the surrounding keratinocytes.[3] However, it is not yet clear how the keratinocytes could change the fibroblasts' behavior. Is it because of the gli-1 oncogene inside the keratinocytes?

The gli-1 oncogene was found in keloid fibroblasts using immunohistochemistry and reverse-transcriptase polymerase chain reaction in five keloid specimens, but it was not found in scar or normal skin fibroblasts.[2] Gli-1 protein was also identified in a subpopulation of mesen-

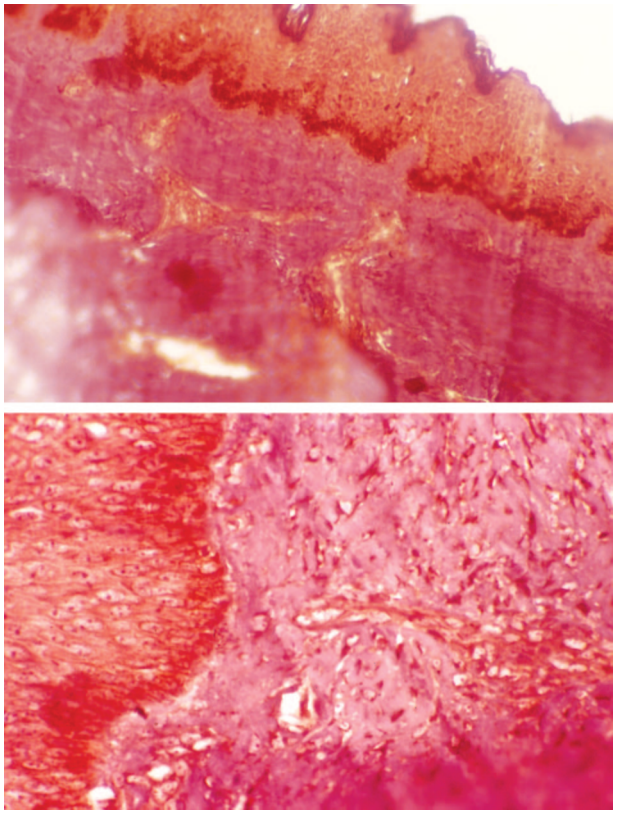

Fig. 1. Immunohistochemical stain for gli-1: (*above*) magnification, 200×; (*below*) magnification, 400×.

chymal cells located at the bulge area of hair follicles in normal skin.[5] The function of these mesenchymal cells is unknown. One possible function is to serve as a supply of stem cells needed at times of wound healing and repair of human skin. Keloids may represent a dysfunction of these mesenchymal stem cells responding to wound healing.[2]

The keloidogenesis (the forming of keloid) is likely due to the gli-1 oncogene within the keratinocytes. Given this evidence, anti–gli-1 oncogene therapy might be used as a novel and effective keloid treatment.

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Folds and Creases

Sir:

We have become aware of an ambiguity in the plastic and aesthetic surgical literature with regard to the nomenclature used to describe the various skin folds and creases.[1,2] We wish to clarify this issue as it relates to the current understanding of anatomic principles and clinical application.

First, it should be established that a "crease" is a visible line in the skin, whereas a "fold" is a redundancy of skin that is responsible in part, often in conjunction with connective tissue attachments, for a skin crease. The difference is intuitive; when one thinks of a palm reader interpreting a "life line," it should be emphasized that it is a hand crease being examined, not the fold of the palm.

"Creases" are frequently discussed as "folds" in discussions of the eyelids, the nasolabial area, and the submammary area. Nearly 30 years ago, Putterman and Urist appreciated this distinction in detailing goals for successful upper eyelid cosmesis.[3] They remarked on the need for symmetry in both the height of the upper lid creases and the folds of skin above these creases. Similar divisions in terminology were made by Barton and Gyimesi as they relate to the nasolabial fold and crease.[4] This language has clinical implications insofar as facial rejuvenating procedures often target skin laxity and reduction of skin folds with indirect crease minimization. In such cases, dermal fillers directly address skin creases by way of mass effect. In the same way, many articles incorrectly use the terms inframammary skin "crease" and "fold" synonymously.

Skin creases should be described as skin creases rather than folds. Along these lines, so to speak, we surgeons should adhere to a common terminology that respects the great advances in anatomic and clinical understanding that have been achieved over the course of medical history. In this way, we can partake in a scientific discourse that promises further precision and thus further clarification of our thinking as well as our surgical capabilities.

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Measurement of Radiation Exposure over a One-Year Period from Fluoroscan Mini C-Arm Imaging Unit

Sir:

Portability, ease of operation, and lack of need for radiological technologists have contributed to the popularity of mini c-arm imaging units among hand surgeons. Low levels of radiation exposure from conventional c-arm units[1–3] have been reported, leading to the perception of added safety for the lower-power mini c-arm units. However, no study has assessed exposures from mini c-arm units in the clinical practice of a hand surgeon. The purpose of this

study was to determine the level of radiation exposure to the hands of a single surgeon over the course of 1 year while using the Fluoroscan mini c-arm imaging device.

The imaging device employed was a Fluoroscan mini c-arm imager. Radiation exposure was measured over a period of 1 year with a dosimeter ring worn on the ring finger of the hand closest to the radiation source during imaging. A control ring of the same make and model was stored in the briefcase of the surgeon collecting the data. The minimum radiation detection limit for this model of dosimeter ring is 1.0 milliSievert (mSv). Every attempt was made to limit exposure times, particularly during real-time fluoroscopy. Twenty-six consecutive cases were collected over the course of one calendar year by a single surgeon.

Over the course of 1 year, a single ring worn during 26 consecutive procedures was found to have an exposure reading of less than 1.0 mSv.

This case series demonstrates that during the treatment of 26 consecutive cases over the course of 1 year, the hands of one surgeon received an exposure to radiation amounting to less than 1.0 mSv. In fact, the dose may have been only a small fraction of that amount, but the detection limit of the dosimeter ring employed was 1.0 mSv. Twelve thousand cases annually could be performed safely at the same dose per case rate.

Of note, the surgeon's own fingers, including the one with the dosimeter ring, were exposed on the visual image of the mini c-arm occasionally when trying to keep finger fractures reduced during active imaging. Despite this close proximity of his hands to the x-ray beam, the dose received by the ring was still below detectable levels. Even if the dosage received were 1.0 mSv, this is well below the annual dose limit for the extremities of 500 mSv,[4] as recommended by the International Commission on Radiological Protection.

It must be emphasized that these findings should not be interpreted to mean that use of the mini c-arm device is without the risk of radiation exposure. Numerous general safety principles for the use of full-sized fluoroscopic devices have been established previously and apply to the mini c-arm devices as well. Taking steps to minimize radiation exposure is in everyone's best interest. This study demonstrates that when used judiciously, the mini c-arm imaging device is safe and convenient and does not expose either surgeons or patients to significant levels of radiation as set out in the guidelines of the International Commission on Radiological Protection.

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