GUIDELINES

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Letters submitted should pose a specific question that clarifies a point that either was not made in the article or was unclear, and therefore a response from the corresponding author of the article is requested.

Authors will be listed in the order in which they appear in the submission. Letters should be submitted electronically via PRS' Ankwell, at www.editorialmanager.com/prs/.

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Letters

Aesthetic and Oncologic Outcome after Microsurgical Reconstruction of Complex Scalp and Forehead Defects after Malignant Tumor Resection: An Algorithm for Treatment

Sir:

I read with great interest the recent article by Dr. van Driel and colleagues in which a "three-step" algorithm for improving the aesthetic outcome of scalp and forehead reconstruction is proposed.[1] Steps I and II address the dura and skull. Step III addresses soft-tissue coverage, matching "preferred" free flaps to recipient sites based on location and skin type. Step III does not address absolute defect size in the way that step II does for bone. In my experience, this is an equally important consideration in flap selection. The authors recommend muscle flaps (latissimus dorsi, rectus) for scalp defects and fasciocutaneous flaps (anterolateral thigh, forearm, scapula) for the forehead. They report the long-term appearance of muscle on the forehead as "skeletonized." Regarding the clinical results, case 1 (latissimus to scalp) has an excellent contour. Case 3 (anterolateral thigh to forehead) suggests that the forehead has less tolerance for the contour discrepancy produced even by a perfectly inset thin anterolateral thigh flap. As the algorithm suggests, this patient might have also done quite well with a skin-grafted latissimus.

The algorithm suggested the forearm flap for forehead reconstruction. In my experience, the forearm flap is also an excellent option for small scalp defects, matching size, contour, and skin type (especially in bald men). Because this donor site is not without morbidity, I reserve this flap for defects up to 5 cm (the width of the volar wrist), regardless of location. An alternative that "bridges the gap" between the latissimus and forearm is the serratus. This flap is useful for medium defects (those whose size might be amenable to a scapula flap) and shares common advantages with both flaps. Like the latissimus, it is a thin muscle with a long pedicle, especially if harvested to the level of the subscapular artery. Its size can be tailored to the defect, depending on the number of muscle slips used. It delivers coverage without bulk and, compared with either flap, has significantly less donor-site morbidity.

I took particular interest in the authors' preference for the temple over the neck as the anastomotic site. Although the superficial temporal vessels may be smaller than those of the proximal external carotid and jugular, they have the superior location. The proximity of the superficial temporal artery allows the flap to be placed higher up on the scalp, increasing the use of the more reliable proximal flap instead of the distal flap, and decreasing the possibility of partial flap necrosis. This is significant when resurfacing large scalp defects with muscle flaps, whose distal "watershed" areas are prone to necrosis.

A recent case demonstrates the ability of the forearm flap to match size, contour, and skin type in the scalp. A 65-year-old man presented with recurrent melanoma involving the parietooccipital skull despite wide resection and radiation. Reexcision and craniectomy created a 5-cm composite defect with dural exposure. Reconstruction was performed with titanium mesh cranioplasty and a forearm flap anastomosed to the superficial temporal vessels. Six weeks later, the contour and skin type matched the hairless scalp (Fig. 1).

DOI: 10.1097/PRS.0b013e31821745d8

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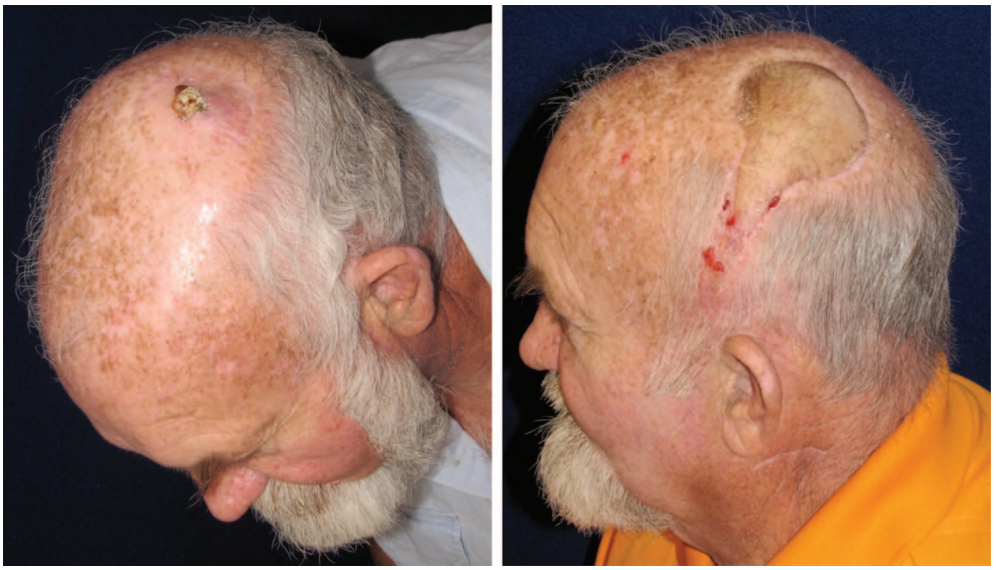

Fig. 1. (*Left*) Recurrent ulcerating melanoma involving the parietooccipital skull. The primary reconstruction was a skin graft. (*Right*) Postoperative appearance following reexcision, craniectomy, cranioplasty, and forearm flap anastomosed in the temple.

PATIENT CONSENT

The patient provided written consent for the use of his images.

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The Anatomy of the Greater Occipital Nerve: Part II. Compression Point Topography

Sir:

In this article, the authors dissected the posterior neck and scalp of 25 fresh cadaveric heads and stated that there are six compression points along the greater occipital nerve.[1] All points of compression of the nerve were found, measured, photographed, and noted. This finding seems to be very interesting and useful information for clinical application.

However, Janis et al. did not mention the criteria used for assessing the compression of the nerve. To determine the criteria, for example, they should have showed us the result of measuring the power to release the compression by tensiometry. Also, the authors did not state which compression points were compressed more than the other points.

Regarding the location of compression, the authors mentioned only mean values of anatomical locations of six compression points. I would like to know the location range of all of the compression points. I hope that the authors can provide the measurement data, because knowledge of the range of each compression point and the ability to compare the degree of symmetry between the two corresponding points will be very valuable.

DOI: 10.1097/PRS.0b013e3182173d78

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Reply: The Anatomy of the Greater Occipital Nerve: Part II. Compression Point Topography

Sir:

I greatly appreciate the inquiry by Dr. Kim regarding the six points of potential compression of the greater occipital nerve described in the articles from 2010.[1,2] Regarding the question of definitively determining the compression of this nerve, this was attempted in 2004 in the original study by measuring the diameter of the nerve proximal and distal to the site of anticipated compression at its emergence from the muscle.[3] Although a trend was found toward a reduction in nerve width as it emerged through the semispinalis, it was not statistically significant. Nonetheless, subsequent clinical studies have proven that chemodenervation of the semispinalis at this point and surgical decompression based on these anatomical data have demonstrated success, lending indirect validity to this anatomical

observation.[4–8] In the live patient, I have seen nerves suspicious for chronic compression morphologically appear yellow-brown with a paucity of fine capillary patterns on their surfaces, much the same as that noted by Ducic et al.[9] In the cadaver, even though fresh, these findings were not observed because of postmortem changes. As with other descriptions of nerve compressions, the studies by my colleagues and me were meant to describe anatomical points where either muscle, fascia, bone, or vessel interacts with the nerve in such a way as to be able to possibly compress, entrap, or irritate the nerve. The real proof of compression is through the reported clinical outcomes of the patients after decompression of these sites, more so than any in vitro test that may be performed in a cadaver. The ranges, means, and standard deviations for the six compression points are provided below:

Point 1:

Range, 12 to 30 mm from the midline; mean, 20.13 mm from the midline (SD, 6.36 mm).
Range, 59 to 104 mm from occipital line; mean, 77.38 mm from occipital line (SD, 16.64 mm).

Point 2:

Range, 8 to 29 mm from the midline; mean, 17.46 mm from the midline (SD, 7.03 mm).
Range, 41 to 77 mm from the occipital line; mean, 59.71 mm from the occipital line (SD, 11.23 mm).

Point 3:

Range, 8 to 21 mm from the midline; mean, 15.52 mm from the midline (SD, 3.74 mm).
Range, 11 to 46 mm from the occipital line; mean, 34.52 mm from the occipital line (SD, 7.50 mm).

Point 4:

Range, 16 to 38 mm from the midline; mean, 24 mm from the midline (SD, 9.66 mm).
Range, 13.5 to 35 mm from the occipital line; mean of 21 mm from the occipital line (SD, 9.91 mm).

Point 5:

Range, 31 to 53 mm from the midline; mean, 37.07 mm from the midline (SD, 7.86 mm).
Range, 0 to 9 mm from the occipital line; mean, 4.36 mm from the occipital line (SD, 4.07 mm).

Point 6:

Helical length, 16 to 68.5 mm.

Range of starting distances from the midline (X):

Helical, 14 to 37.5 mm.
Cross point, 24 to 43 mm.

Range of starting distances from the nuchal line (Y):

Helical, 4 to 59 mm.
Cross point, 11 to 37 mm.

Range of ending distances from the midline (X):

Helical, 21 to 72 mm.
Cross point, 24 to 43 mm.

Range of ending distances from the nuchal line (Y):

Helical, 0 to 74 mm.
Cross point, 11 to 37 mm.

DOI: 10.1097/PRS.0b013e318218fd86

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A Four-Type Classification System for Microvascular Reconstruction of Oncologic Midface Defects: But What about Maxillofacial Allotransplantation?

Sir:

I read with great interest the insightful article entitled "Microvascular Reconstruction of Oncologic Defects of the Midface" published by McCarthy and Cordeiro in December of 2010 (*Plast Reconstr Surg.* 2010;126:1947–1959). In this article, the authors describe a novel four-type classification whose purpose is to help guide reconstructive plastic surgeons challenged with complex

maxillectomy defects. In their vast experience, an algorithm based on the extent of oncologic resection, in combination with the total number of walls involved, associated soft-tissue deficits, and critical structures included (i.e., oral commissure, eyelids), has been most useful.

However, I feel compelled to suggest some modifications to this valuable classification system as a team member and project coordinator for the world's first face and maxilla transplant performed at the Cleveland Clinic in December of 2008 in Cleveland, Ohio.[1] As many of us know, the promising subspecialty of composite tissue allotransplantation, particularly maxillofacial allotransplantation, may soon become a clinical standard.[2,3] In fact, since our team identified the inaugural candidate in July of 2008, I have remained dedicated to Le Fort III–based maxillofacial allotransplantation and have been performing mock cadaver transplants rigorously for the past 20 months (Figs. 1 and 2).

Although the indications are rare, patients with bilateral orbitomaxillectomy defects arising from either "oncologic resection, trauma, or congenital disease," as described by McCarthy et al., may deserve consideration. Of course, committing these patients to lifelong immunotherapy should not be taken lightly. I am proposing that they modify their classification system. First, according to the photographs presented, a majority had unilateral defects. By contrast, on rare occasion, some patients present with bilateral maxillary defects accompanied by devastating soft-tissue injuries. Thus, it would be extremely helpful for the sake of future studies if the authors could adjust their system to specify whether the maxillectomy defect(s) are unilateral or bilateral.

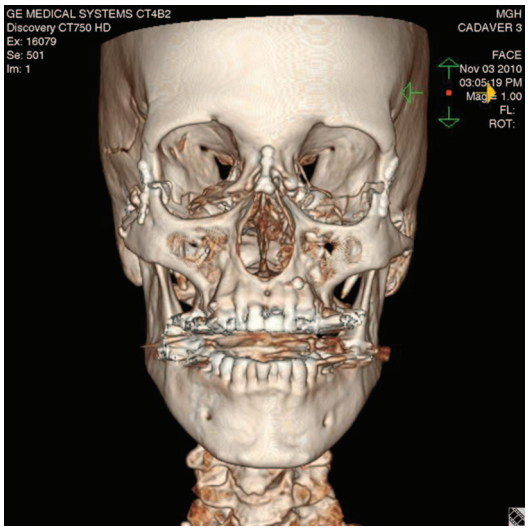

Fig. 2. Three-dimensional computed tomographic scan reconstruction of a mock Le Fort III–based maxillofacial allotransplant recently performed at the Massachusetts General Hospital/Harvard Medical School.

Second, I believe a fifth group could be added to accommodate for the rare instance of maxillofacial

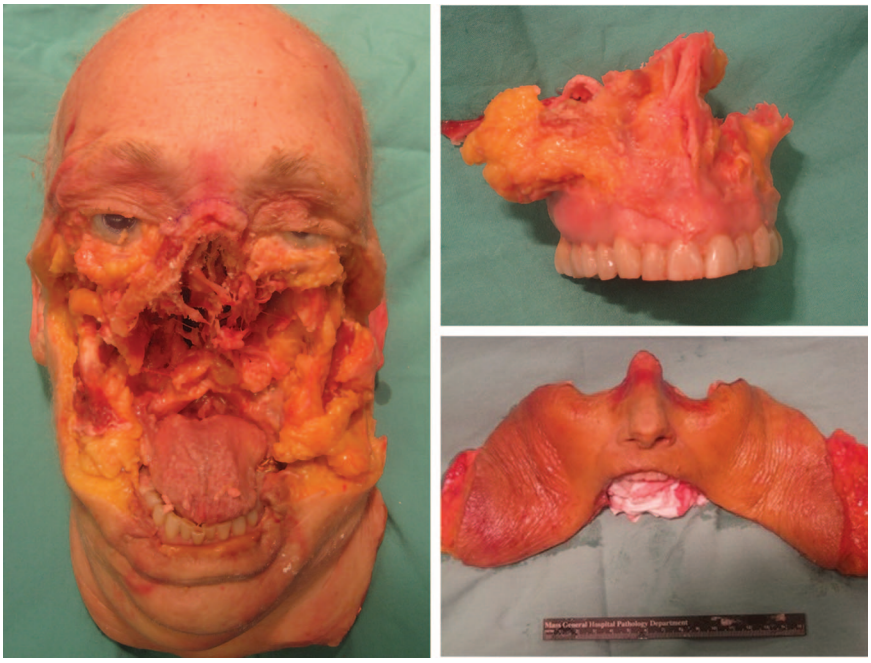

Fig. 1. Photographs demonstrate a proposed "type V" bilateral naso-orbitomaxillectomy defect with an extremely large surface area and volume (*left*), a bilateral maxillectomy specimen (*above*, *right*), and a customized maxillofacial alloflap for necessary reconstruction based on bilateral external carotid artery/internal jugular vein pedicles (*below*, *right*).

allotransplantation.[4] This fifth type could encompass "naso-orbitomaxillectomy defects with extremely large surface area and extremely large volume." As the authors eloquently described, "the morbidity associated with maxillectomy is rarely trivial and potentially includes impairment of deglutination, nutrition, vision, speech, facial appearance, and social acceptability"; and there is no greater example of this than the improvements found in maxillofacial transplant patients postoperatively.[5]

Also, I would like to reinforce the advantages of maxillofacial allotransplantation as compared with standard autologous methods for the proposed type V defects. Although the authors, using free tissue transfer, have elegantly provided autologous tissue available for obliterating dead space and have replaced "like with like," there is no possibility that they can match the intricate details of a complete nose following rhinectomy and a functional upper lip, and simulate mimetic musculature influencing "one's unique appearance" as compared with maxillofacial allotransplantation. However, again, solutions to complications related to immunosuppression and the quest to obtain "donor-specific tolerance" have to date eluded us. Nevertheless, there is still a role for this option, and therefore I believe the classification system should be modified accordingly.

DOI: 10.1097/PRS.0b013e3182174373

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DISCLOSURE

The author has no conflicts of interest to report. No sources of external funding were used for this study.

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A 5-Year Assessment of Safety and Aesthetic Results after Facial Soft-Tissue Augmentation with Polyacrylamide Hydrogel (Aquamid): A Prospective Multicenter Study of 251 Patients

Sir:

We read with great interest the article entitled "A 5-Year Assessment of Safety and Aesthetic Results after Facial Soft-Tissue Augmentation with Polyacrylamide Hydrogel (Aquamid): A Prospective Multicenter Study of 251 Patients" by Pallua and Wolter[1] and would like to congratulate the authors on their impressive work.

The rate of complications caused by polyacrylamide hydrogel injection seems to be lower than the rates associated with other products regarding its hydrophilic nature.[1] However, there are some severe complications after hydrogel injection that should not be overlooked even though the rate is low. In a previous study, we retrospectively reviewed the complications of soft-tissue fillers (including polyacrylamide hydrogel) that we treated in our department. The complications that we have observed were migration, redness, palpable induration, and fever.[2] Unfortunately, all of these complications were serious enough to necessitate an operation for treatment. Some of these patients received additional injections with some other fillers after the first injection with polyacrylamide hydrogel; thus, it was difficult to identify whether the complications were caused by polyacrylamide hydrogel or the other material injected, as pointed out by Pallua and Wolter.[1]

There are two points that we want to further emphasize in this letter. First, even though it is biocompatible, injection of polyacrylamide gel may cause immunologic reactions, sometimes as a late-onset complication. Fernández-Cossío and Castaño-Oreja reported that polyacrylamide hydrogel induces a prolonged inflammatory reaction in murine tissues that would make removal of the implant difficult if that became necessary.[3] This prolonged inflammatory reaction may be the background of late immunologic reactions to polyacrylamide hydrogel. Alijotas-Reig et al. also reported delayed immune-mediated adverse effects related to polyacrylamide dermal fillers.[4] The complications that they have observed were painful inflammatory nodules, pseudoabscesses, and severe localized or generalized facial edema. Microorganisms were yielded in only one case, with negative results in the other cases, documenting the immunologic, noninfectious nature of the complications.

The second point that we want to emphasize in this letter is that polyacrylamide hydrogel injection should be used with appropriate informed consent, and long-term follow-up should be performed carefully, as both we and the authors have previously stated.[1,2] Patients should be provided with detailed information about the possible complications to give them the opportunity to avoid the complication risk themselves because it is difficult to treat complications once they occur. Unfortunately, we observed that that was not done appropriately in some

cases.[2] We strongly agree with the authors that the frequency of complications will definitely be lower if operations are carried out under appropriate conditions with an appropriate product and technique. Nevertheless, nonphysicians or even patients themselves sometimes self-inject fillers under inappropriate conditions because the injection of soft-tissue fillers seems to be an easy method, although this is not true. Moreover, in our clinical practice, we have observed that use of polyacrylamide hydrogel in the lower eyelid and nasal dorsum leads to a higher incidence of complications; therefore, injection into these areas should be avoided.[2]

In conclusion, scientific and ethical use of nonabsorbable fillers by aesthetic surgeons is mandatory to prevent serious complications. We hope these above comments on the authors' valuable current study will help to improve the results of polyacrylamide hydrogel injections.
DOI: 10.1097/PRS.0b013e3182173e3e

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Reply: A 5-Year Assessment of Safety and Aesthetic Results after Facial Soft-Tissue Augmentation with Polyacrylamide Hydrogel (Aquamid): A Prospective Multicenter Study of 251 Patients

Sir:

We read the letter by Orbay et al. with great interest and appreciate the valuable input on complications associated with this filler material. We strongly agree with their conclusion that injection of a permanent filler should be performed only after appropriate informed consent and by a qualified physician who has had proper training. Regular and long-term follow-up is essential to treat possible complications early and successfully.

Orbay et al. raise the question about immunologic reactions. Any foreign substance, including polyacrylamide hydrogel, will elicit an initial foreign body response. The continuous bioactivity of Aquamid described by Fernandez-Cossio et al. has also been researched in a rabbit model by Bello et al.[1] In contrast, the latter have found an initial tissue response with a gradual decrease over time, resembling wound repair in the long run. In humans, moderate tissue integration preventing movement of the material has been described. In clinical practice, we have found that removal of polyacrylamide hydrogel is easy even after several years.[2] This is in accordance with the description by Ono et al.[3] of removing the gel in complications through a small incision.

Granuloma formation is a dreaded complication after any filler injection. However, the widely used term "granuloma" is nonspecific and only characterizes the presence of macrophages in a nodule. With polyacrylamide hydrogel, there are conflicting results regarding whether the macrophages are attracted by the injected substance itself, as implied by Alijotas-Reig et al., or by a bacterial colonization (biofilm) that is injected with the substance. Several studies have shown bacterial colonization in all clinically significant granulomas after injection of Aquamid.[4] To prevent or reduce such biofilm formation, sterile manufacturing, handling, and injection are of utmost importance. Interestingly, the role of viral contamination has not been studied yet.

Another important issue is that fillers based on the same biochemical compound may have different complication rates. In the patient group reported by Ono et al.,[3] the gel is manufactured by two different companies and used for differing indications. Of the 15 cases described, six were injected with Aquamid, and five of these cases resolved with conservative treatment. All of these patients were seen in a clinic specializing in complication management. Therefore, no percentage of adverse events can be deduced. In our study, which included only patients injected with Aquamid, we found two severe adverse events among 251 patients, both of which resolved during the study period.[5] In conclusion, Aquamid injection is a safe procedure if used for the correct indication and with the adequate injection technique.

In contrast to permanent fillers, absorbable filler injections need to be repeated on a regular basis. Even in light of economic issues, the use of permanent fillers should not be precluded, because fewer injection sessions reduce the risk of biofilm infection and therefore can contribute to patient safety. However, the authors make a valuable point in reminding us that we constantly need to reevaluate our tech-

niques and materials to achieve an optimal result and prevent complications.
DOI: 10.1097/PRS.0b013e3182173e9f

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DISCLOSURE

The authors have no financial interest to declare in relation to the content of this communication or of the associated article.

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Reconstruction after Partial Hypopharyngectomy with Larynx Preservation

Sir:

We would like to comment on the reconstructive plans described by Soares et al. in their article entitled "Reconstruction of the Posterior Pharyngeal Wall with a Deltopectoralis Flap in One-Step Surgical Intervention with Larynx Preservation" (*Plast Reconstr Surg.* 2010;126:143e–144e). Although the authors reported two successful cases, a partially deepithelialized deltopectoralis flap is generally not suitable for reconstruction of partial hypopharyngeal defects with larynx preservation, for two reasons. First, one-stage reconstruction using a pedicled flap has the risk of downward traction caused by postoperative scar contracture. This inhibits the physiologic elevation of the larynx and diminishes swallowing function.[1] Second, unstable marginal circulation of the flap leads to a high rate of fistula formation.[2] In particular, the border between the deepithelialized and nondeepithelialized areas has a strong tendency to break down and develop a fistula.

As the authors described in their report, free tissue transfer represents the optimal reconstructive option for partial hypopharyngeal defects with larynx preservation.[1,3–5] A free jejunum patch graft is the best treatment for posterior wall hypopharyngeal defects (Figs. 1 and 2),[1,4] because free jejunum has excellent wound healing properties and is associated with a low fistula rate. Postoperative swallowing function is also better with a jejunal flap, rather than a cutaneous flap, because its lubricated surface permits the smooth passage of food.[1]

Partial hypopharyngectomy with larynx preservation requires thorough knowledge of the anatomy and should be performed only by experienced surgeons. Furthermore, microsurgical skills are essential, because there is always the possibility, depending on the extent of the disease, that the larynx cannot be preserved and the procedure might need to be converted intraoper-

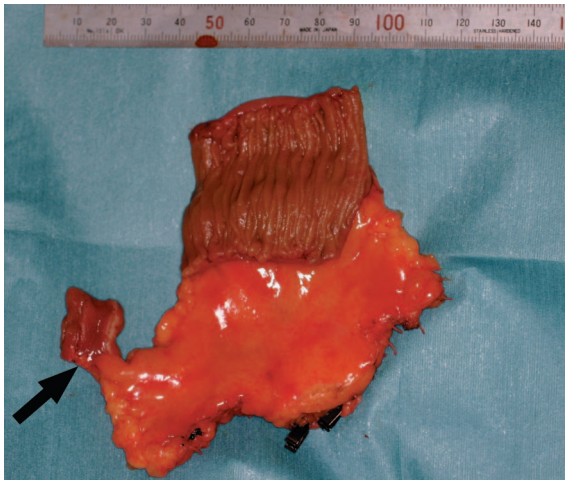

Fig. 1. The jejunum patch graft after prefabrication. The *arrow* indicates the segment to be exteriorized for postoperative monitoring.

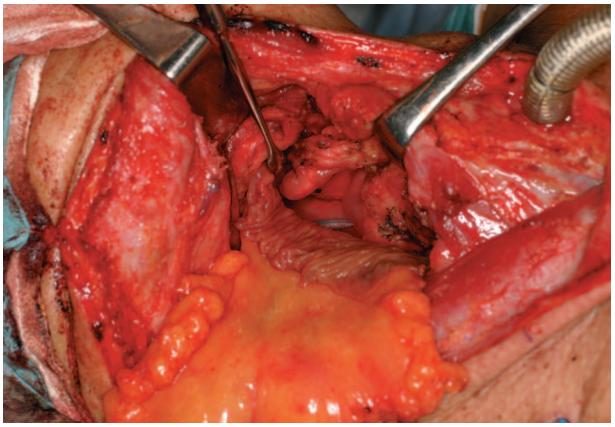

Fig. 2. Intraoperative view after the jejunum has been sutured to the posterior margin of the defect.

atively to total pharyngolaryngectomy. The operation should not be performed in a facility that cannot meet these demands.
DOI: 10.1097/PRS.0b013e318217464d

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DISCLOSURE

The authors have no financial interest to declare in relation to the content of this communication or of the associated article.

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Reply: Reconstruction after Partial Hypopharyngectomy with Larynx Preservation

Sir:

Patient follow-up has shown that the question of whether the deltopectoral flap is unsuitable for reconstruction of the hypopharynx because of the required retraction of the flap and the resultant limited elevation of the larynx represents a theoretical concern rather than a practical one. Both patients recovered their swallowing function and were able to consume foods of all consistencies within a short time. One of these patients is currently alive and is disease free, with completely intact swallowing and speech function. We operated on a third patient 3 months ago; this patient underwent the same reconstruction and is experiencing favorable outcomes, similar to those of the previous two patients.

The risk of fistula exists, and this complication occurred in one of the cases. However, the fistula was repaired by resuturing the skin of the pharyngeal mucosa; there were no major complications, and additional flaps were not required. We agree that microsurgical flaps are excellent for repairing defects that result from large tissue resections, and microsurgical flap procedures have been used for several years at our hospital. However, this reconstructive technique cannot be performed in many head and neck surgery clinics in Brazil and also in many other countries around the world. Therefore, other options for surgical reconstruction are required because the unavailability of the microsurgical flap reconstruction procedure should not prevent the surgical treatment of patients. However, a critical question arises: Does the jejunum remain lubricated after radiotherapy? We believe that the answer to this question is because some of these patients might require adjuvant radiotherapy treatment, and the level of lubrication depends on the extent of the resultant actinic damage to the jejunal mucosa.

We agree that during the course of the procedure, it may be determined that a laryngectomy is required. If this occurs, the posterior wall of the hypopharynx is reconstructed as part of the formation of a new pharynx using the pectoralis major muscle flap that is anchored to the prevertebral fascia.[1]

We also agree that such resections should be performed only by experienced surgeons with extensive knowledge of anatomy and physiology; therefore, these procedures are performed in our department.
DOI: 10.1097/PRS.0b013e3182173ec5

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Decolonization Strategies to Control *Staphylococcus aureus* Infections in Breast Implant Surgery

Sir:

We read with interest the article by Feldman et al., which showed that the majority of periprosthetic infections after breast implant surgery were caused by *Staphylococcus aureus*, mainly methicillin-resistant *S. aureus*.[1] The authors concluded that an antibiotic with

anti–methicillin-resistant *S. aureus* activity is justified as empiric therapy until sensitivity data of cultures are available. We would like to comment on these conclusions, as the authors ignored the role of decolonization strategies in *S. aureus* infection control.

Most infections are endogenous (i.e., caused by potential pathogens carried by the patient in the nose, throat, and gut). Exogenous infections without previous carriage will also occur, but less frequently (15 percent); they are prevented by the use of sterile equipment, high levels of hygiene, and handwashing. Patients carrying *S. aureus*, sensitive or resistant to methicillin, are more likely to have infections attributable to this microorganism than the noncarriers. Consequently, eradication of the carrier state would seem a rational strategy for controlling *S. aureus* infections, and can be achieved by the use of antimicrobials and/or antiseptics.

High doses of cephradine (100 mg/kg/day) have been shown to eradicate the carrier state of methicillin-sensitive *S. aureus*.[2] Oral cephradine should be administered 3 days before surgery and continued parenterally for 3 days after surgery. A meta-analysis of four randomized trials using mupirocin in surgical patients carrying *S. aureus*, both sensitive and resistant to methicillin, demonstrated a significant reduction in *S. aureus* infection rate after surgery (relative risk, 0.55; 95 percent confidence interval, 0.34 to 0.89; $p = 0.02$), whereas the reduction of surgical-site infections was not significant, probably because of a lack of statistical power (relative risk, 0.64; 95 percent confidence interval, 0.38 to 1.06).[3] In a recent randomized trial, rapid detection of *S. aureus* carriage followed by decolonization of nasal and extranasal body sites with mupirocin plus chlorhexidine significantly reduced *S. aureus* deep surgical-site infection by 80 percent.[4] Finally, enteral vancomycin has been demonstrated to eradicate methicillin-resistant *S. aureus* carriage, both oropharyngeal and intestinal, and to reduce methicillin-resistant *S. aureus* infection of the lower airways and outbreaks in critically ill patients and burns.[5]

Given the prevalence of methicillin-resistant *S. aureus* infections in their study, the authors recommended the use of antibiotics active against this microorganism, "possibly prophylactically and definitely empirically," such as oral cotrimoxazole in case of mild infections, intravenous vancomycin, or even daptomycin for more severe infections. We would prefer to prevent these infections by eradicating methicillin-resistant *S. aureus* carriage preoperatively through decolonization strategies, rather than waiting for the infection and treating it with parenteral antibiotics. All patients scheduled for breast implantation surgery should be screened for carriage of this microorganism (e.g., nose, throat, and rectal swabs) preoperatively. Subsequently, it should be eradicated using enteral vancomycin, or nasal mupirocin, or chlorhexidine.

We believe the time has come to extend the vast body of experience on decolonization strategies to mammary implant surgery, especially in patients who undergo reconstructive implantation. This could prompt the authors to embark on a new project with prevention of methicillin-resistant *S. aureus* infection in mind.

DOI: 10.1097/PRS.0b013e3182173e51

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DISCLOSURE

The authors have no financial interest to declare in relation to the content of this communication or of the associated article. No external funding was required.

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Retrocapsular Pocket to Correct Symmastia

Sir:

We have noted with interest recently published articles on symmastia correction.[1–5] We have had success in treating a case of symmastia with a further variation of technique.

A 45-year-old patient presented with symmastia and medial rippling. She had undergone several breast augmentations and implant exchanges over

the previous 4 years at another unit. Subglandular breast augmentation was complicated by capsular contracture within 1 year of the initial procedure. Capsulectomy and implant exchange were performed but she had significant rippling. Implants were replaced this time in the submuscular plane. The new implant position was not acceptable to her because of discomfort and distortion; thus, at a fourth operation, the implants were replaced once again back into a subglandular pocket.

To correct the symmastia, a new implant pocket was created behind the posterior wall of the existing capsule. Dissection of the posterior capsule off the chest wall was continued medially to allow adequate placement of the implant, but to leave a zone of undissected capsule in the midline. In this zone, the posterior capsule remained adherent to the chest wall separating the breast mounds, creating an aesthetically acceptable cleavage. This was achieved by dissection of the retrocapsular pocket alone. No internal sutures were required to shape or strengthen the medial limit of the newly created pocket, and a satisfactory outcome was achieved (Figs. 1 and 2). In this instance, the double thickness of capsule overlying the implant anteriorly and medially served also to ameliorate the rippling of the implant.

DOI: 10.1097/PRS.0b013e3182174661

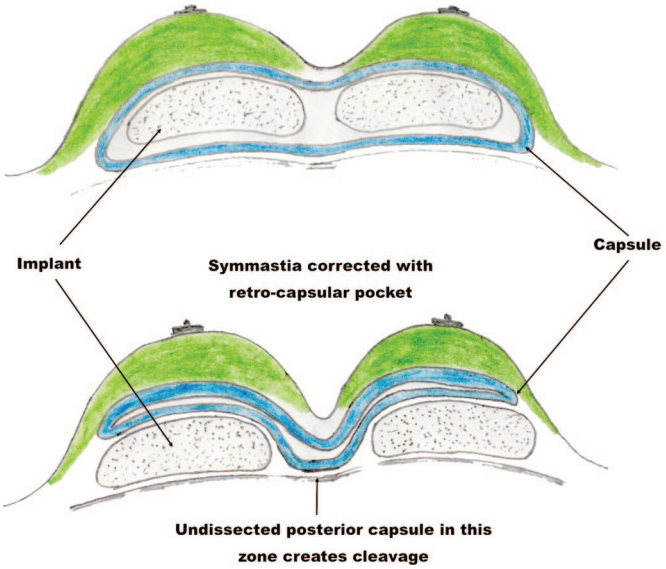

Fig. 1. Cross-sectional diagram illustrating symmastia with left and right breast capsules communicating (*above*). Symmastia is corrected by positioning implants behind the posterior capsule wall, leaving an adequate zone of attachment centrally to create cleavage (*below*).

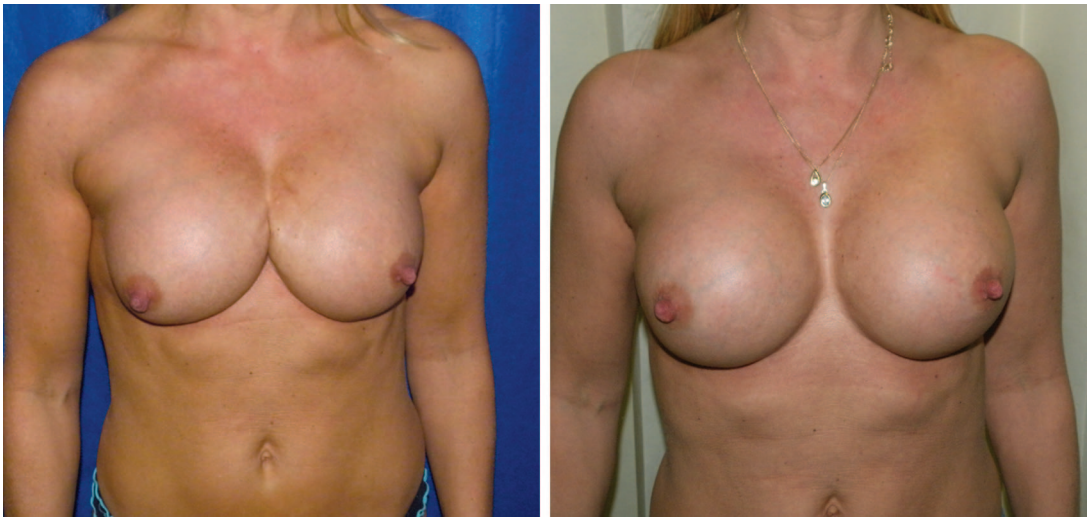

Fig. 2. (*Left*) Preoperative view of symmastia and medial implant rippling. (*Right*) Postoperative result at 2 months showing satisfactory cleavage and no rippling.

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Should Cosmetic Augmentations of Breasts by Autologous Fat Injections No Longer Be Performed Because of Mammographic Confusion?

Sir:

I just read the publish-ahead-of-print article "Clinical Analyses of Clustered Microcalcifications after Autologous Fat Injection for Breast Augmentation" by Wang et al. online and applaud their forthrightness and your effort to alert plastic surgeons to their findings as soon as possible.[1] Both the abstract and the article contain this concluding sentence: "The mammographic confusion constitutes the problem rather than the success of the procedure itself; the method should be prohibited continuously."

This important sentence is, in itself, confusing. Please allow the authors to clarify their conclusion. First, what did they mean by "prohibited"? In the United States, the only prohibitions on cosmetic procedures are the surgeon's understanding of the art and science of plastic surgery, his or her conscience, and the forces of the marketplace. In contrast to late termination of pregnancy, there are no legal prohibitions on cosmetic surgery in the United States. Cosmetic breast augmentation by means of autologous fat injections can be and has been performed in the office setting; thus, there is no possibility of a hospital prohibiting it.

Second, what did they mean by "continuously"? One interpretation of continuously is that patients should not have fat injections performed 24 hours per day, 7 days per week, 365 days per year, but I doubt that is what the authors intended. [*Editor's Note*: The passage quoted by Dr. Freshwater appeared in Advance Online, and had not been copyedited. When the article appeared in print and online as part of our April issue, the text read "should continue to be prohibited."]

I believe that an important message was lost in translation. If the authors are recommending that we should no longer perform cosmetic augmentation of breasts by autologous fat injections because of mammographic confusion, please allow them to say so.

DOI: 10.1097/PRS.0b013e3182173e7a

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Reply: Should Cosmetic Augmentations of Breasts by Autologous Fat Injections No Longer Be Performed Because of Mammographic Confusion?

Sir:

I am grateful for Dr. M. Felix Freshwater's remarks regarding the article. In the article, we said that the method of autologous fat injection for breast augmentation should be prohibited continuously. We meant that the technique should be prohibited by the American Society of Plastic Surgeons and by the surgeons themselves, not by law.

In 1987, the American Society of Plastic and Reconstructive Surgeons Ad-Hoc Committee on New Procedures issued a position article stating the following: "The committee is unanimous in deploring the use of autologous fat injection in breast augmentation."[1] The dispute regarding fat injection for breast augmentation has lasted for the many years since then. Because the American Society of Plastic and Reconstructive Surgeons 1987 article existed, we said "continuously."

In our article, the digitized mammographic films of eight of 48 patients (16.7 percent) showed clustered microcalcifications.[2] The rate is too high, so we believe we should no longer perform autologous fat injection for breast augmentation because of the possibility of the clustered microcalcifications, which can lead to mammographic confusion.

DOI: 10.1097/PRS.0b013e3182173ed8

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Arm Position Artifact in Three-Dimensional Breast Scanning Technique

Sir:

I enjoyed the article by Liu et al. on three-dimensional scanning of breast augmentation patients in the December 2010 issue of the *Journal*.[1] The authors carefully described their method of arm positioning and stated that "The patient was asked to stand upright with her. . . hands on the anterior suprailiac spine [sic]." Other articles on three-dimensional scanning of breasts in this *Journal* and elsewhere did not present as clear a picture of their standardized patient positioning, as they fail to describe the position of patients' arms.[2,3] Nevertheless, from examining the figures in these articles, I believe that the scanning was performed with the patients' arms akimbo.

I am concerned about the possibility that scanning patients with arms akimbo introduces an artifact that impacts the very accuracy of the measurements that we are attempting to achieve. A well-known method for examining the pectoralis major or latissimus dorsi muscles is to have the patient put her hand on her hip and apply pressure. We have all seen patients (either our own or someone else's) who can distort their implants by firing their pectoralis or latissimus muscles. Particularly with submuscular implants, our inability to measure how much pressure patients are applying to their hips could impact the very results that we are attempting to measure.

The American Society of Plastic Surgeons has photographic standards that are based on an article by DiBernardo et al. [4] These standards mandate that the arms be at the sides. With the availability of three-dimensional scanning, we have the opportunity to answer the question of whether there is indeed any measurable difference in breast shape or anatomical landmarks based on arm position. Should the American Society of Plastic Surgeons standards be revised or should data that do not conform to the standards be discarded?

I suggest that those groups with an interest in three-dimensional scanning perform the simple experiment of comparing groups of patients with their arms in three different positions—at the sides, akimbo, and akimbo with maximal force—to discover whether there is a clinically important difference.

DOI: 10.1097/PRS.0b013e3182173f03

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Body Contouring Surgery with the V-loc Suture

Sir:

We read with great interest the article by Shermak, Mallalieu, and Chang on the impact of barbed sutures on wound closure in body contouring surgery.[1] We have used barbed sutures in over 100 body contouring procedures over the past 2 years.

Unlike the authors, whose experience has been with the Quill suture (Quill SRS; Angiotech, Vancouver, British Columbia, Canada), we have used a different barbed suture, the V-loc (Covidien, Dublin, Ireland), in body lifts, abdominoplasty, breast reduction, brachioplasty, and thigh reduction.

Like the authors, we have also had a few complications with wound healing. However, these wound healing problems occurred from our initial experience. We report few cases of wound breakdown, delayed healing, and suture spitting with the use of this absorbable barbed suture.

We believe that these complications occurred at the ends of incision lines. Instead of completing a subcuticular closure with the V-loc by coming out at the end of the incision line, our initial experience was to reach the end of the incision line and suture back for several passes. Once we reverted to completing the subcuticular by coming out at the end of the incision line, our complication rate decreased dramatically. We believe that the other wound healing complications were related to complications such as fat necrosis.

Overall, we have found that the V-loc suture is easy to handle and durable, and that its use for major wound closure has potentially reduced operative time. Furthermore, the scars at 12-month follow-up have been satisfactory.

DOI: 10.1097/PRS.0b013e3182173eb1

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DISCLOSURE

The authors have no financial interest to declare in relation to the content of this communication or of the associated article.

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Reply: Body Contouring Surgery with the V-loc Suture

Sir:

Barbed suture technology is garnering a great deal of interest, and there is no doubt in my mind that the profession is ravenous for more data on this somewhat mysterious technology. Plastic surgeons are innovators by nature, so we continue to explore ways of making our outcomes that much better, both functionally and aesthetically. Evidence-based medicine helps more conservative adopters such as myself in introducing new technology into practice, particularly because of the added expense often associated with more sophisticated products. Barbed suture technology became so compelling for my practice because of the potential for improvement in form and function; expediting surgery; and providing more even, "knotless" tension across wound closures that would theoretically improve the scars. Objective assessment of outcomes is what stimulated my interest in looking at this group of patients: was there an actual benefit or not?

My experience with the Quill SRS 0 polydioxanone absorbable suture in body contouring surgery demonstrated that this is a technology with definite potential, but I did experience some problems with the relatively prolonged absorption period of 180 days. Just like you, it was exposure of the suture to the environment or to a deep dead space and the associated inflammatory response that led to problems, more so in the arm than in other body regions. I similarly modified my technique to not backtrack at the end of the suture so much and found some improvement: it did seem like a double layer of the suture exacerbated inflammation and spitting. If one part of the suture became exposed, the process would wick across the suture, and skip areas of redness and exposure sometimes resulted. This typically resolved with suture removal, but did cause some anxiety and unhappiness on the part of the patients who had to deal with this.

I have adopted the V-loc 3-0 and 4-0 sutures for approximation of the dermis, a more superficial layer than that applied for the Quill 0 polydioxanone suture. I have been very happy with the scar results in the procedures I have performed. I have used the suture for breast, abdomen, back, and arm surgery. Two patients had an inflammatory process that was easily quelled with suture removal, one in the abdomen at the end of the incision and one in the mid arm incision. I have not yet tried the V-loc larger caliber suture for Scarpa fascia approximation, but I am looking forward to using it. It sounds like you are using the V-loc primarily for dermal closure. Across long incisions under tension, I am still placing absorbable 3-0 monofilament buried dermal interrupted sutures deep to the V-loc running intracuticular suture, though they are fewer in number and there is a greater span between the 3-0 sutures, so I am not sure we are saving a significant amount of time. The scars do seem better than those resulting from traditional closure. I have not formally studied this yet. I thank you for your interest in my group's article and, like you, look forward to seeing more scientific study on barbed suture technology, investigating different calibers, barb lengths and angles, and layers of closure in body contouring surgery.

DOI: 10.1097/PRS.0b013e3182173eea

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The Nomenclature of Perforator Flaps

Sir:

We read with great interest the recent article by Sinna et al. in *Plastic and Reconstructive Surgery*, which sought to present some clarity to the confusion surrounding the nomenclature of perforator flaps.[1] The article raises the pertinent issue of nomenclature in perforator flaps. The semantics in defining a perforator flap have certainly been a topic of great discussion in the literature, with some authors suggesting that increasingly refined classification systems are of value and others suggesting that these classification systems add unnecessary complexity to similar procedures.[2–4] The authors offer a new classification system, and although there is certainly value in analyzing the vascular anatomy of differing perforator flaps, we would like to caution against the use of new terminologies, particularly as so many classification systems already exist for perforator flaps. This is evidenced by the fact that the same authors, Sinna et al., published a series of "perforator flaps" for perineal reconstruction in a recent edition of the *Journal of Plastic, Reconstructive & Aesthetic Surgery*,[5] in which only two to three of the nine flap options shown in their decision tree (and none of the clinical photographs presented) were perforator flaps by any of the definitions proposed throughout the literature, and most constituted musculocutaneous flaps, which are not perforator flaps by any definition.[1–4]

The authors also comment on the term "free-style perforator flap," which certainly has been a widely used term to describe the ability to dissect an "unnamed" perforator and design a flap on a "random" perforator. With the advent of preoperative imaging, we feel that many such flaps may not truly be free-style, and this term may yet become outdated. With the use of computed tomographic angiography, a defect can be visualized in three dimensions on imaging, and the surrounding vascular

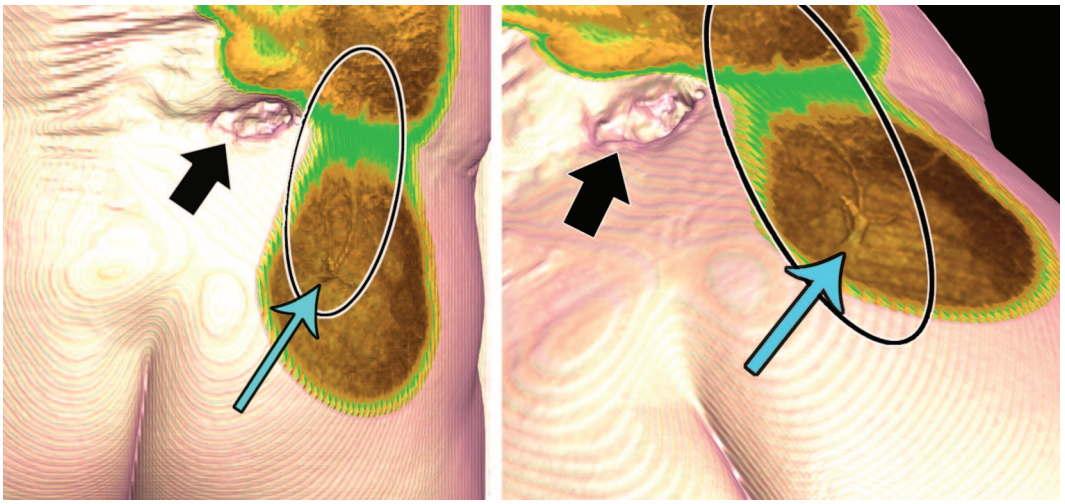

Fig. 1. Computed tomographic angiograms of the trunk demonstrating a lumbar defect after surgical resection of a large infiltrating basal cell carcinoma (*blue arrow*), for which reconstruction with a local perforator flap was planned. A large perforator is shown emerging from the gluteus maximus musculature and found to arise from the superior gluteal artery on three-dimensional reconstructions. This perforator emerged from the gluteal fascia 6 cm distal and 2 cm lateral to the margin of the defect (*black arrow*). A superior gluteal artery perforator flap was thus designed as a local island transposition flap, with its subcutaneous course mapped to plan an axial flap, shown with longitudinal (*left*) and oblique views (*right*).

anatomy can be highlighted to a degree to which the subcutaneous course of a perforator can be planned to run axially along a flap, and the perforator can be traced to its source, named regional origin on imaging. As shown in Figure 1, an "axial" local perforator flap can be planned preoperatively on a perforator immediately adjacent to the defect (Fig. 1). The flap is thus an axial flap and based on a perforator that is shown to originate from the superior gluteal artery.

The review by Sinna et al. nicely highlights the changes in terminology since the term "perforator flap" was introduced in 1989 and demonstrates that the nomenclature is far from ratified. With the use of preoperative imaging, new surgical approaches, and refinements in our appreciation of microvascular anatomy, perforator nomenclature will no doubt continue to evolve.

DOI: 10.1097/PRS.0b013e3182174515

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DISCLOSURE

The authors declare that there is no source of financial or other support or any financial or professional relationships that might pose a competing interest.

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Reply: The Nomenclature of Perforator Flaps

Sir:

I read with great interest the letter by Behrenbruch et al.[1] concerning my article entitled "What Should Define a 'Perforator Flap'?" and appreciate the caution that should be taken before introducing a new nomenclature. I agree with their view and, as my colleagues and I concluded in our article, "We do not think that this classification should be the one and unique."[2]

However, as highlighted in the title, more than the nomenclature, it is the concept of the perforator flap

that we wanted to discuss. As the authors said, there is a lot of discussion on what should be a perforator flap. We believe that, more than anatomical details of what crosses the perforator vessel before arising in the subcutaneous tissue, the real shift of paradigm of perforator flaps is the understanding that a tiny vessel, independent of its origin, is able to vascularize a large cutaneous flap without any underlying muscle or aponeurosis. Therefore, as explained in the article, we believe that "As far as the perforator vessel has been identified, dissected free of neighboring tissue and the skin paddle islanded, or not on it, the flap can be defined as a perforator flap."[2]

This shift of paradigm was illustrated in our article "Perforator Flap: A New Option in Perineal Reconstruction."[3] This article is not, as suggest by Behrenbruch et al., a series of perforator flaps but a review article that highlights how the classic options for perineal reconstruction can be customized to diminish the morbidity of the donor site by understanding the perforator flap concept. To illustrate this, we had mentioned the muscle-sparing or fascia-sparing techniques as examples, although they are not perforator flaps. Our detailed clinical series of perforator flaps in abdominoperineal reconstruction, in the prone position, is about to be published.

However, the main problem that remains is the communication to allow sharing of knowledge in a reproducible manner. Therefore, "we believe that before acceptance of any oral communication abstract or articles dealing with perforator flaps, reviewers should be sure that every element necessary to understand the surgical and anatomical description is present in the articles."[2] Furthermore, if the authors can now trace the perforator precisely with computed tomographic angiography, even in the "free-style flaps,"[1] we believe that these surgical details (origin, extent of vascular dissection, muscle involved, and type of perforator) should be available and shared in their future articles, maybe with our nomenclature.

DOI: 10.1097/PRS.0b013e3182173e63

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DISCLOSURE

The author declares that there is no source of financial or other support or any financial or professional relationships that might pose a competing interest.

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A Prospective Trial on the Use of Antibiotics in Hand Surgery

Sir:

We would like to commend the authors for this excellent prospective study aiming to identify whether the use of prophylactic antibiotics in hand surgery improves clinical outcomes.[1] With the seemingly ubiquitous use of antibiotics in the United States, this study has tremendous implications for their role in surgery. To be sure, there are six operations (i.e., vascular, cardiac, colon, hip/knee arthroplasty, hysterectomy, and coronary artery bypass grafting) where there is evidence that prophylactic antibiotics do carry clear benefits or that a randomized controlled trial is too risky to carry out. Not surprisingly, one of the performance measures used by the Centers for Medicare & Medicaid Services to evaluate hospitals is whether or not prophylactic antibiotics are given in these specific cases. Higher reimbursement rates are provided for those hospitals that comply.[2] However, we believe, at least in our institution with a study in progress by the senior author, that such financial implications have led to overuse of prophylactic antibiotics in cases where there is no proven benefit. One could argue that this overuse may even be harmful to the patient. Thus, the debate among surgeons over the use of prophylactic antibiotics has obvious clinical, economic, and policy implications.

Certainly, the impetus for administering prophylactic antibiotics is multifactorial and includes the financial incentives created by the Centers for Medicare & Medicaid Services National Voluntary Hospital Reporting Initiative and National Quality Data Project. First, on a superficial level, antibiotics fight infection; thus, giving antibiotics a priori would certainly decrease, if not eliminate, the likelihood of postoperative infection.[3] Second, practicing medicine in our current medicolegal climate drives defensive medicine that can be implicated in the overuse of antibiotics. Third, the harm of prophylactic antibiotic use does not garner as much attention as is deserved. The misuse and overuse of antibiotics can lead to pseudomembranous colitis, fungal infections, breeding of multidrug-resistant organisms, and allergic reactions. This does not speak to the health care costs associated with such behavioral patterns that often neglect evidence-based support.

In reference to plastic surgery, our specialty's preponderant use of prophylactic antibiotics has been documented. Krizek et al. analyzed 1025 and 1718 questionnaires in 1974 and 1985, respectively, examining antibiotic use in seven major categories: con-

genital anomalies, aesthetic, hand, head and neck, maxillofacial, burns, and miscellaneous.[4] They noted that there was an increase in use of prophylactic antibiotics without any evidence that their administration decreased the rate of perioperative infection. Lyle et al. performed a similar study in 2003, analyzing 1804 surveys and finding a 100 percent increase in the use of antibiotics in breast reduction, suction-assisted lipoplasty, thigh lift, buttock lift, abdominoplasty, septum rhinoplasty, and chemical peel as compared with the 1985 survey.[5] This finding paled in comparison to the 200 percent increase for blepharoplasty, rhytidectomy, rhinoplasty, and arm contouring on comparison for the same years. Admittedly, these studies have some methodologic limitations but overall have done a magnificent job of highlighting that plastic surgeons are increasingly using prophylactic antibiotics under a wider variety of circumstances without referring to the literature to determine the types of operations (particularly aesthetic surgery) for which prophylactic antibiotics decrease postoperative infection.

Therefore, the surgical community must strive harder to continue practicing evidence-based medicine using studies such as the one discussed to tailor our use of prophylactic antibiotics. Although Aydin et al. demonstrate that prophylactic antibiotics do not provide any superior outcomes in prevention of infection in hand surgery, we believe the authors should clarify the number of patients in the placebo group. Specifically, the text refers to the placebo group consisting of $n = 647$, whereas Table 3 lists the placebo group as consisting of $n = 447$. It seems that this is most likely a typographical error, and the text should be the point of reference, which would enable a total sample size of 1340 patients. When comparing the placebo and antibiotic groups regarding presence of infection, the authors found this to be insignificant ($p = 0.759$). Using the same statistical test with the placebo group as $n = 447$, findings are still insignificant ($p = 0.129$) but leave the reader confused regarding how to interpret the study's findings. Regardless, the authors have provided the literature with a study that those abiding by evidence-based medicine can use to bolster their argument to not administer antibiotics "unnecessarily" and to fight the urge to do so despite traditional teaching and national health care policy measures.

DOI: 10.1097/PRS.0b013e3182173e28

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The Solar System Model for the Reconstructive Ladder

Sir:

It was with interest that we read the article by Knobloch et al. entitled "The Reconstructive Clockwork of the Twenty-First Century: An Extension of the Concept of the Reconstructive Ladder and Reconstructive Elevator" published in the October issue of *Plastic Reconstructive Surgery.*[1] Knobloch has described the reconstructive clockwork model to explain the complexity of reconstructive approaches and to emphasize the roles of composite tissue allotransplantation, robotics, and regeneration tissue engineering in the daily reconstructive procedures. In this model, the reconstructive ladder concept has been abandoned. The authors consider all techniques as integral parts of a reconstructive sequence that is not necessarily consecutive but simultaneous. This concept is expressed by clockwork, where each cogwheel represents a different technique that varies from the easiest one to the most complex one; all of these techniques interrelate to achieve the best reconstructive result.

In this communication, we would like to point out some historical notes regarding the reconstructive ladder. The concept of the reconstructive ladder has been proposed to establish priorities for technique selection based on the complexity of technique and defect requirements to ensure a perfect wound closure.[2]

Unfortunately, the simplest reconstructive options may not produce a superior reconstructive result: sometimes, the most complex procedure is necessary to achieve an optimal result. Thus, the surgeon first has to improve his or her abilities and then he or she is ready to climb the ladder.

The reconstructive elevator model described by Gottlieb and Krieger let the surgeon range from the simplest to the most complex techniques with the freedom to reach directly the chosen level of complexity. Clearly, this decision is based on the needs of the patient and the skill of the surgeon.[3–5]

The reconstructive triangle, described by Mathes and Nahai, emphasizes the necessity of selecting the best treat-

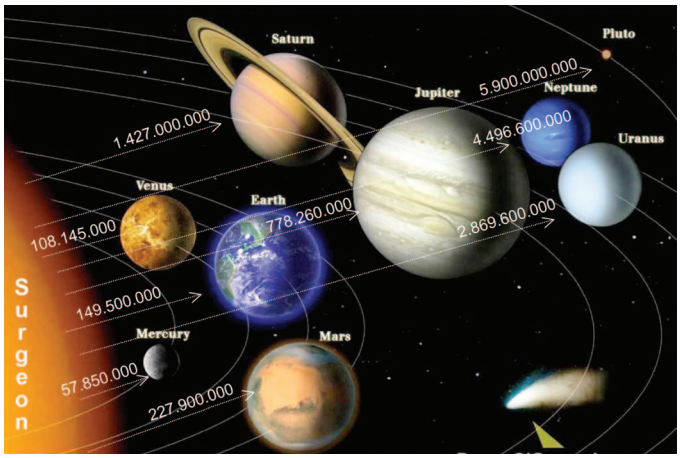

Fig. 1. Illustration of the solar system.

Table 1. Comparison of Each Planet with a Plastic Surgery Term

Celestial Object	Plastic Surgery Correlate
Sun	Surgeon
Mercury	Direct closure
Venus	Skin grafts
Earth	Regeneration tissue (e.g., artificial dermal graft)
Mars	Local flap
Jupiter	Tissue expansion
Saturn	Free flap
Uranus	Robotics
Neptune	Allotransplantation
Pluto	New techniques

ment, not necessarily the simplest one.[6] The surgeon uses the reconstructive triangle, exploiting his or her individual experience, for the best technique selection to achieve the goals. In this model, the surgeon is the central figure and has an active part in the choice of the treatment. However, the triangle does not convey the idea of increasing complexity that the ladder concept suggests.

The reconstructive stages, described by Wong and Niranjan, are a metaphor for plastic surgeon growth.[7] Like a baby who first crawls and then stands up and when his or her confidence increases, and then walks and runs, so too does the surgeon improve his or her surgical ability. This concept conveys the dynamism of surgical training and the sense of maturing as each stage is mastered.

On this basis we propose a new iconographic idea of the reconstructive ladder concept: we compare the solar system to the reconstructive diagram. The surgeon represents the sun and the planets symbolize all the reconstructive options. The planets, whose orbits are closer to the core, represent the most simple treatments, and often the most used ones, in medical practice.

In the following model, all levels of complexity are represented, and the close correlation between the surgeon's skill, the available techniques, and the patient's needs is exhaustively displayed (Fig. 1 and Table 1).

DOI: 10.1097/PRS.0b013e318217452a

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Reply: The Solar System Model for the Reconstructive Ladder

Sir:

"Exploration, discovery, and creative scientific research are the keys to new knowledge, and essential toward understanding our origins and destiny."[1] This statement is guiding the future of the National Aero-

nautics and Space Administration. It also applies to our stimulating speciality perfectly.

It is with great pleasure that we acknowledge the recent interest of ideas concerning the various reconstructive options that we can offer our patients in need today. The recent stimulating letter by Valentina Giordano et al. from Italy entitled "The Solar System Model for the Reconstructive Ladder" in the *Journal* again highlights the fundamental achievements of current plastic and reconstructive surgery.[2] The current momentum in terms of conceptual thinking in reconstructive surgery appears to drift somehow into outer space, as reflected by the concept of the "reconstructive matrix" proposed by Erba et al. in 2010 in this *Journal*.[3] Including the concept of reconstructive stages with maturation of the surgeon by Wong and Niranjan in 2008,[4] and our "reconstructive clockwork" in 2010,[5] it seems that with the evolving reconstructive techniques including acellular dermal matrices, composite tissue allotransplantation, tissue engineering and regeneration, and robotics, an increased complexity of procedures and surgical skills is mandatory.

As far as the proposed reconstructive solar system is concerned, the surgeon in the center of the Milky Way has been assigned an extraordinary role; however, at least in our humble perception, the patient might better fit in the center of our common reconstructive efforts to obtain the best clinical result. Thus, we are in the midst of the discussion of who should be in the center of the universe, the surgeon or the patient. Likewise, Nicolaus Copernicus was involved in some discussion while proposing the heliocentric cosmology in his publication *De Revolutionibus Orbium Coelestium* in 1543.

Giordano et al. state that "the planets, whose orbits are closer to the core, represent the most simple treatments, and often the most used ones, in medical practice." We would like to question, for example, whether using acellular dermal matrices to represent the Earth in their concept is more often used than local flaps (Mars) or free flaps (Saturn).

Considering our closest neighbors, Venus (skin grafting), being close in size to Earth but 400°C warmer because of greenhouse gases, or Mars (local flap) with its carbon dioxide atmosphere, skin grafting and local flaps are fundamental, long-standing techniques in plastic and reconstructive surgery. Mercury as a planet, representing direct closure, is the smallest and fastest moving planet in our solar system, with no natural satellites and almost no atmosphere. Besides, Project Mercury was the name of the first human spaceflight program of the United States from 1959 to 1963, followed by the Gemini and Apollo programs. Thus, direct closure as probably the fastest way to achieve defect closure is reiterated by the metaphor Mercury, the Roman symbol for speed.

From an astronomical point of view, we have to correct the authors that Pluto is currently classified as a dwarf planet and no longer as a planet, which was changed in 2006 by the International Astronomical Union because it lacks the gravitational muscle to sweep up or scatter objects near its orbit.

DOI: 10.1097/PRS.0b013e3182173e8d

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Comment on the Safe Management of Sedation for Plastic Surgery

Sir:

We read Dr. Thomas Mustoe's article with great interest,[1] as it reflects our surgical experience over the past 16 years. We perform plastic surgery in a private practice clinic complying with national and state regulations. During that time, we have operated on 3790 patients. Deep and conscious sedation techniques to support local anesthesia with the tumescent approach were used as described in the article. In our cases, for lipoabdominoplasties, extensive liposuctions, and gluteal prosthesis placement, we added the use of epidural anesthesia followed by immediate conscious sedation. A premedication combination of orally administered midazolam and nonsteroidal antiinflammatory drugs (ketorolac and diclofenac) with ondansetron, dexamethasone, and an antibiotic agent is also a part of our routine.

During the first 10 years of practice, only midazolam-ketamine was used extensively for deep and conscious sedation with satisfactory results, always under the supervision of an anesthesiologist. Over the past 6 years, the use of fentanyl as an addition to midazolam-ketamine or midazolam-fentanyl alone was added to our routine. Flumazenil was also added to our practice dur-

ing this period. Flumazenil is now used frequently at the end of procedures to assist in transport of the patients to the recovery area. The use of naloxone was necessary in only one case.

Intubation of a patient has never been encountered in our practice, and airway assistance by cannulas or bag to support ventilation is a rare event. Low-molecular-weight heparin is routinely used for long procedures or those involving epidural anesthesia. Deep venous thrombosis confirmed by Doppler studies was a complication in two of our patients. Both cases resolved favorably after anticoagulation. Thromboembolism has never occurred. The incidence of nausea and vomiting is estimated to be approximately 3 percent as stated in the article. Postoperative pain control is good enough to allow all of our patients to be discharged on the same day of the procedure, with the exception of lipoabdominoplasty patients. Readmission after discharge is unusual.

Not mentioned in the article is that the use of midazolam is associated with the appearance of two frequent adverse effects: tachyphylaxis and paradoxical excitation. In our practice, none of these situations has ever threatened the surgical outcome or patient comfort. The use of nalbuphine is of great help in these cases.

Fluid communication and knowledge and understanding of the advantages and limitations of sedation and the nature of the local anesthesia technique by both surgeon and anesthesiologist play a vital role in the success of these procedures. Understanding when sedation should be limited and local anesthesia adjusted to avoid oversedation is critical. We frequently perform combined operations. In these cases, sedation has advantages over general anesthesia, because change in the supine position, although assisted, is performed mainly by the patient, avoiding potential lesions caused by immobilization under general anesthesia, and also reducing the possibility of thrombosis.

DOI: 10.1097/PRS.0b013e318217401c

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Correction of an Important Typographic Error in *Grabb and Smith's Plastic Surgery*

Sir:

We would like to thank the editors of the enlightening textbook *Grabb and Smith's Plastic Surgery* for their excellent work.[1] All the basic principles and fundamental techniques needed for daily practice of a plastic surgeon are presented in the book.

This comprehensive book is preferred by plastic surgeons worldwide who want to access all necessary theoretical data in a well-organized manner. Despite the delicate and accurate revision of the editors, we noticed an overlooked fault in the book.

In the second paragraph of page 419, the most superficial facial muscles are listed as depressor anguli oris, zygomaticus minor, and orbicularis oris. However, in the next sentence, the depressor anguli oris is noted to take place in the deepest muscle layer of the face.

In their original article, which is cited in this paragraph, Freilinger et al. emphasize that facial muscles are arranged in four layers regarding their origins, and the depressor anguli oris is included in the most superficial layer.[2] Moreover, this article is cited twice as a reference in this paragraph but is listed in consecutive numbers at the end of the chapter.

It is clear that there is a wrong opponent statement and an overlooked typesetting error in this chapter. Therefore, we would like to draw the attention of the editors to this fault, which can be easily corrected in subsequent editions.

To help the owners of this edition to correct the fault in the book, we send this letter to the Editor of *Plastic and Reconstructive Surgery*, which is followed globally, considering that both the book and the *Journal* are published by the same company. In conclusion, we appreciate the opportunity to thank the Editors of this informative book for their effort.

DOI: 10.1097/PRS.0b013e318217400a

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Correction: Alloplastic Augmentation of the Facial Skeleton: An Occasional Adjunct or Alternative to Orthognathic Surgery

In the article by Yaremchuk et al. entitled "Alloplastic Augmentation of the Facial Skeleton: An Occasional Adjunct or Alternative to Orthognathic Surgery," published in the May 2011 issue of the *Journal* (*Plast Reconstr Surg.* 2011;127:2021–2030), the second author's first name is misspelled. The correct spelling is *Gaby* Doumit, M.D. (correction in italics).

DOI: 10.1097/PRS.0b013e31822463ac

REFERENCE

Yaremchuk MJ, Doumit G, Thomas MA. Alloplastic augmentation of the facial skeleton: An occasional adjunct or alternative to orthognathic surgery. *Plast Reconstr Surg.* 2011;127: 2021–2030.

Correction: Elongated Cell Morphology and Uniaxial Mechanical Loading Contribute to Tenocyte Microenvironmental Niche

In the abstract by Li et al. entitled "Elongated Cell Morphology and Uniaxial Mechanical Loading Contribute to Tenocyte Microenvironmental Niche" (abstract 84), published in the May 2011 Plastic Surgery Research Council 56th Annual Meeting Abstract Supplement (*Plast Reconstr Surg.* 2011;127 (5 Suppl):50), the name of the first author is misspelled. The list of author names should read *Li J,* Zhu J, Zhang WJ, Zhou GD, Cao YL, Liu W (correction in italics).

DOI: 10.1097/PRS.0b013e318225bae8

REFERENCE

Li J, Zhu J, Zhang WJ, Zhou GD, Cao YL, Liu W. Elongated cell morphology and uniaxial mechanical loading contribute to tenocyte microenvironmental niche. *Plast Reconstr Surg.* 2011;127 (5 Suppl):50.

Contribute to Plastic Surgery History

The *Journal* seeks to publish historical photographs that pertain to plastic and reconstructive surgery. We are interested in the following subject areas:

- Departmental photographs
- Key historical people
- Meetings/gatherings of plastic surgeons
- Photographs of operations/early surgical procedures
- Early surgical instruments and devices

Please send your *high-resolution* photographs, along with a brief picture caption, via email to the Journal Editorial Office (ds_prs@plasticsurgery.org). Photographs will be chosen and published at the Editor-in-Chief's discretion.