Venous Drainage Disorders as a Cause of Severe Chemosis following Fronto-Orbital Advancement

Sir:

We read the article entitled “Severe Chemosis following Fronto-Orbital Advancement” by Hatef et al. (Plast Reconstr Surg. 2010;125:56e–58e) with great interest. This proposal is about presenting an alternative to the theory recently stated by Hatef et al. regarding severe chemosis following fronto-orbital advancement procedures.

Craniosynostosis is a condition in which one or more of the fibrous sutures in an infant’s skull fuses prematurely. This results in restricted skull and brain growth. Reshaping the skull with advancement of the fronto-orbital forehead is the cornerstone of many surgical procedures in craniosynostosis. Severe chemosis following fronto-orbital advancement in the treatment of craniosynostosis had never been reported before the article by Hatef et al.1 The pathophysiology of this unexpected postoperative morbidity is not understood. The authors stated that chemosis was caused by a combination of increased postoperative swelling with jugular veins occluded by the tracheostomy collar.

Our similar case suggests another cause. A 2-month-old girl presented with severe chemosis in the postoperative course of fronto-orbital advancement. She was operated on for a complex craniosynostosis involving all the skull sutures, called kleeblattschädel syndrome. This syndrome results in increased intracranial pressure and a cloverleaf-shaped head.2

We performed fronto-orbital advancement and cranial vault remodeling. This procedure dramatically improved intracranial pressure and also the shape of the skull. However, unexpected severe chemosis occurred 12 hours after surgery, with a high risk of damaging our patient’s vision (Fig. 1). The head of the patient’s bed was elevated. The protruding conjunctivae were treated by generously applying an ophthalmic ointment (Lacrivisc gel) and performing bilateral blepharorrhaphy. Chemosis resolved spontaneously and completely in 72 hours, with no visual impairment (Fig. 2).

Our patient had neither tracheostomy nor any type of jugular compression. This case clearly suggests that jugular compression may not be the only factor causing chemosis. It was already demonstrated that in the event of severe craniosynostosis, cerebral venous drainage is compromised by malformation of the jugular foramen. Veinous drainage is then diverted to transcalvarial emissary veins. During scalp dissection, section of those veins would cause brain swelling, hydrocephalus, and even death.3 Starting from this fact, we believe that chemosis is probably attributable to a disorder of the orbital venous drainage, which deteriorated after emissary vein interruption. Because of the risk of chemosis, we suggest that the surgeon should precisely assess cerebral and orbital venous drainage by means of magnetic resonance angiography, adapt the surgical procedure in every individual case, and take special care of the main emissary veins.

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![Figure 1](image_url)

**Fig. 1.** Twelve-hour postoperative view demonstrating severe bilateral chemosis.

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Longevity of SMAS Facial Rejuvenation and Support

Sir:

We read with much interest the article by Michael J. Sundine et al. entitled “Longevity of SMAS Facial Rejuvenation and Support” (Plast Reconstr Surg. 2010;126:229–237). The article asks how long a well-performed superficial musculoaponeurotic system (SMAS) flap face lift will last on average. We think the surgical technique performed by the authors is safe and effective; however, we have a few comments regarding some of the topics.

First, the specimens include 43 secondary face lifts performed after the primary face lift had been performed by the senior author. Nevertheless, from January of 2001 to December of 2008, the senior author performed 299 face lift procedures. We think the number of specimens is tiny compared with the total number of cases, and that the results based on the 43 specimens are insufficient representation.

Second, the authors draw a conclusion that a well-performed SMAS flap face lift will last 12 years on average. The result of 12 years was obtained according to the time interval from the primary to the secondary face lift performed by a single author. The determinations on the timing of the secondary procedure were based on the patient’s needs. There are many reasons why a patient who has had a primary face lift may return for a secondary face lift. Subjectivity and life stress are major factors among those reasons, and the decision to have such a procedure may have little relationship to how the patient actually appears. Ascertaining longevity in face lifting remains difficult because of the multiple variables that affect long-term outcomes. Thus, there is a need for a standardized, objective, and reliable method for measuring the severity of facial wrinkles and folds to evaluate and compare the efficacy of cosmetic treatments. Over the years, a variety of assessment systems to measure the severity of wrinkles have been proposed.1–5 Among those assessment systems, the Fitzpatrick wrinkle score1 is a reliable method for quantitative assessment of skin wrinkles and folds that plastic surgeons and dermatologists could use to assess their treatments. The Fitzpatrick classification is based on generalized wrinkling, elastosis, and dyschromia as well as wrinkle depth. Using reference photographs, the wrinkles are classified into one of three classes (1, 2, or 3), which are defined as mild, moderate, or severe. Hence, all of these face lift patients should be available for yearly follow-up observation. The longevity of face lift can be assessed from the long-term follow-up photographs with the Fitzpatrick wrinkle score. We think the results of the study would have been more objective and significant if the above methods had been applied in the authors’ study.

We would like to express our thanks to the authors for this valuable article reporting so much experience with the technical considerations in performing a successful secondary SMAS face lift.

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PATIENT CONSENT

Parents or guardians provided written consent for the use of patient images.

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Fig. 2. One-week postoperative view showing complete resolution of chemosis after generous application of Lacrivisc ointment and bilateral blepharorrhaphy.
There are really not many reasons that patients undergo a secondary procedure following a successful first procedure. Quite simply, the patients want to maintain their youthful appearance. There are many other reasons that patients who had a successful primary procedure do not proceed with a secondary procedure, and we reviewed many of these in our article. Our study does suffer from many of the problems that plague studies regarding aesthetic surgery, including the flaws associated with retrospective reviews, the lack of a clearly defined classification system, and subjective review of results.

A retrospective application of any of the classification systems proposed by Lihong et al. would likely add little value to the study. If the Fitzpatrick classification had been applied prospectively to the study, approximately 40 patients would have been excluded from the study. Classification systems can be useful for comparing various treatments or for comparing a treatment modality in various subgroups. However, there are very few classification systems that allow for clinical decision making such as the Breslow classification, which aids in determining the appropriate care for patients with malignant melanoma.

At this time, there is no classification system that is sophisticated enough and yet subtle enough to aid in decision making in something as complex and with the nuances of secondary face lifting. A clinical example is the patient with a pixie ear deformity following a primary face lift who also has no hollowness anterior to the tragus along with a preauricular scar and widening of the scar in the occipital area. Even though the patient is interested in improving the appearance of the face and neck, he or she is actually skin deficient and would be best served by delaying the secondary procedure regardless of what the wrinkle score shows. These difficult judgment calls are precisely the reason why the timing of surgery is a collaborative decision between the patient and the surgeon. They are also the exact reasons why simplistic classification schemes are inadequate for secondary face lifting and probably also for primary face lifting. We appreciate Dr. Lihong and colleagues’ thoughts, which have stimulated us even further to refine our thinking and treatment of patients with facial aging.

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Reply: Longevity of SMAS Facial Rejuvenation and Support
Sir:

We would like to thank Dr. Lihong et al. for their interest in our article and would like to attempt to address their specific concerns. As we noted in our article, there is a lack of published series regarding secondary face lifting. The largest of these published series included 33 patients, and of these patients, only three had both the primary face lift and the secondary face lift performed by the same surgeon. We would argue that our series of 43 patients where both the primary and secondary face lifts were performed by the same surgeon is unprecedented. Further discussion regarding this timing is included below.

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REFERENCES
The Fascial Planes of the Temporal Region Related to the Frontal Branch of the Facial Nerve

Sir:

We read with great interest two recent articles regarding the anatomical study of the course of the frontal branch of the facial nerve: (1) the article by Agarwal et al. entitled “The Course of the Frontal Branch of the Facial Nerve in Relation to Fascial Planes: An Anatomic Study” (Plast Reconstr Surg. 2010;125:532–537); and (2) the study by Trussler et al. entitled “The Frontal Branch of the Facial Nerve across the Zygomatic Arch: Anatomical Relevance of the High-SMAS Technique” (Plast Reconstr Surg. 2010;125:1221–1229).

In the first article, Agarwal et al. describe the frontal branch of the facial nerve as it crosses the zygomatic arch within the innominate fascia, which is located deep to the superficial musculoaponeurotic system (SMAS) and superficial temporal fascia. They beautifully show the hemiface dissection in a layer-by-layer fashion and illustrate histologic evaluation of the relationship between the nerve and the fascial planes above and below the zygomatic arch. Their fascial dissection demonstrates that the frontal branch travels within the innominate fascia as it crosses the zygomatic arch into the temporal region. In the second article, Trussler et al. defined the depth and fascial boundaries of the frontal branch of the facial nerve over the zygomatic arch using histologic evaluation. They demonstrated that the frontal branch coursed under a separate fascial plane, the parotid-temporal fascia, which is deep to the SMAS in its course to the zygomatic arch and remained within this deep fascia over the arch. They concluded that the frontal branch of the facial nerve was protected by a deep layer of fascia, termed the parotid-temporal fascia, which is separate from the SMAS as it travels over the zygomatic arch.

We agree with the authors that the position of the frontal branches of the facial nerve were described in the literature on two-dimensional nerve trajectory, with conflicting descriptions of the fascial depth of the frontal nerve branches, such as in subcutaneous tissue, within the superficial temporal fascia, in the loose areolar layer and in close proximity to the zygomatic periosteum. The authors stated that the confusion stems in part from the dense adherence of fascial planes in preserved cadavers. Also, the inconsistency of nomenclature used to describe the different fascial layers makes it more difficult to understand the anatomy in this region.

Generally, there are nine layers in the temporal region, including the skin, the subcutaneous tissue, the superficial temporal fascia, the loose areolar layer, the superficial layer of the deep temporal fascia, the superficial temporal fat pad, the deep layer of the deep temporal fascia, the deep temporal fat pad, and the temporalis muscle. The superficial temporal fascia (temporoparietal fascia) in the temporal region is the most superficial layer beneath the subcutaneous tissue. It is continuous with the galea superiorly, the frontalis muscle anteriorly, and the SMAS inferiorly. Deep to the superficial temporal fascia is a loose areolar fascial layer, which is called the innominate fascia by Agarwal et al. in the first article, the parotid-temporal fascia by Trussler et al. in the second article, and the intermediate temporal fascia by Wang et al. in a Chinese journal in 1992. Deep to the fascia is the deep temporal fascia that covers the temporalis muscle. Wang et al. stated that the intermediate temporal fascia was in loose continuity with the parotid masseteric fascia in the cheek. The frontal-temporal branch of the facial nerve exited the parotid gland within the parotid-masseteric fascia and continued within the intermediate temporal fascia across the zygomatic arch. The result is the same with Agarwal et al. and Trussler et al., although the nomenclature is different.

It is necessary to clarify the relationship of the fascial planes of the temporal region with consistent nomenclature, because the planes may be obliterated when previous surgical procedures have been performed in this region. We suggest that the fascia be named by its location as the intermediate temporal fascia, because it is located between the superficial and the deep temporal fascia. It is also uniform in the temporal region and easy to understand.

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Reply: The Fascial Planes of the Temporal Region Related to the Frontal Branch of the Facial Nerve

Sir:

In response to the Letter to the Editor, the authors of the referenced article “The Frontal Branch of the Facial Nerve across the Zygomatic Arch: Anatomical Relevance of the High-SMAS Technique” appreciate the comments about the article and agree that this area in the temporal region is confusing. This was one of our major inspirations for performing the anatomical study. We feel that the recommendations in the letter may continue to add layers within this area rather than formally define them. The parotid temporal fascia as described in our article is a defined fascial plane below the transition between it and the temporal parietal fascia. From our anatomical dissections with histologic evaluation and the senior author’s vast clinical experience, it appears that there is a fusion plane between the parotid temporal fascia and the temporal parietal fascia where the frontal branch transitions from a deep to a more superficial depth. This point was captured on histologic evaluation and contains an anterior branch of the superficial temporal artery. This fascial plane can be separated easily in anatomical and clinical dissection, thus explaining the conclusion of our article that the division of the superficial musculoaponeurotic system can be performed safely above the zygomatic arch. Above this fusion point, it is very difficult to define two distinct fascial planes between the temporal skin and deep temporal fascia. Therefore, we would simplify rather than complicate this area and keep the temporal parietal fascia as the intermediate fascial plane and not name an additional intermediate temporal fascia.

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Surgical Procedure for Direct Excision of Glabellar Furrows May Be Problematic

Sir:

I read with interest the article entitled “Direct Excision of Glabellar Furrows: An Alternative Treatment for Severe Glabellar Rhytides” in the June 2010 issue of *Plastic and Reconstructive Surgery.* In their article, the authors present a prospective study to assess direct excision for treatment of severe glabellar rhytides. The group included 10 patients. They stated that all patients were very satisfied with their results. They concluded from their study that direct excision should be considered a viable option for suitable patients.

On the basis of my clinical experience and review of the relevant literature, I have strongly opposed this surgical method. I believe there is at least one line of evidence supporting my point of view. Scar, especially hypertrophic scar, would present formidable difficulty with regard to direct excision of glabellar furrows. Tanna et al. reported that one of 10 patients had mild hypertrophic scar and required scar resurfacing; in other words, the rate of hypertrophic scarring was 10 percent. I wondered whether the patients included in the study by Tanna et al. were white people, because the race of the patients was not mentioned in their article. However, I inferred from the figures in the article by Tanna et al. that the patients were white people. As we all know, race is very closely associated with the incidence of hypertrophic scarring. Black and Asian people have a higher rate of hypertrophic scarring and keloids than do white people. Therefore, if the rate of hypertrophic scar was 10 percent according to the results of Tanna et al., I inferred that direct excision of glabellar furrows would result in a very high rate of hypertrophic scarring in patients who were black or Asian. Thus, direct excision of glabellar furrows was never suitable for Asians or blacks. I even doubted that it was suitable for white people because of the 10 percent rate of hypertrophic scarring.

*Plastic and Reconstructive Surgery* is such an attractive journal that many plastic surgeons have regarded it as the bible of plastic surgery worldwide. Because of its academic authority, its readers are spread all over the world and include many Asian and African plastic surgeons. Therefore, I wrote this letter to avoid misleading many Asian and African plastic surgeons.

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Sir:

Reconstruction of the Upper Lip

One of 500 to 1000 babies are born with a cleft. Two Cleft lip, with or without cleft palate, is the most common of these facial clefts. Reconstruction of the upper lip and restoration of the continuity of the circular muscle in the lip are important steps in treatment. However, because of many comprehensive factors, bilateral cleft lip revisions lead to unsatisfactory results such as scars, more tension, and a paucity of vermilion, leaving stigmata that require multiple revisions throughout childhood.

A major difficulty for the surgeon is how to solve the problems. Multiple methods, such as the Abbe flap, have been reported in the plastic surgery literature for upper lip reconstruction of bilateral cleft lip revisions. However, there is no completely satisfactory procedure for repair. Restoring functionality and appearance of the upper lip is difficult to achieve because of its composite functions, the many anatomical subunits, and the limited availability of adjacent skin. A transposition flap such as the Abbe flap is preferred in medium size defects and has an excellent cosmetic result when it is used to replace the entire philtrum during the second operation for correction of secondary deformity. The Abbe flap can also be used in lateral defects, including the commissure. However, this repair takes two operations and often requires a second procedure of commissurotomy to ensure sufficient oral access and maintenance of commissure competence. Meanwhile, the method is difficult for beginners to master.

The reverse Yu flap is described as a very reliable new technique that combines the advantages of reconstruction using both rotation and advancement flaps. The study by Belmonte-Caro et al. indicated that the Yu flap could be suitable for bilateral cleft lip reconstruction. We hypothesized that there could be a very satisfactory procedure for repair if we used the Yu flap during the initial operation for bilateral cleft lip revision (Fig. 1). The blue arrows could indicate the flap rotation and advancement (Fig. 1, above, left). The green lines could indicate the new locations of the bilateral cleft lip (Fig. 1, above, right). Because the bilateral lips were closer and because there was sufficient tissue, bilateral cleft lip reconstruction was much easier. This flap may also be a versatile procedure that allows in one operative procedure acceptable functional and aesthetic effects. Meanwhile, the design recreated the normal structure of the upper lip and the central and lateral subunits, without distorting the commissure. We could make incisions along the labiofacial sulcus (Fig. 1, above, left). That could reduce and cover the scar formation. Another goal was to avoid color differences between the reconstructed skin and the remaining original skin of the lip. The thickness of the half reconstructed lip matched the original dimensions of the opposite upper half-lip.

Because the flap reestablished the circumferential nature of the lip muscles, the closure could have a very good functional outcome combined with other operative methods such as that of Millard, which allowed for

Use of the Reverse Yu Flap for Difficult Reconstruction of the Upper Lip

Sir:

We read with great interest the recent article “Bilateral Cleft Lip Revisions: The Abbe Flap.” We would like to take the opportunity to further expand on the topic of using the reverse Yu flap for difficult reconstruction of the upper lip.
oral competence, continuity of vermilion border, and adequate size of the opening. The method may reduce the continuous muscle contraction and skin tension and create good conditions for wound healing and achieve a satisfactory effect after lip repair (Fig. 1, below); more importantly, the method is much easier for beginners. Finally, the Yu flap may have effective clinical applications on bilateral cleft lip revisions and help the patients to obtain a straighter and more naturally appearing smile.

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REFERENCES


Reply: Use of the Reverse Yu’s Flap for Difficult Reconstruction of the Upper Lip

Sir:

The authors are to be congratulated on their innovative use of reverse Yu flaps. There is no question that this technique allows for the effective recruitment of tissue from the cheek to the upper lip while preserving the oral commissure. This, however, seems better suited for elderly patients with pononcologic defects. In the primary situation, placing additional scars on an infant’s cheek, outside of those classically used for bilateral cleft lip repair, is generally not accepted. Unlike older patients, these scars, however small, are poorly concealed in the plump cheeks of children. In situations in which the cleft is wide, it is still preferable to use a molding device, either passive or active, to reduce the size of the defect. Although it is a very time-consuming step, there can be no question that this improves results
in the wide bilateral cleft. Should these resources not be available, a lip adhesion is also an acceptable option when adequate tissue is unavailable. The advantage of this when compared with the authors’ technique is that the scars remain within the generally accepted locations for bilateral cleft lip repair. Finally, the use of this technique to correct secondary deformities has several disadvantages. The recruitment of additional tissues does not help to recreate the normal dimensions or appearance of the philtral aesthetic unit, does nothing to add healthy muscle to an area full of scar, and fails to conceal subsequent scars well. Although the Abbe flap also places an additional scar in the perioral region, the technique manages to better address the secondary deformities associated with the bilateral cleft lip by recreating a more anatomical central lip subunit. Although this technique may have some utility in the occasional case, as a general rule, our current techniques are preferred for both primary and secondary bilateral cleft lip repair.

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Second Human Facial Allotransplantation to Restore a Severe Defect following Radical Resection of Bilateral Massive Plexiform Neurofibromas

Sir:

A fter reading the excellent article entitled “Facial Aesthetic Unit Remodeling Procedure for Neurofibromatosis Type 1 Hemifacial Hypertrophy: Report on 35 Consecutive Adult Patients” by Hivelin et al.,1 we would like to provide some details from our personal experience. We recently had one patient that received a facial composite tissue allograft of the lower two-thirds of the face to repair a severe defect after excision of bilateral massive plexiform neurofibromas. On January 26, 2010, our team carried out the second partial face transplantation in Spain. The success of face transplantation performed in Paris in 2007 by Lantieri et al.2 greatly encouraged us. To our knowledge, our case represents the world’s second face transplantation in neurofibromatosis type 1 with involvement of the second and third divisions of the trigeminal nerve. As our case greatly resembles that of the authors, we would like to share some aspects of our preliminary experience to provide a better understanding of the indications and perioperative implications of composite tissue allografts in this particular group of patients.

First, the recipient was a 35-year-old Caucasian man from Seville, Spain, who received a composite tissue allograft of the lower two-thirds of the face, including a chin osseous segment, to repair a severe defect resulting from a radical resection of bilateral massive plexiform neurofibromas (Figs. 1 and 2). Before this operation, the patient had undergone 17 surgical interventions.

Second, a composite tissue allograft of the two lower parts of the face comprising skin, subcutaneous tissue, lips, mouth, oral mucosa, perioral muscles, bilateral cheeks, bilateral parotid glands, bilateral facial nerves, bilateral mental nerves, bilateral infraorbital nerves, bilateral common carotid arteries, bilateral internal jugular veins, and an osseous chin segment was transplanted. The allograft was harvested from a donor after cardiac death, starting shortly after the recovery of the heart and lungs.

Third, we performed a monobloc tumoral resection from the superficial skin to the periosteum, including bilateral facial, bilateral mental, and bilateral infraorbital nerves from their main trunks, because they were completely infiltrated by the tumor. Most facial nerve

Fig. 1. Preoperative appearance of the patient.
branches had already been damaged in previous debulking of neurofibromas. As expected, uncontrolled bleeding was present during tumor removal. Our patient had considerable blood loss that required transfusion of 24 units of packed red blood cells, 1500 cc of fresh plasma, one pool of platelets, and 7 g of fibrinogen. The patient required revision surgery on day 7 for removal of an extensive hematoma on the right side of the composite tissue allograft and then received three units of packed red blood cells, 1000 cc of fresh plasma, and two pools of platelets.

Fourth, the macroscopic histologic study of the specimen demonstrated at least six major massive plexiform neurofibromas, ranging from 11 to 3 cm, located in the entire right face, subnasal area, upper lip, left cheek, and chin. The entire specimen measured $27 \times 16$ cm and weighted 940 g.

Finally, Hivelin et al.\textsuperscript{1} identified patients with plexiform neurofibromas associated with neurofibromatosis type 1 as possible candidates for face transplantation. The nonexistence of feasible reconstructive options led them, and also to us, to perform this procedure. We agree with the authors that patients afflicted with severe, locally aggressive neurofibromas are a potential group of patients that may require facial reconstruction with composite tissue allografting. However, this procedure must still be regarded as an experimental option and patients should be selected carefully. Among other criteria in patients with bilateral massive plexiform neurofibromas with high malignant risk, having undergone many other previous procedures must be taken into account. As a global scientific opinion, we consider that, because only two transplantations have been reported thus far, further research will be necessary to consider this group of patients as suitable candidates for facial transplantation. As stated by Lantieri et al.,\textsuperscript{2} our teams must be hypervigilant in surveillance for any increased malignant transformation attributable to long-term immunosuppression.

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

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REFERENCES

An Improved System for Large Volume Lipomodeling

Sir:

I read with interest the article by Curnier et al. regarding an improved system for large volume lipomodeling.\textsuperscript{1} Now that fat transfer has become ac-
ceptable as a modality in a variety of cosmetic and reconstructive procedures, instrumentation has and will become available to improve the effectiveness and ease of performing the operation. The authors here present a collection receptacle in series as an effective method of collecting fat for reinjection. I agree wholeheartedly with their conclusions and I also believe they are proving the old adage “What is old, is new again.” The apparatus looks very similar to the simple in series “trap” bottle that I demonstrated and described in my first presentation of autologous fat transplantation in 1984 and again in 1987 (Fig. 1). My contributions to their method was absent from their bibliography.

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REFERENCES

Voice Restoration after Laryngectomy

Sir:

Your recent publication of the Special Topic article “Reconstruction of the Esophagus and Voice” needs to be addressed to clarify a number of misconceptions that will mislead plastic surgeons and oncologists that may treat patients with head and neck cancer, particularly after total laryngectomy. I originated the technique of tracheoesophageal puncture with placement of a one-way silicone valve to prevent tracheal aspiration 30 years ago. It remains the method of choice for voice restoration after total laryngectomy, irrespective of the method of neopharyngeal reconstruction that is used. It is widely accepted because of its simplicity, safety, reproducibility, success, and cost. The authors state that “voice reconstruction in patients with total laryngectomy is a complex and exciting topic.” This misleads your readers, because the puncture is not complex, and the authors further go on to list the disadvantages of this method. Obstruction of the valve is easily cleared, dilation of the puncture is unusual, bacterial and fungal growth do not predispose the user to airway infections, dislodgement is rare, tracheal stenosis is unrelated, and there is no association or rationale for esophageal perforation from the valve. When valve failure occurs after months, the valve is simply replaced as with contact lenses. They further state, “For patients with a longer life expectancy, we prefer to reconstruct the voice tube using a segment of jejunum, or ileum with ileocecal valve, to prevent complications associated with long-term use of voice prosthesis.” They present a number of imaginative techniques progressing from efforts to establish a valved shunt to using the ileocecal valve itself. I doubt that the modified jejunal shunt tube illustrated in Figure 8 would be satisfactory, and their other proposed methods leave an enteric segment to drain into the unprotected trachea. The authors fail to report their experience with these methods or to share any data regarding the acoustic analysis of the resultant speech or to compare it to tracheoesophageal phonation with a voice prosthesis. There is no comment on the effect of either preoperative or postoperative radiation therapy on the voice tubes. They do tabulate literature reports that include gastric interposition, pedicled flaps, fasciocutaneous flaps, and jejunal and colic flaps for reconstruction of the pharynx, many of which are no longer used and are outdated except for fasciocutaneous free flaps. The final discussion of laryngeal transplantation neglects the critical issue of immunosuppression in cancer patients, making this an exceedingly rare approach; the three reported non-cancer cases are of no practical value. The placement of a silicone voice prosthesis has served the needs of a large number of laryngectomy patients over the past 30 years throughout the world. It is a bridge until there is a better procedure, possibly using microsurgical techniques. The described procedures are imaginative, but they are not used in North America and are of doubtful value. Reconstruction of the tracheal airway, with elimination of the tracheostoma, is the challenge for future surgeons, and would represent a milestone in surgery. I submit these comments so that colleagues in collaborating disciplines for the treatment of head and neck cancer are not misled by this well-intentioned article.

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The Buccal Fat: A Convenient and Effective Autologous Option to Prevent Frey Syndrome and for Facial Contouring following Parotidectomy

Sir:
In their article entitled “The Buccal Fat: A Convenient and Effective Autologous Option to Prevent Frey Syndrome and for Facial Contouring following Parotidectomy,” Kim et al.1 describe the use of buccal fat pad grafting during parotidectomy as a prophylactic measure against Frey syndrome in 17 patients. These patients (group III in the article) were compared with patients who underwent superficial parotidectomy alone (group II) and patients who underwent parotidectomy with the interposition of various autologous barriers, including dermofat, temporoparietal fascia or fascia lata grafts, and a superficial musculoaponeurotic system flap (group I).

The only outcome considered was the occurrence of postoperative Frey syndrome, with the patients being postoperatively classified as mildly or strongly positive on the basis of their iodine starch test scores. However, they did not consider the impact of salivatory sweating on the patient’s quality of life, which we believe should not be neglected. It has in fact been suggested that the severity of Frey syndrome should be evaluated using standardized clinical scores that also consider the patient’s perception of their symptoms.2

After a long follow-up, Kim et al. observed postoperative Frey syndrome in 19 of 22 patients in group I, one of 13 patients in group II, and one of 17 patients in group III: the differences between groups I and II and between groups I and III were statistically significant, but the difference between groups II and III was not. On the basis of these findings, it could be argued that any barrier is effective in preventing Frey syndrome, regardless of the graft.

Furthermore, on the basis of the precise anatomical relationship between the buccal fat pad and the buccal branches of the facial nerve, the latter are vulnerable during manipulation of the pad, and there is an approximately 26 percent chance of their injury during its total removal.3 Unfortunately, the authors did not report the postoperative rate of facial nerve injury, but it would be interesting to know whether there was a difference in this rate between groups II and III. Finally, it was not stated whether there was any residual facial asymmetry in the cheek region after unilateral buccal fat pad mobilization. These data may be useful in evaluating the real benefit of buccal fat pad grafting over other autologous barriers in preventing postparotidectomy Frey syndrome.

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REFERENCES

High- and Low-Evolutive-Potential Premalignant Skin Lesions: What about the Role of Photodynamic Therapy?

Sir:
It is with interest that we read the article by Lee et al. entitled “Benign and Premalignant Skin Lesions” published in the May of 2010 issue of the Journal.1 We congratulate the authors for the brilliant and compre-
hensive CME article. We would like to take the opportunity to discuss some more key points.

Premalignant skin lesions can be classified into high- and low-evolutive-potential lesions that do not show real cellular atypia and seldom degenerate (Table 1). According to the World Health Organization (1972), a precancerous lesion is defined as a tissue morphologic alteration that can degenerate more probably than healthy tissue. On this basis, precancerous lesions can be classified as obliged (degenerative probability more than 90 percent as erythroplasia of Queyrat) and facultative, which can be distinguished in high-degeneration incidence as leukoplakia and lichen planus (5 to 10 percent) and low-degeneration incidence as submucous fibrosis, solitary papilloma, systemic lupus erythematosus, glossitis, and nicotinic leukokeratosis (1 percent). A further step is the identification of potentially malignant conditions, which means a general disease linked to increased risk of cancer evolution (i.e., Plummer-Vinson syndrome, xeroderma pigmentosum, lichen ruber planus, chronic liver disease, immunodeficiency, and hypovitaminosis).

Plastic surgeons are often faced with distinguishing between benign and malignant lesions and thus deciding whether to perform biopsy, observe, or reassure the patient. Signs that should lead physicians to perform biopsy include crusting and bleeding, scaling, pain, and changes in color and size.

Because of the recently developed second- and third-generation photosensitizers, photodynamic therapy has been rediscovered and indications extended compared with past years. It works through microvascular degeneration; apoptotic, immunologic, and inflammatory processes; and up-regulation of cytokines (interleukin-1, tumor necrosis factor-α) against abnormal tissue. Photodynamic therapy is a noninvasive and low-morbidity treatment that can be repeated as much as is needed at the same site and can be applied before or after conventional treatments without disfigurement in any outpatient clinics. Most photosensitizers are administered systemically, and because of recent progress, light delivery systems have reduced treatment times and residual photosensitivity and increased the depth of penetration.

Current evidence shows photodynamic therapy to be effective in the treatment of actinic keratoses, mucosal dysplasia, leukoplakia, Bowen disease, nonmelanoma skin cancer (superficial basal cell carcinoma as first), and carcinoma in situ, especially when size, location, and number of lesions (Gorlin-Goltz syndrome) could limit the acceptability of conventional therapy (i.e., surgery, radiotherapy, chemotherapy) or be compromised itself. Bowen disease represents one of the best clinical indications because the lesions are often large and surgery would result in extensive scarring. Photodynamic therapy can also be considered for nodular basal cell carcinoma less than 2 mm thick in cosmetically sensitive areas. Response rates and outcomes durability can overlap surgery, with negligible effects on the underlying structures.

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Table 1. Premalignant Skin Lesions

<table>
<thead>
<tr>
<th>High evolutive potential</th>
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<tr>
<td>Chronic radiodermatitis</td>
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<tr>
<td>Burn scars</td>
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<tr>
<td>Invertebrate tubercular lupus</td>
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<tr>
<td>Erythematous discoid chronic lupus</td>
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<tr>
<td>Scleroatrophic and ulcerous lichen</td>
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<tr>
<td>Xerotic occluding balanitis</td>
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<tr>
<td>Chronic ulcus crusis</td>
</tr>
<tr>
<td>Pseudopitheliomatous hyperplasia (Gottron syndrome)</td>
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<tr>
<td>Sebaceous adenoma (Jadassohn)</td>
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<tr>
<td>Tricoepithelioma cylindroma</td>
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<tr>
<td>Low evolutive potential</td>
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<tr>
<td>Actinic keratosis</td>
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<tr>
<td>Xeroderma pigmentosum</td>
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<tr>
<td>Leukoplakia</td>
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<td>Actinic cheilitis</td>
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<td>Actinic keratosis</td>
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DISCLOSURE
The authors have no conflicts of interest to disclose.

REFERENCES
Reply: High- and Low-Evolutive-Potential Premalignant Skin Lesions: What about the Role of Photodynamic Therapy?
Sir:
We read with intrigue the letter from Agostini et al. entitled “High- and Low-Evolutive-Potential Premalignant Skin Lesions: What about the Role of Photodynamic Therapy?” The group has interesting points about premalignant skin lesions and classified them into high and low evolutive potential and further divided precancerous lesions into obliged and facultative based on the degenerative probability. Lesions classified under high evolutive potential, as we interpreted, have a high risk of transformation into cancerous lesions. Among those listed are benign skin lesions such as sebaceous adenoma (Jadassohn), trichoepithelioma, and cylindroma. The risk of malignant transformation of these benign tumors is quite low; the risk of a basal cell carcinoma developing in a sebaceous adenoma (Jadassohn), trichoepithelioma, and cylindroma is less than 1 percent,1 and malignant transformation of a cylindroma is rare.2 Oral lesions were not included in our discussion, as the focus of the review was on cutaneous lesions. However, there is an array of premalignant oral lesions, as nicely shown by Agostini et al., and if the plastic surgeon frequently encounters oral mucosal lesions, he or she should be familiar with the presentation and clinical course. The definition and interpretation of a premalignant or precancerous lesion may vary among sources but, according to the National Cancer Institute, it is a “condition that may (or is likely to) become a cancer.”3 For example, an actinic keratosis is characterized by partial-thickness epidermal atypia and is historically considered to be a precancerous lesion. Erythroplasia of Queyrat is a squamous cell carcinoma in situ of the glans penis and is characterized by full-thickness epidermal atypia. Although the atypical cells are confined to the epidermis, given that this is an early skin cancer, it should be treated accordingly.

The plastic surgeon will be identifying suspicious lesions and will perform biopsy if indicated for timely diagnosis and management. A comprehensive understanding of the malignant potential of a skin lesion should also be interpreted in the context of the host. The risk of developing a skin malignancy is multifactorial and based on several host factors, including environmental exposures (e.g., ultraviolet light), host phenotype (e.g., fair skin, red hair, freckling), predisposing dermatologic conditions (e.g., nonhealing wounds, longstanding inflammatory disorders such as discoid lupus and lichen planus, genetic disorders), and host immune status (e.g., immunocompromise, human papillomavirus infection). It is essential to identify patients at high risk for developing a skin cancer and those at risk for an aggressive course. This is of particular importance for squamous cell carcinoma, as patients with chronic lymphocytic leukemia and solid organ transplantation are at higher risk for local recurrence and metastasis.1,5

Photodynamic therapy is a noninvasive treatment modality that has gained momentum in the dermatologic community for its effectiveness in treating premalignant skin lesions and nonmelanoma skin cancers. In its topical form, a photosensitizer such as aminolevulinic acid or methyl aminolevulinate is applied and taken up by target cells. A light source at a wavelength corresponding to the excitation peak of the porphyrin derived from the photosensitizer initiates a photochemical reaction, leading to cell apoptosis. Currently in the United States, the only U.S. Food and Drug Administration–approved indication for topical photodynamic therapy in dermatology is the treatment of actinic keratoses. However, there are multiple studies demonstrating efficacy of photodynamic therapy in squamous cell carcinoma in situ and in some types of basal cell carcinoma.6 Notably, studies with the methyl aminolevulinate form demonstrates long-term efficacy data, and it is an excellent treatment option for superficial basal cell carcinoma and squamous cell carcinoma in situ.7 Photodynamic therapy is noninvasive and may be a valuable therapeutic option in patients with large or multiple lesions, nonsurgical candidates, and selective cases where cosmesis or functionality is a concern. Therefore, if photodynamic therapy resources are available, it is an option that should be offered by the plastic surgeon or dermatologist for treatment of premalignant and malignant skin conditions.
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REFERENCES
Bra Stuffing for Implant Sizing? Satisfaction? Who, When, and Compared to What?

Sir:

I am writing regarding the article entitled “Preoperative Sizing in Breast Augmentation” published in the June of 2010 issue (Plast Reconstr Surg. 2010;125:1781–1787). The authors characterize their bra stuffing implant sizing methodologies as “simple” and “accurate.” Simple? Up to three visits to the surgeon’s office to ruminate over shades of gray using a totally subjectively derived decision process based on indefinable cup size parameters and the patient’s visual perceptions? Accurate? Thirty percent of sized respondents reported that the sizing methods were inaccurate.

Choosing breast implant size by bra stuffing has a repetitive, three-decade track record of 15 to 25 percent reoperation rates (and a major percentage of reoperations for size change) in the most stringent, independent studies, and the lowest incidence of uncorrectable tissue deformities. Armed with that knowledge, most patients rationally temper their wishes with what is best in the long term for their tissues. Absent that knowledge and an opportunity to use those methodologies, patients and surgeons historically revert to intuitive, subjective, and outdated methodologies that make them feel temporarily comfortable and “satisfied,” although those same “satisfied” patients report in this study that the methods are only 70 percent accurate. Other issues with this article include the following:

- Statistical analysis does not accord scientific validity to data when survey methodologies and questions were not validated initially.
- References and discussion conveniently omit U.S. Food and Drug Administration premarket approval data that have shown for more than three decades that the methodologies advocated in this article generated consistently excessive reoperation rates of 15 to 25 percent or more.
- Satisfaction of 142 respondents with bra stuffing at a median 12 months postoperative hardly implies any substantive outcomes information that substantively affects patients’ welfare.
- Subjective methodologies for implant sizing are nonscientific, not reproducible, not consistently transferable, and difficult to defend medicolegally because the very definitions on which they are based (cup size) are not definable. Patients cannot be held accountable for decisions using parameters that neither surgeons nor patients can define.
- Forcing breast tissues to a subjectively defined, desired result instead of allowing objective, scientifically validated measurements and methodologies to drive a much more predictable and reproducible decision process and outcomes guarantees tissue and aesthetic compromises.
- The surgeon, not the patient, choosing “most often a C cup, although a D cup may be used in some young nulliparous patients and a B cup in some older and more conservative postpartum patients.” The former is more likely to force nulliparous tissues to a temporary result that may later result in irreversible stretch, tissue thinning, and parenchymal atrophy; and the latter is likely to inadequately fill a parous envelope and produce a “rock-in-a-sock” empty upper breast in the longer term.
- Encouraging patients to believe that they can simply choose what they want, regardless of their quantifiable tissue characteristics and the consequences of their wishes on their tissues in the long term.
- Basing implant size decisions on height, weight, hip width, “personality,” and geographic demographics, when none of those parameters has anything to do with the dimensions and tissue characteristics and limitations of the tissues that exist on the breast.
- Concluding that a methodology is “reasonably accurate,” when 30 percent of sized respondents said

it was not accurate, uses the very data in the article to disprove the article’s conclusions.

- Seeking to resurrect and validate an obsolete, subjective methodology based on indefinable cup size parameters using a 52.5 percent response rate of 142 respondents, 30 percent of whom, despite their supposed satisfaction, reported that the sizing methodology is inaccurate.

The sad story for patients is that surgeons continue to promote four-decade-old subjective methodologies and try to tell the old story with a different (“patient satisfaction”-oriented) twist, when existing peer-reviewed and published studies prove the inadequacies of those outdated methodologies, and especially when other peer-reviewed and published studies, independently monitored, offer methodologies with proven superior processes and outcomes.

Satisfaction? Who is satisfied, when, and compared with what? Seventy percent accuracy may satisfy some patients, but it should not satisfy any surgeon. I trust that informed patients and surgeons will not be satisfied with reverting to four-decade-old methodologies based on a small series of survey respondents at a median 6-month response time when 30 percent of sized respondents stated that the methods did not accurately predict their size.

The authors concluded that “It is expected that future improvements in methodology and equipment will improve the precision of the technique.” I could not agree more, but the “future” was 4 years ago, the required equipment is a measuring tape, and the methodologies and processes are peer-reviewed and published in this Journal.

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REFERENCES

Reply: Bra Stuffing for Implant Sizing?
Satisfaction? Who, When, and Compared to What?

Sir:
Criticism is always welcome as an opportunity for discussion and further clarification of the role of pre-operative sizing in breast augmentation. To begin with, the writer implies that progress in medicine over time is strictly linear and that by that measure “four-decade-old subjective methodologies” must be obsolete. What is truly outdated is the model of the surgeon as an autocratic figure that dictates what is best while ignoring patient input beyond presenting anatomy. The trend today instead is toward personalized medicine. Consistent with that, breast augmentation should be viewed more as a couture procedure that intimately involves the patient rather than a purely scientific process focused largely on efficiency.

We disagree with the notion that “up to three visits to the surgeon’s office...” is a waste of time. If one’s core beliefs include that a breast augmentation lasting more than 24 minutes is inefficient, that each minute step of a surgical procedure should be precisely choreographed without variation, and that implant size selection should not take more than 5 minutes, then the value placed on efficiency is disproportionate. In our experience, seeing patients more than once is mandatory. Patients are often nervous and uncertain at the first visit. They are also usually on their best behavior. Several visits allows the staff, patient, and surgeon to become comfortable with one another (or not) and establish a definitive operative plan.

Although U.S. Food and Drug Administration premarket approval studies may show a 15 to 25 percent reoperation rate, preoperative sizing techniques are not specifically implicated as the source of the problem, as implied. In fact, the vast majority of reoperations today are for capsular contracture, implant malposition, and saline implant deflations. Operations for size change soon after the initial procedure are infrequent, but patient anxiety regarding size immediately after surgery is not. Preoperative sizing has virtually eliminated early postoperative calls in our practice that question size, because the patient has actively participated in the size selection process beforehand. She has experienced a simulation of the final result that includes both appearance in clothes and feeling the weight of the implants, which is not something achievable by tissue analysis and lecturing to the patient alone. Although an imperfect method, it is a far more collaborative tool compared with the practice of having the patient sign multiple consent forms that bind her to taking full responsibility for size before even seeing the surgeon for the first time.

Preoperative sizing does not ignore an analysis of tissue characteristics and breast dimensions. It is an adjunct, albeit an important one, in allowing the surgeon and the patient to collaborate on selecting an implant size that satisfies her wishes while being respectful of the limitations imposed by anatomy. When the sizing process is complete, there is a narrow size range that will be considered during surgery: perhaps two options, compared with presumably only one determined by the “high five” system. This gives the surgeon some latitude during surgery to make the best judgment based on operative conditions, something
the patient willingly accepts and is often grateful for. Our critic has misunderstood the process when he says it allows the patient to “simply choose what they want.” The writer says that “optimally educated patients rarely opt for bra stuffing size selection.” How would he know? Has he tried it? Does he offer it as an option? Better yet, after scientifically applying the high five technique and determining the exact implant size for the patient, why not let the patient try it on beforehand as an additional adjunct in managing the patient’s expectations, not to mention individually verifying a purely numbers-driven size determination? We are baffled by the assertion that a patient’s height, weight, hip width, personality, and even geographic demographics have nothing to do with implant size selection. Even the most doctrinaire methodology must reveal many instances where more than one size will be compatible with a patient’s tissue characteristics. These other factors are important determinants in final size selection within the range permitted by the patient’s anatomy. Not to consider factors other than breast anatomy ignores the patient as a whole. Furthermore, these factors speak to the artistry involved in breast augmentation, an element that is not served by a purely numbers-driven technique that does not focus outside of the breast base diameter.

It is well understood that too large an implant can result in late tissue stretch and its sequelae. Most of the implants that we use are between 250 and 350 cc, and in postpartum patients, they are typically less than that. We rarely use sizes that begin with a 4, a practice that minimizes late stretch problems. It is also axiomatic that sufficient upper pole fill is a key goal in the postpartum patient. We disagree, however, that a postpartum patient must always be larger than a B cup to achieve this.

To be clear, preoperative sizing is not a precise method and is of course subjective. Improvements in the technique would be helpful and possibly forthcoming. We do not believe that the ongoing advances in three-dimensional patient photography with implant size simulation are the answer. There is no substitute for the patient trying on different sizes and visualizing the effect in clothing and experiencing the implant weight. The method is very instructive in revealing the patient’s aesthetic vision in a way that dictating a size based on tissue characteristics alone can never do.

Finally, what can we say to the individual who makes vociferous arguments supported only by his own publications? It logically follows that the ideas of others will not be considered without strong prejudice. We have not witnessed the cognoscenti in plastic surgery today taking up the charge of perfecting choreographed surgery, using the high five system, permitting their patients to go out for dinner, shopping, and dancing on the day of surgery,1 or replicating the perfect record of 50 consecutive breast augmentations without a single instance of reoperation (itself a gift to the plaintiff’s bar). Our system operates on a different value system that fosters a collaborative bond between the patient and surgeon, embraces the role of artistry beyond scientific analysis alone, and pursues a unique solution for each patient.

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REFERENCES

Where Are the Data?

Sir:

In “Preoperative Sizing in Breast Augmentation” (Plast Reconstr Surg. 2010;125:1781–1787), the authors’ conclusion that “sized patients were more satisfied than controls” was not supported by their data. Each of their three endpoints, “would prefer a different size,” “procedure satisfaction average,” and “had size change,” was statistically insignificant. Moreover, 16 percent of the sized patients “would prefer a different size,” and 30 percent felt the sizing was inaccurate. Far from supporting their conclusion, such data repudiate it, and with a follow-up of only 34 percent in the controls and 53 percent in the study group, one cannot conclude that the 21 percent (versus 16 percent) that preferred a different size and the 4.2 (versus 4.5) satisfaction average represented even a mild trend. The greater length of follow-up for the controls might alone explain the difference in the reoperation rate (21 months versus 12 months).

The same surgeon performed surgery on both groups successively, not concurrently. It is unimaginable that he would not have learned lessons managing the unhappy control patients who influenced his subsequent counsel to the study patients. Thus, from the beginning, this study was not designed to reasonably isolate the variable of using preoperative implant sizers. Saying that tissue-based planning “represents a fait accompli without participation beyond her anatomy” is a
common misunderstanding. To the contrary, the patient participates demonstratively. She tells the doctor she wants an implant that ideally fills her breast, leaving the upper pole neither empty nor bulging. The surgeon uses a measurement system to achieve that very goal.

The questionnaire should have asked whether patients were happy with the “fill” of their breasts. Whether or not a patient mentions a specific size, they always make a request regarding whether or not they want to be natural. Implant volume affects not just size but also fill and thereby shape. Failing to ask a follow-up question about satisfaction with fill misses at least half of what is relevant about implant volume.

Asking a patient whether she wants to be a different size is relevant only if they are also asked about whether they would accept the corresponding consequences. Those wanting smaller should be asked whether they would still do it if it meant being empty and underfilled; those wanting to go larger should be asked whether they would change if it meant being unnaturally bulging in the upper breast and perhaps more stretched over time.

This concept is ingrained in patients sized with a tissue-based system. However, when an exercise such as this is performed at the beginning, by definition patients are led to believe that they can pick their size on the basis of their wishes on the days of their sizing visits. This sets them up to second-guess their earlier decision or to even change their mind, always leaving the door open to a revision for size.

Finally, the complete recipe for this article was not given to readers. It described how the patients used the sizers, but it did not describe the critical roles of the nursing staff (how they decided what bra size to give and whatever else they said during the visits) and the surgeon (how he determined the limitations of their tissues). No reader of this article has reason to believe that they can copy what was described in this article and achieve similar results themselves.

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Reply: Where Are the Data?

Sir:

As this writer appears philosophically aligned with the previous one, our response to that letter generally applies to this one as well. We would like to focus instead on some of the different questions raised.

The assertion that lessons in patient management learned from unhappy control patients somehow biased the results from the experimental group that followed is not true. In fact, it was the practice of using a consistent approach and still experiencing a small percentage of emotionally charged wrong-size situations postoperatively that led to the need to do things differently. Having now done that, and becoming a bit smarter in the process, we would never go back to practicing breast augmentation without the benefits that preoperative sizing techniques offer.

We disagree that the questionnaire was faulty because it did not specifically ask about fill. Patients with wrong-size issues in our experience like their breasts after surgery but just want them larger in a global sense (no pun intended). They do not isolate their complaints specifically to a “fill” issue.

We also completely disagree that a patient wishing for a smaller size is condemned to an empty upper pole or that those wishing for a larger size are equally condemned to late tissue stretch problems. There are usually solutions in both scenarios that are not extreme enough to cause these problems. We feel instead that these are “come-from-behind” arguments offered to the unsatisfied patient that has not been given the opportunity to be a partner in size selection in the first place.

This writer also misunderstands the preoperative sizing process. The patient is not allowed to unilaterally decide what size she wants. She is given a range to try on that is consistent with both her breast anatomy and her body habitus. The patient is counseled during the process when she strays outside of the range that the surgeon believes he can deliver. In this way, preoperative sizing serves a useful adjunct, not as a process that overrides anatomical constraints.

It is true that the patient does sometimes change her mind or cannot decide what she wants. That is why we repeat the process on a separate day when such issues arise. More time spent with the patient usually resolves the problem. Sometimes, patients cannot decide between a 25-cc difference in size, which is quite understandable. In these instances, the patient willingly cedes control to the surgeon to make the final decision intraoperatively, a process aided by visualizing both options in situ using sizers. In our experience, patients do not wander over a wide size range even when uncertain.

Finally, the role of the nurses can be clarified. The nurses are given a starting range to work with that is dictated by the surgeon following patient examination. Bras are selected that best accommodate these sizes for the individual patient. Factors such as breast position on the chest (both vertically and transversely), skin quality, breast base diameter, existing breast volume and its anticipated influence on tissue compliance, nipple position, areolar diameter, inframammary crease location relative to the inferior areolar margin, inframammary crease configuration, body habitus, height, patient goals, and of course any ptosis issues are among those considered in setting the volume and diameter parameters for the sizing process.

Although not perfect, preoperative sizing is effective. Moreover, the ultimate responsibility for size selection is shared between the patient and the surgeon using this collaborative method. As stated in the article, this has made breast augmentation a much more “uni-
formly rewarding and positive experience for both the patient and the physician.”
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Spector regarding preoperative sizing in breast augmentation. Although many techniques have been described as effective in accomplishing this task from a technical standpoint, it is the concept of making the effort at all that I would like to support and emphasize. This well-written article outlines many of the challenges all plastic surgeons face as they try to balance the wish...
breasts. This information can be used for different purposes, as follows:

1. Indication of the volume of the implant to use on the smaller breast if unilateral augmentation has been planned.
2. Indication of the volume difference to keep in the implant size choice if differential bilateral breast augmentation has been planned.
3. Indication of the volume of reduction from the larger breast required to achieve volume symmetry if unilateral breast reduction has been planned (Figs. 1 and 2).
4. Indication of the volume difference to maintain between the two reduction specimens where bilateral differential breast reduction is indicated.

The first author (F.S.) has used this technique for preoperative assessment of breast asymmetry over the past 10 years in 47 consecutive patients, achieving good results in terms of symmetry. The advantage of this procedure is that it is simple, easily adapted, and very cost effective. It is clear that, if required intraoperatively, appropriate changes to the preoperative measurements should always be made.

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REFERENCES


Preoperative Sizing for Breast Augmentation

Sir:

Drs. Hidalgo and Spector demonstrate a valuable adjunct technique for preoperative sizing in breast augmentation in the June of 2010 issue of Plastic and Reconstructive Surgery. Their approach is a patient-centric process that allows the patient to be the key decision-maker in a critical step in the implant-selection process. Unfortunately, the study design has significant flaws, including use of a nonvalidated questionnaire and use of small cohorts that yield insignificant numbers for statistical analysis. These shortcomings of this
“scientific article” should not deter the reader from understanding that this important patient-centric process is extremely valuable; however, “preoperative sizing” must be coupled with objective data from measurements and assessments of the patient’s chest wall; breast skin; tissue quality, quantity, and adherence; and nipple-areola position, size, and shape.

The finesse in the preoperative implant-selection process evolves as the plastic surgeon allows the patient to actively participate in the process while only offering patients implants to use for sizing that are tissue and dimensionally appropriate in width, height, projection, and volume so that the implant ultimately selected will fulfill the patient’s preexisting soft-tissue and chest wall requirements. Many patients will only reveal their unrealistic expectations through this type of patient-centric preoperative sizing process. Three-dimensional imaging may also help to evaluate patient expectations; however, the software is costly.

To achieve high rates of patient satisfaction and low reoperation rates for size dissatisfaction, the plastic surgeon must actively involve the patient. This method does provide the patient with that important decision-making power, but it must be mastered by the plastic surgeon who has measured and evaluated the objective data from the clinical examination so that the patient is guided to choose the appropriate implant that will safely fill her unique breast dimensions yet provide her with an aesthetic result with which she is satisfied. As always, as plastic surgeons, we must not only merge scientific data with the artistry of plastic surgery but also practice the art of medicine in our approach to patient care. Bravo to Drs. Hidalgo and Spector for merging scientific data with the artistry of plastic surgery and satisfied. As always, as plastic surgeons, we must not only fulfill the patient’s preexisting soft-tissue and chest wall requirements. Many patients will only reveal their unrealistic expectations through this type of patient-centric preoperative sizing process. Three-dimensional imaging may also help to evaluate patient expectations; however, the software is costly.

To achieve high rates of patient satisfaction and low reoperation rates for size dissatisfaction, the plastic surgeon must actively involve the patient. This method does provide the patient with that important decision-making power, but it must be mastered by the plastic surgeon who has measured and evaluated the objective data from the clinical examination so that the patient is guided to choose the appropriate implant that will safely fill her unique breast dimensions yet provide her with an aesthetic result with which she is satisfied. As always, as plastic surgeons, we must not only merge scientific data with the artistry of plastic surgery but also practice the art of medicine in our approach to patient care. Bravo to Drs. Hidalgo and Spector for reminding us that the patient’s desires are a critical part of this implant-selection process.

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Study Suggests Trend toward Greater Donor-Site Morbidity in TRAM Flap Patients

Sir:

We read with interest the article by Chun et al., “Comparison of Morbidity, Functional Outcome, and Satisfaction after Bilateral TRAM versus Bilateral DIEP Flap Breast Reconstruction,” in the June of 2010 Supplement. There is a growing body of quality-of-life/outcomes research in the literature but a limited understanding of how to conduct and interpret results from these very important studies. For example, this abstract concluded “no significant differences in donor-site morbidity...between bilateral TRAM and DIEP flap breast reconstruction,” despite the fact that none of the instruments used was designed to measure donor-site morbidity. Furthermore, the authors did not report study power, so it is impossible to know whether the sample size of 105 transverse rectus abdominis musculocutaneous (TRAM) flap patients and 58 deep inferior epigastric artery perforator (DIEP) flap patients was adequate to detect a difference between the two groups.

The instruments chosen by the authors did not seem appropriate for their conclusions—it was as if they were trying to measure sternal notch-to-nipple distance with a compass instead of a ruler. The Medical Outcomes Study 36-Item Short Form Health Survey is a generic quality-of-life instrument designed to compare broad areas of health-related quality of life, such as differences in cancer versus noncancer patients. Questions along the lines of “how would you rate your general health now?” lack the sensitivity to identify differences between TRAM and DIEP flap breast reconstruction. Although the Functional Assessment of Cancer Therapy—Breast is disease-specific to breast cancer, it is not surgery-specific—questions are geared more toward chemoradiation treatment and other factors (e.g., “Do you have nausea?”). The Michigan Breast Satisfaction scale examines general and aesthetic satisfaction but is not calibrated to assess donor-site morbidity. According to Pusic et al., “Without proper measurement tools, study results are considerably less meaningful. That is, if a study produces results based on an instrument that cannot be shown to measure what it is intended to measure in a consistent and reproducible fashion, then the conclusions drawn cannot claim to be reliable or valid.”2

Furthermore, the authors do not report study power, but their low number of patients draws into question whether the sample size is large enough to detect a difference between the two treatment groups. A study with inadequate power can lead to dangerously erroneous conclusions. For example, multiple studies have shown 90 percent risk reduction in BRCA-positive patients who undergo bilateral prophylactic mastectomies3-5; however, if the sample size were too low, the study would lack the power to detect a difference in risk reduction. It would be disastrous if an underpowered study determined health care policy for BRCA-positive women—and prevented patients from obtaining coverage for life-saving treatment. Likewise, it would be tragic if an underpowered study blocked women from obtaining coverage for DIEP flap breast reconstruction and forced patients to undergo an operation with greater potential donor-site morbidity. The study by Chun et al. of bilateral TRAM versus DIEP flap surgery reports that abdominal bulges occurred in three TRAM and 58 deep inferior epigastric artery perforator (DIEP) flap patients was adequate to detect a difference between the two groups.

The instruments chosen by the authors did not seem appropriate for their conclusions—it was as if they were trying to measure sternal notch-to-nipple distance with a compass instead of a ruler. The Medical Outcomes Study 36-Item Short Form Health Survey is a generic quality-of-life instrument designed to compare broad areas of health-related quality of life, such as differences in cancer versus noncancer patients. Questions along the lines of “how would you rate your general health now?” lack the sensitivity to identify differences between TRAM and DIEP flap breast reconstruction. Although the Functional Assessment of Cancer Therapy—Breast is disease-specific to breast cancer, it is not surgery-specific—questions are geared more toward chemoradiation treatment and other factors (e.g., “Do you have nausea?”). The Michigan Breast Satisfaction scale examines general and aesthetic satisfaction but is not calibrated to assess donor-site morbidity. According to Pusic et al., “Without proper measurement tools, study results are considerably less meaningful. That is, if a study produces results based on an instrument that cannot be shown to measure what it is intended to measure in a consistent and reproducible fashion, then the conclusions drawn cannot claim to be reliable or valid.”2

Furthermore, the authors do not report study power, but their low number of patients draws into question whether the sample size is large enough to detect a difference between the two treatment groups. A study with inadequate power can lead to dangerously erroneous conclusions. For example, multiple studies have shown 90 percent risk reduction in BRCA-positive patients who undergo bilateral prophylactic mastectomies3-5; however, if the sample size were too low, the study would lack the power to detect a difference in risk reduction. It would be disastrous if an underpowered study determined health care policy for BRCA-positive women—and prevented patients from obtaining coverage for life-saving treatment. Likewise, it would be tragic if an underpowered study blocked women from obtaining coverage for DIEP flap breast reconstruction and forced patients to undergo an operation with greater potential donor-site morbidity. The study by Chun et al. of bilateral TRAM versus DIEP flap surgery reports that abdominal bulges occurred in three TRAM flap patients and no DIEP flap patients. If anything, this suggests a trend toward greater donor-site morbidity in TRAM flap patients than in DIEP flap patients, which could become statistically significant in a study with adequate power.

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REFERENCES


Reply: Study Suggests Trend toward Greater Donor-Site Morbidity in TRAM Flap Patients

Sir:

We are not too surprised that the presentation of our study results has already generated some controversy. However, it is somewhat unusual to have such strong criticism leveled in a formal Letter to the Editor based on a summary abstract and before actual publication of the full article. Nevertheless, given the Journal’s decision to publish this letter, we are obliged to attempt a considered response.

The major criticism by Dr. Chen appears to be the absence of a formal power analysis. Although we acknowledge that such analyses are certainly useful, we do not believe that they are essential for every retrospective study. The purpose of a power analysis is to assess the probability that a study will make a type II error (i.e., fail to identify a true difference between groups). It is generally accepted that such an analysis is essential for planning any prospective study that involves possible harm to study subjects or use of substantial resources (e.g., financial costs or time). However, a formal a priori power analysis is not required before conducting a retrospective analysis. Bayesian statistical principles consider all data from properly conducted studies as valid regardless of the formal power of the study. In many cases, a collection of smaller studies considered together allows one to draw reasonable conclusions regarding questions that would otherwise never be answerable through a single study. For example, it is easy to perform a post hoc power analysis of our study. For a given complication with an estimated rate of 3 percent, such an analysis indicates that to perform a study with 80 percent power (standard threshold) to detect a 50 percent reduction in the complication rate would require a total study size of 5034 patients. It is unlikely that such a study will ever be conducted, although as the number of reported series such as ours increases, the potential for a meaningful meta-analysis also increases. Moreover, requirement of such a large sample size to find a statistical difference in the outcome of interest reinforces the fact that the observed complication rate is low.

The second criticism is that inappropriate outcome instruments were used in our study. However, in their letter, Dr. Chen and her colleagues failed to suggest a more appropriate alternative. In fact, there are no validated outcome measures specifically designed to assess donor-site morbidity. Clinical outcome instruments cannot simply be concocted out of thin air, and this is the reason that relatively few exist and that they tend to be designed to address major public health issues or are intended to be as generalizable as possible. This is precisely the reason we selected a combination of a general quality-of-life instrument such as the Medical Outcomes Study 36-Item Short Form Health Survey and a validated breast-specific instrument, the Functional Assessment of Cancer Therapy—Breast. Since the time of our study, a new patient-reported outcome instrument specific to breast surgery, the BREAST-Q, has been developed, and we plan to incorporate this valuable tool into our future clinical studies involving reconstructive breast surgery.

Finally, although our study is small and potentially underpowered, we believe that the idea that missing a small reduction in abdominal bulges is akin to missing a significant increase in death from breast cancer, as Dr. Chen and her colleagues appear to be implying, is somewhat absurd and unnecessarily sensationalistic. An arguably more serious mistake would be inappropriately allocating large amounts of limited health care resources for much more costly deep inferior epigastric artery perforator (DIEP) flap reconstructions when the superiority of this procedure versus transverse rectus abdominis musculocutaneous (TRAM) flaps remains largely unproven.

On a separate note, although we do not believe that this applies in the case of Dr. Chen and her colleagues, we are aware that for some, the issue of DIEP versus TRAM flap reconstruction may be unintentionally
Marcaine for Augmentation Mammaplasty

Sir:

I have read the article by Dr. Pacik entitled “Pain Control in Augmentation Mammaplasty: The Use of Indwelling Catheters in 813 Consecutive Patients” (Plast Reconstr Surg. 2010;125:1814–1815) and would like to point out that other authors have devised similar and simpler ways of controlling pain after this surgery.1,2 These previous, relevant published articles were citations omitted from Dr. Pacik’s article.

Despite the low infection rate reported by Dr. Pacik with the use of indwelling catheters, I have found that simply putting 10 cc of 0.5% bupivacaine (Marcaine; Hospira, Inc., Lake Forest, Ill.) into the dissected pocket of an augmentation mammaplasty before inserting the implant takes care of postoperative pain in most of my patients and eliminates entirely the need for an indwelling catheter and later injection. In some hands, use of an indwelling catheter could lead to a higher infection rate. This technique was published in Plastic and Reconstructive Surgery in August of 2003.2 My article was written in response to Dr. A. Weiss, who used Marcaine in his tumescent injections.1

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DISCLOSURE

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previously. I would add that except for approximately 50 augmentation patients, no prophylactic antibiotics were used in any of my patients unless there were well-defined needs. These 50 patients had surgery during a year in which I used intravenous Kefzol preoperatively because of concern about malpractice suits and questions about standard of care. In this time frame of using prophylactic antibiotics, two patients developed allergic cephalosporin hepatitis and were very sick for approximately 3 months. Therefore, I discontinued prophylactic antibiotics; 50% povidone-iodine solution irrigation has become a religion in the operating room for all cases, large or small, dating back to approximately 1985. Irrigation with this solution is performed periodically throughout the procedure and again before and during closure. My long-term overall infection rate has been close to zero. In the catheter study, 5 cc of 50% povidone-iodine solution is instilled into each indwelling catheter after closure to test the patency of the indwelling catheter. I am not aware of any complications that can be attributed to the use of 50% povidone-iodine solution. Complications of antibiotic use are well known. Unexpected benefits of the indwelling catheter system include the following:

- Irrigation of the catheters with saline when bloody drainage is noted the following morning. The catheters are flushed until a clear return is noted.
- The ability to maintain the catheters for a second day for patients requesting continued pain control (approximately 15 percent of patients).
- Monitoring the amount of bleeding in patients with von Willebrand disease.

Reoperation for hematoma has not been necessary in any of the 852 patients with indwelling catheters. These Letters serve as an excellent forum for presenting preliminary ideas, some of which lead to more sophisticated evidence-based studies. I thank Dr. Rosenblatt for the opportunity to further discuss the importance of postoperative pain control in augmentation mammoplasty.

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REFERENCES

Use of the Subpectoral Fascia Flap for Expander Coverage in Postmastectomy Breast Reconstruction

Sir:

We read with interest the recent articles by Dr. Saint-Cyr and colleagues regarding the use of serratus anterior fascia and/or subpectoral fascia for expander coverage in postmastectomy breast reconstruction. As they highlighted, a key point in the two-stage prosthetic breast reconstruction is the expansion of the lower breast pole to achieve a pleasant breast shape once the definitive implant is placed.

The authors underscored the importance of the pedicled serratus anterior/subpectoral fascia flap for inferolateral expander coverage and to prevent expander/implant lateralization. The technique reported would represent an alternative to muscle flap expander/implant coverage (with their higher donor-site morbidity) and to the use of allograft because of the cost and the increased risk of seroma formation. However, the authors are using a pedicled serratus anterior/subpectoral fascia flap for the lateral expander/implant coverage and the acellular dermal graft for inferior implant coverage.

In the thoracic wall, we have to distinguish two different fascial layers that are at different depths at some levels and fuse at other levels. In this setting, we distinguish the pectoralis major fascia (more superficial) and the deeper coracoacromioaxillary or axillary fascia enveloping the pectoralis minor, the subclavian, and the coracobrachial muscles and overlying the serratus anterior.

The pectoralis major fascia consists of the superficial pectoralis fascia on the outer part of the pectoralis major, which continues caudad overlying the rectus sheath. The deep pectoralis fascia is located on the inner part of the pectoralis major and terminates at its inferior border. At the lateral border of the pectoralis major, the superficial pectoralis fascia and deep pectoralis fascia fuse, and overlie the axillary fascia, creating a unique fascial system.

Thus far, the terms “serratus anterior” and “subpectoral fascia” represent a new nomenclature given to regional subdivisions of an already well-defined fascia, the coracoacromioaxillary or axillary fascia. We share with the authors the belief in the importance of the thoracic fascias in defining a partial submuscular pocket for postmastectomy implant reconstruction. However, we use a different approach.

In our technique, the expander/implant is placed in the submuscular-subfascial pocket (Fig. 1). At the inferior edge of the pectoralis major, we undermine the superficial pectoralis fascia in continuity with the pectoralis major itself, up to the inframammary fold. At this level, the superficial pectoralis fascia is cut to release the fascial tension. In this way, we define the condition for the major expansion in the lower pole. No further coverage is needed in the inferior pole and laterally. In skin expander reconstructions, the skin at the lower pole is raised in an inferior direction, and the expander is inserted in a submuscular-subfascial pocket.
pole will expand as the implant is filled. In case of skin-preserving mastectomy, when staged skin expansion is unnecessary, the major expansion is immediately gained at the lower pole when the definitive implant is placed.\(^4\)\(^5\)

In conclusion, the thoracic fasciae should be taken into consideration as valuable structures that allow an easy and pleasant breast reconstruction, with few complications.

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DISCLOSURE

The authors have no financial interests to disclose.

REFERENCES


Reply: Use of the Subpectoral Fascia Flap for Expander Coverage in Postmastectomy Breast Reconstruction

**Sir:**

I would like to thank Dr. Marzia Salgarello and colleagues for their interest in “Use of the Serratus Anterior Fascia Flap for Expander Coverage in Breast Reconstruction” (*Plast Reconstr Surg.* 2010;125:1057–1064). Dr. Salgarello et al. describe the anatomical highlights of the superficial and deep pectoralis major fascia, and the axillary fascia, which wraps the pectoralis minor, subclavian, and coracobrachial muscles and also covers the overlying serratus anterior. I use the terms serratus fascia and subpectoral fascia, which are synonymous with the axillary portion of the fascia covering the serratus and the deep pectoral fascia, respectively, for

![Fig. 1. Intraoperative view of an immediate definitive implant reconstruction after bilateral skin-reducing mastectomy with a T pattern. The mastectomy flaps are everted. The submuscular-subfascial pocket is shown. It has been dissected through the upper lateral border of the pectoralis major muscle (PM). The asterisk indicates the textured anatomical implant in place. The dotted line indicates the point of merging of the superficial pectoralis fascia (SPF) with the axillary fascia overlying the serratus anterior muscle. The fascial planes allow definition of the submuscular-subfascial pocket that is separated from the mastectomy plane.](image-url)
simplicity and clarity when describing both of these fascial flaps. The subpectoral fascia, in essence, corresponds to the deep pectoral fascia, as mentioned by Salgarello et al., and the serratus fascia corresponds to the serratus fascia portion of the axillary fascia or coracoacromial axillary fascia. The authors describe an innovative technique in which the inferior edge of the pectoralis major muscle is undermined in continuity with the superficial pectoral fascia of the inframammary fold. This can be performed when the soft tissue overlying the infra mammary fold is intact and acts as a bridge or as a link between the inframammary fold and the inferior edge of the pectoralis major muscle. The caudal portion of the fascia overlying the pectoralis muscle is then cut at the inframammary fold level, leaving subcutaneous tissue attachment only, linking the inframammary fold to the inferior edge of the pectoralis major muscle. As the authors described, this allows distention and relaxation of the pectoralis major muscle, because of a fasciotomy of the superficial pectoral fascia at the inframammary fold level. Again, this is made possible only if adequate soft tissue is present following mastectomy from the inframammary fold up to the pectoralis major muscle. This technique is a very elegant demonstration of use of the fascial system overlying the pectoralis major muscle and serratus muscle, with applications for either immediate implant- or expander-based breast reconstruction. This technique also underscores the importance of considering local tissue options before allo graft when considering any implant-based breast reconstruction. The use of local tissue helps maximize vascularity around the expander implant and helps diminish costs associated with reconstruction. Again, I am very much in agreement with this technique, and when continuity between the inframammary fold and the pectoralis major muscle is preserved, I abort the use of dermal allo graft and use only an autologous-based expander reconstruction. It is only in cases where this continuity has been disrupted and the lower portion of the pectoralis major muscle has been completely denuded from its overlying subcutaneous tissue that dermal allo graft is used to restore continuity with the inframammary fold. Once again, I would like to thank Dr. Salgarello and colleagues for their interest in my previously published articles on the subpectoral fascia and serratus fascia flap for expander-based reconstruction, and I would like to congratulate them for their innovative use of the superficial pectoral fascia flap in breast reconstruction.

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DISCLOSURE

The author has no financial conflicts of interest to disclose.

Oncoplastic Breast Surgery in Britain

Sir:

We read with interest the letter by Dr. Elias entitled “Oncoplastic Surgery Articles” and Dr. Nahabedian’s reply, published in Plastic and Reconstruc
tive Surgery (2010;125:1294–1295). In Britain, plastic surgical trainees are not routinely exposed to breast oncology and would not always be confident managing the oncology side. Although the concept of oncoplastic breast surgery was introduced earlier in Europe, the U.K. government only started funding these 1-year training posts in 2002.1 Traditionally, we have had nine highly competitive National Oncoplastic Breast Fellowships in the United Kingdom. These posts are open to both general surgical and plastic surgical senior specialist registrars (residents), who have a special interest in breast oncology and reconstruction. Over time, these fellowships have evolved and have now become established training programs. As with many new things, there are pros and cons to these posts, which may phase out over time. On the surface, it may seem like a backward step to have all procedures performed by the same surgeon, which was the case before specialization and superspecialization. However, in reality, it is becoming a superspeciality of its own and thus should be viewed as a step forward. The concept of oncoplastic breast surgery is here to stay and continues to grow. In the United States, the American Society of Breast Surgeons is becoming the fastest growing society in the country, with 33 Society of Surgical Oncology Breast Fellowships.2 We agree with Dr. Losken and Dr. Nahabedian3 that we as plastic surgeons need to grow alongside it, rather than competing against it. This will enable us to practice in the same way as Dr. Elias, working as an oncoplastic breast and plastic surgeon. Moreover, it also provides the chance to exchange new ideas across the different specialities, thus enabling the development of innovative techniques. As for the duration of the fellowships, this is currently under review. It is thought that for some of the fellows, a period of 1 year might not be enough to encompass the full spectrum of the speciality, which was previously a concern raised by Dr. Spear.3 We are both trained and qualified as plastic surgeons and have also completed the National Oncoplastic Breast Fellowship. Change is the only constant in this universe, and only time will tell the true value of the concept of these “oncoplastic breast surgeons.”

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REFERENCES

Dominant Radial Artery Perforator in the Proximal Forearm

Sir:
I read with interest the article “The Radial Artery Pedicle Perforator Flap: Vascular Analysis and Clinical Implications” by Michel Saint-Cyr et al. (Plast Reconstr Surg. 2010;125:1469–1478) that reports an elegant study of radial artery cutaneous perforators and their clinical importance with regard to the radial artery pedicle perforator flap.

By identifying two important radial artery perforator clusters, in the proximal and distal thirds of the forearm, safer harvest and design of the radial forearm pedicle perforator flap is facilitated. Although at least one dominant perforator (>0.5 mm) is found in the distal cluster of perforators, the presence of dominant perforators in the proximal cluster was not mentioned in the anatomical study.1

Fasciocutaneous and adipofascial flaps from the proximal forearm are useful in arm, elbow, and head and neck reconstruction. Knowledge of the presence and site of dominant perforator arteries in the proximal forearm would ensure better planning of flap harvesting. Lamberty and Cormack2 described the inferior cubital artery that arises from the intermuscular septum between the brachioradialis and pronator teres 4 cm below the midintercondylar point that runs obliquely inferolateral to Lister’s tubercle on the dorsum of the radius. Injection studies suggest that this vessel has a cutaneous angiotome larger than other perforating cutaneous vessels in the forearm.

More recently, Hwang et al.3 identified this perforator in all but one of a total of nine forearm specimens dissected, supplying an average cutaneous territory of 19.92 cm². Tiengo et al.4 looked at 16 cadaveric forearms and described this as the most proximal perforator branch of the radial artery, originating from its superficial aspect at a mean distance of 2.4 cm from the origin of the radial artery. The diameter of this artery is reported to vary between 0.5 and 1.5 mm.3

Several studies have reported that the size of radial artery perforators decreases as we travel distally down the forearm,1,3 suggesting that smaller flaps should be raised distally compared with the proximal forearm. Improved knowledge of these perforators would have important clinical implications, especially when free flaps are considered and perforator size and their cutaneous territory are important factors.

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Reply: Dominant Radial Artery Perforator in the Proximal Forearm

Sir:
We thank Dr. Kuen Yeow Chin for his comments and interest in our recent article “The Radial Artery Pedicle Perforator Flap: Vascular Analysis and Clinical Implications.”1 Dr. Kuen Yeow Chin highlights the usefulness of proximally based radial forearm perforator flaps for local coverage (pedicle) or head and neck reconstruction (free). Although we did not find a consistently dominant perforator in the proximal forearm, dissection of 639 perforators revealed that 240 were clinically relevant (≥0.5 mm diameter). Of these, two main clusters were identified. There are two main clusters of clinically relevant perforators at a relative distance of 17.6 ± 11.5 percent (distal cluster) and 61.7 ± 14.8 percent (proximal cluster) along the radial styloid–to–lateral epicondyle interval. The two cluster locations in essence represent pivot points for potential pedicle perforator flap designs proximally or distally. These are also conveniently located where clinically relevant upper extremity wounds most often occur.

The proximal cluster of perforators has been discussed by Lin et al. in Taiwan and has served as the basis for radial artery perforator free flaps in 14 head and neck reconstruction cases.2 More recently, Tiengo et al.3 have reported an anatomical study on the proximal radial artery perforator flap. The authors found that the first four proximal perforator arteries originated...
within a mean distance of 4.3 cm from the origin of the radial artery, with a 95 percent confidence interval of 3.8 to 4.8 cm. As muscle mass size increases in the proximal forearm, many of the perforators encountered will be musculocutaneous and will have a greater length compared with their distal septocutaneous counterparts (Fig. 1). Source arteries are more deeply located proximally, and their cutaneous perforators will have a greater distance to travel to the skin. The same holds true for perforators in the proximal versus distal thigh or proximal versus distal leg. Proximal perforators also provide blood supply to a larger volume of tissue, which thus explains their potential greater size compared with distal perforators, which mostly provide blood supply to the skin (Fig. 2). As further anatomical studies emerge, so will our increasing knowledge and experience with regard to the perforator radial forearm flap. We are thankful again for the useful comments provided by Dr. Kuen Yeow Chin.

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Islanded Posterior Tibial Artery Perforator Flap for Lower Limb Reconstruction: Review of Lower Leg Anatomy

Sir:

We read with great interest the recent article entitled “Lower Limb Reconstruction Using the Islanded Posterior Tibial Artery Perforator Flap,” by Dr. Mark Schaverien et al.1 We congratulate the authors on their article, which provides a review of the anatomy, their surgical technique, and indications for use of the posterior tibial artery perforator flap.

This article prompted a lengthy discussion among plastic surgeons at our institution. However, our review of lower limb anatomy differs slightly from that described by the authors in this article. In 1997, the posterior tibial artery perforators and their fasciocutaneous territories were described by Whetzel et al.2 A number of studies have described the lower limb arterial supply, including the anterior tibial, peroneal, sural, and posterior tibial artery perforators. Schaverien and Saint-Cyr published a detailed anatomical study of...
the lower extremity perforators using arteriovenous latex cure and cadaver dissection in 1998. In this article, the authors describe the posterior tibial artery perforators as “arising from within the intermuscular septum between the soleus and the flexor hallucis longus.” According to our review of the above studies and anatomy textbooks, the posterior tibial artery traverses the deep posterior compartment of the lower leg. The posterior tibial artery perforators and their two associated venae comitantes pass through two intermuscular septae: between the soleus and flexor digitorum longus; and the medial aspect of the tibia and the flexor digitorum longus (not the flexor hallucis longus, which lies lateral to the posterior tibial artery).2–4

The islanded posterior tibial artery perforator flap does seem to be a reliable, versatile fasciocutaneous flap for use in reconstruction of lower limb soft-tissue defects. However, as with any flap, it is vital to have accurate knowledge of the vascular and musculoskeletal anatomy of the flap and surrounding tissue before proceeding with such an endeavor. In addition, it would be interesting to determine whether there are any differences in complication rates based on the location of injury (e.g., lower leg, ankle, heel).

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DISCLOSURE
The authors have no financial interests with respect to the content of this communication or the article being discussed.

REFERENCES

Reply: Islanded Posterior Tibial Artery Perforator Flap for Lower Limb Reconstruction: Review of Lower Leg Anatomy

Sir:

We would like to thank Drs. Jack, Newman, and Barnavon for their communication regarding our article “Lower Limb Reconstruction Using the Islanded Posterior Tibial Artery Perforator Flap” (Plast Reconstr Surg. 2010;125:1735–1743). We would like to indeed confirm that the perforators from the posterior tibial artery emerge from between the flexor digitorum longus and the soleus, and between the tibia and the flexor digitorum longus in the distal third. In our experience and according to anatomical dissections, the periosteal perforators tend to be smaller than those passing between the flexor digitorum longus and the soleus, and we base our flap on the larger perforators emerging between the muscles. Manchot noted that “larger cutaneous arteries, as a rule, appear from the fissure between the soleus and flexor digitorum longus muscles.”1

In our experience, we have not found differences in flap complication rates based on the location of the injury. Where the propeller design is used, it is important to adequately mobilize the perforator to allow the flap to rotate without compromising the venae comitantes. We hope that this information will be useful to the authors when embarking on harvesting this flap and thank them very much indeed for raising the above points.

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REFERENCE

Perforasomes

W e would like to compliment Dr. Sanit and his team for proposing the concept of the perforasome.1 Mentioned in the article, the concepts of perforasome and “linking vessels” between adjacent perforasomes give evidence to our study in 1998 that
the perforators have a chain-link vessel in the adipose layer. Based on the advanced anatomical and graphic methods, the authors propose the concept of the perforasome. We consider it to be great progress after the “angiosome” concept of Taylor; however, we have a few questions.

1. Priority of perfusion. The authors describe two forms of linking vessels: Direct linking vessels travel within the suprafascial and adipose tissue layers. Indirect linking vessels communicate with adjacent perforators by means of recurrent flow through the subdermal plexus. There are communicating branches between them. On dynamic computed tomographic angiography, we note the recurrent flow from the subdermal plexus into a communicating branch that connects with a large direct linking vessel (Video 3). Does this video indicate that indirect linking vessels are flooded more easily than direct linking vessels? It is important to understand which type of vessel has priority of perfusion, especially when we design an adipofascial flap without skin.

2. Pressure of perfusion. There is no detailed value of pressure mentioned in this article. We noted that high perfusion pressure through the single perforator opens multiple direct and indirect linking vessels and allows perfusion of four or five perforasomes, as shown in Figure 5. However, by clinical practice, we found that even if the flap is designed according to the linking vessel, the distal part of a long flap has a poor blood supply. Thus, we want to define the area that can be filled by a single perforator by physiologic pressure.

3. Neurovascular axis. Our investigation and many other studies have confirmed that if the superficial sensitive nerves are incorporated in the subcutaneous adipose tissue, the perforators will give specific branches to these structures to form a paraneural vascular plexus to nourish them (i.e., a distally based sural neurofasciocutaneous flap including the sural nerve component could be designed and used for repair of soft-tissue defects in the distal lower limb). We look forward to more detailed information about how the vessels link perforators to the paraneural vasculature. When designing a flap, the surgeon has to estimate the value of taking a nerve in the flap, because he or she has to face the sensible lack of a donor site at the same time.

To sum up the above arguments, we think that the anatomical study of vessels has moved from static research to dynamic research. We believe that, in the near future, researchers will be able to simulate physiologic blood flow to help surgeons design the most suitable flaps for patients.

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REFERENCES


Reply: Perforasomes

Sir:

We are very appreciative of Dr. Ying-qi Zhang for sharing his comments and experience regarding our article on the perforasome theory, and are happy to respond to his questions.1,2 The first question asked is whether a priority of flow exists between direct and indirect linking vessels. We have not found this to be the case. Perfusion by means of both types of linking vessels occurs simultaneously, and we have not observed any perfusion bias between them. Communicating branches exist between direct and indirect linking vessels to maximize flow and provide a protective mechanism against vascular injury. In the case of adipofascial flaps, flow is ensured by means of multiple direct linking vessels and segments of indirect linking vessels that are left intact within the adipofascial layer. These communicating branches also confer additional protection by allowing flow through direct-indirect linking vessels, direct-indirect linking vessels, and indirect-indirect linking vessels (Fig. 1).

When an adipofascial flap is used, the majority of its perfusion will be based on direct linking vessels and their communicating branches. Some perfusion will still occur through indirect vessels but only through its intact extradermal segments. The second question asked concerns perfusion pressure and its relation to interperforator flow and maximal potential flap length. All of our flaps were injected using a standard computed tomographic angiography protocol and a precision Harvard pump, with a constant infusion volume of 0.5 ml/minute. Filling pressures were not recorded, but this infusion volume is certainly well below physiologic values. We also found that the majority of the perforator flaps studied were maximally filled after injecting only 5 ml of contrast solution. With regard to the third comment, many authors have described the vascular basis and clinical applications of various neurofasciocutaneous flaps, with the sural flap being a good example. We agree that the potential morbidity and sensory loss associated with these flaps must be weighed against their reconstructive benefits. Not all neurofasciocutaneous flaps depend on their neural component.
The Reconstructive Ladder in Light of Evidence-Based Medicine

Sir:

We read with great interest the recent report by Dr. Erba et al. suggesting a reconstructive matrix as a new paradigm in reconstructive plastic surgery.1 We would like to highlight one issue in this regard that has been somewhat neglected in the recent and not-so-recent paradigms, starting with the reconstructive ladder, the reconstructive elevator, the reconstructive triangle, and now the reconstructive matrix. The reconstructive ladder proposes a staged-approach to select the appropriate reconstructive procedure in a stepwise fashion, and was introduced in 1982 by Stephen Mathes and Foad Nahai in their book Clinical Applications for Muscle and Musculocutaneous Flaps. The selection of a given technique is based on its ability to satisfy the particular reconstructive requirements of the defect, aiming to restore form, contour, and function as well as possible.

The reconstructive elevator sought to surpass the reconstructive ladder by directly choosing the appropriate floor (e.g., the appropriate reconstructive procedure for a given reconstructive challenge). The recently proposed reconstructive matrix, albeit undoubtedly complex and sophisticated, addressing “technological sophistication,” “surgical complexity,” and “patient-surgical risk,” does not address evidence-based medicine as far as we understand the authors correctly. As stated in the Journal, “Evidence-based medicine is defined as the conscientious, explicit, and judicious use of current best evidence, combined with individual clinical expertise and patient preferences and values, in making decisions about the care of individual patients.”2 Given the fact that evidence-based medicine is highlighted and endorsed in an article series in Plastic and Reconstructive Surgery, we would like to ask, where is the evidence in terms of the reconstructive ladder or the reconstructive matrix?3

Notably, as far as the reconstructive ladder is concerned, literature research hardly results in a vast number of meta-analyses of randomized controlled trials as evidence level Ia or in several randomized controlled trials to determine the value of two distinct reconstructive procedures to address a reconstructive problem in a controlled way. Is a staged, stepwise approach from primary closure, skin grafting, pedicled musculocutaneous flaps to free musculocutaneous flaps better in a clinically significant way? Furthermore, as far as outcome studies are concerned, is free flap surgery more cost-effective than pedicled flap surgery in randomized controlled outcome studies? Given the recent innovations in preoperative perforator imaging techniques,4 where is the randomized controlled evidence that preoperative perforator imaging (e.g., by computed tomographic angiography) is superior in terms of clinical endpoints and cost-effectiveness?
There is reasonable evidence that prospective randomized controlled trials are mandatory to overcome the current rationale for choosing a reconstructive procedure for a given reconstructive problem. Especially with novel techniques and innovations evolving in reconstructive surgery, the results of controlled clinical trials are to be expected to assess their values for clinical practice. An analysis of four plastic reconstructive journals over 15 years revealed that, among 10,476 original articles, only 183 randomized controlled trials were published. Notably, only 20 percent of all trials mentioned funding, with reconstructive trials \((n = 33)\) funded either by industry or by public institutions, equally.

In conclusion, we strongly suggest and support the need for further prospective clinical trials at best in a randomized controlled design to help us decide in a more evidence-based way which reconstructive solution or combination to choose to achieve the best reconstructive result.

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REFERENCES


Reply: The Reconstructive Ladder in Light of Evidence-Based Medicine

Sir:

We would like to thank Drs. Knobloch and Vogt for their commentary on our recent article. They bring up the important subject of evidence-based medicine as an aid to decision-making in plastic surgery and cite the paucity of well-designed randomized controlled clinical trials within plastic surgery. We certainly support their view that plastic surgeons should be involved in more randomized controlled clinical trials and have previously made this same plea. As we examined the dramatic technological, surgical, and sociocultural changes influencing all subspecialties of reconstructive plastic surgery on a global level, we concluded there exists a need for a better framework to aid reconstructive surgeons in deciding among a multitude of options to address specific reconstructive needs. Often, the breathtaking pace of innovation exceeds our ability to conduct randomized controlled clinical trials before new technologies or procedures are used clinically. Compared with previously proposed reconstructive algorithms, the reconstructive matrix is a shared framework allowing surgeons the ability to navigate between newer combinations of technology and surgery while accounting for each individual patient’s surgical risk and reconstructive environment. In certain situations, evidence-based medicine is the surest way of comparing the efficacy of newer combinations against time-proven standards of care. This is exemplified by case study 1 in our article. In these situations, the reconstructive matrix allows surgeons to compare two options from the nearly infinite that exist in the three-dimensional matrix to facilitate decision-making based on evidence. Although the reconstructive matrix provides a useful framework for conceptualizing and comparing all viable alternatives, we believe that in many circumstances it is the responsibility of the surgeon to then use the most cost-effective, proven solution. Furthermore, it is the responsibility of the plastic surgery community to undertake the necessary studies that compare and assess, through well-designed studies, the seemingly endless combinations of therapeutic options available to address a reconstructive challenge. In fact, it is our intention and hope that the reconstructive matrix will provide the necessary framework to conceptualize, delineate, and understand with better precision the multitude of existing options, each in need of better studies to evaluate it against other potential strategies or to establish standards of care. Clearly, this is where evidence-based medicine and the reconstructive matrix can and should act in synergy.

More than any other specialty, outcomes in plastic surgery are highly dependent on patient-specific factors and subjective evaluations by patients and physicians. Therefore, although evidence-based medicine clearly has a role in helping physicians navigate between the various axes of the reconstructive matrix, evidence-based medicine cannot alone guide reconstructive surgeons in their decision-making. This point is illustrated by case studies 2 and 3 in our article. In such situations, the patient and his or her reconstructive environment often dictate care. Practically, randomized controlled clinical trials remain challenging to perform because of high costs, inadequate subject availability, and difficulty enrolling and/or blinding subjects to various treatment modalities. Furthermore, as a specialty, we have struggled to develop reliable and objective aesthetic outcome tools that could be applied
using an evidence-based medicine approach. Despite the challenges of obtaining quality evidence-based medicine studies, the reconstructive matrix is still useful as a framework for assessing all available options, as each surgeon amasses personal experience with various methodologies for solving a particular reconstructive challenge. By providing a framework for conceptualizing technology, surgical sophistication, and patient-specific factors, surgeons use the reconstructive matrix to find optimal solutions on a case-by-case basis within the realm of patient-specific factors and the surgeon’s own knowledge base and level of comfort.

There is a growing movement to design registry studies as a randomized controlled clinical trial alternative that allow for useful review of large amounts of data on diverse patient populations. Still, the reality is that we have few high-level evidence-based studies to guide our practice. For those decisions where evidence is clear from multiple well-designed clinical trials, we owe it to our patients to be well-educated on their results and to implement them in our practice whenever possible. We believe that the proposed reconstructive matrix is an appropriate tool to integrate evidence-based medicine in reconstructive surgery when available while still keeping in mind the need to organize, understand, and evaluate the myriad of still not fully proven reconstructive approaches now available to us.

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**The Role of Academic Plastic Surgery Institutions in Addressing the Global Burden of Surgical Disease**

**Sir:**

Much of the global burden of disease can be addressed through surgical intervention. Recently, it has been demonstrated that surgery can be performed in developing countries in a cost-effective manner. Plastic surgeons are poised to treat a broad range of surgical abnormality, including complex wound and skeletal defects, enabling plastic surgery to play a crucial role in alleviating the global surgical burden of disease. However, there exists considerable controversy surrounding the value provided by surgical missions.

Recently, Drs. Campbell, Sherman, and Magee indicated how participation by academic plastic surgery institutions can enrich the international mission. Drawing from our overseas experiences, we suggest how academic plastic surgery institutions can collaborate globally to provide guidance and direction, enabling current and future plastic surgeons to be global health leaders.

The first role is raising the awareness regarding global surgical burden of disease. Academic plastic surgery institutions can introduce students and residents to the concept of the global surgical burden of disease by means of grand round talks and didactics. Surgical missions permit a first-hand encounter with the global surgical burden of disease and assist in cultivating a lifelong commitment to alleviating it. The operative screenings, which inevitably result in a plethora of patients with common surgical issues, demonstrate the overwhelming need for surgeons worldwide. It must be stressed, however, that surgeons should perform operations routinely performed in their practice to avoid the stereotype of “practicing on the poor.” With nonprofit organizations such as Operation Smile and Cents of Relief already successfully collaborating with the Harvard and Yale plastic surgery programs to facilitate students and residents on cleft/burn missions, academic plastic surgery institutions may consider protecting time during plastic surgery training for an international mission. This tangible interaction with patients, many of whom will not receive treatment because of the paucity of resources, highlights the lack of access to surgical care while instilling the spirit for humanitarian missions.

The second role is promoting interdisciplinary research necessary in the global surgical burden of disease. The World Health Organization has published guidelines for cost-effectiveness studies—specifically, for surgical interventions—that policy makers are using.

**REFERENCE**


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in distribution of health care resources. This type of research, dependent on epidemiology and economic metrics, often requires assistance with public health and business school faculty. Academic plastic surgery institutions are in a setting where a surgeon, epidemiologist, and economist can collaborate to identify cost-effective strategies for decreasing the global surgical burden of disease. For academic plastic surgery institutions, this would enable its faculty and residents to participate in innovative research and offer recommendations to policy makers on the management of plastic surgery issues.

The third role is aiding the development of local infrastructure for long-term sustainability. Medical missions have the potential to create a permanent, sustainable solution for decreasing the global surgical burden of disease with support from academic plastic surgery institutions. Academic plastic surgery institutions can obtain basic surgical supplies (e.g., wound vacuums, dermatomes) at discounted rates from vendors. Using the cost-effective methodology, academic plastic surgery institutions and local staff input can optimize the allocation of resources and the scaling of infrastructure for the long run. Furthermore, a relationship between academic plastic surgery institutions and local hospitals has the potential to garner credibility from the government and potential donors and engenders a teaching environment.

With surgery becoming more recognized as an integral tool in addressing the global health disparities, academic plastic surgery institutions will play a critical role in the reduction of the global surgical burden of disease. Incorporating these roles may improve the ability of academic plastic surgery institutions to produce excellent plastic surgeons and public health leaders armed to tackle the global surgical burden of disease.

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REFERENCES


Introduction of the Implantable Doppler System Did Not Lead to an Increased Salvage Rate on Compromised Flaps: A Multivariate Analysis

Sir:

We have read with interest the past two issues of Plastic and Reconstructive Surgery, with both issues publishing studies that have investigated the usefulness of the implanted Doppler probe for monitoring free flaps.1,2 This probe is unique among monitoring techniques because of its direct and instantaneous monitoring of pedicle status. Having published some of our own results and research with the use of this device, a combined analysis with these articles yields interesting findings.3,4

Paydar et al. have clearly had a positive experience with their use of the device, with an enviable salvage rate of 95 percent, which includes a significant proportion of buried flaps. Such success rates have been matched only by Kind et al. in their series of patients who were also monitored with the implanted Doppler probe.3 However, Paydar et al. compared their cohort to literature values only, rather than any control group, such as other free flaps performed by their unit that were not monitored with the implanted Doppler probe. This makes statistical analysis of the effectiveness of their success impossible, and data from a similar series of cases that were performed by the same surgical unit and not monitored with implanted ultrasonic monitoring would be highly beneficial.

Smit et al. did make a direct comparison between flaps monitored by implanted Doppler devices and flaps that were not. Their research method is thus robust, showing an increase in their salvage rate from 60 percent to 69 percent, which they argued was not statistically significant (p = 0.44) and that therefore their study showed a lack of benefit for the use of the implanted Doppler system. With such a large magnitude in the improvement in salvage rate, it simply appears that the study performed was not sufficiently powered to establish the significance of the results. With greater numbers, significance of such results may yet be realized. This may be a more appropriate conclusion to that study.

There are now six studies of this device that have all shown salvage rates of 69 percent or higher (the others
Sequential Heart and Composite Tissue Allotransplantation in Rats

Sir:

I read with great interest the article entitled “A Model of Sequential Heart and Composite Tissue Allotransplant in Rats” published by Yang and colleagues in July of 2010 (Plast Reconstr Surg. 2010;126:80–86). In this article, the authors estimate “600,000 patients with solid organ allotransplants currently in need of reconstruction with a composite tissue allotransplant.” With this in mind, they were quite clever in their initiative and should be congratulated for investigating the role of composite tissue allotransplantation following solid organ transplantation.

However, I have a few additional concerns regarding this article. First, their study design was encouraging and novel, but its potential to provide translational value will require adjustment in the future. For instance, recipients received 10 days of cyclosporine following their heart transplantation, and then immunosuppression was discontinued following abdominal wall transplantation on postoperative day 10. Therefore, the only data gathered were from heart and abdominal wall allotransplants assessed/monitored on the way to inevitable acute graft rejection. Although the objective of Yang et al. “was to assess rejection severity and not to prevent rejection as would be done clinically,” the authors’ study will be much more insightful when it includes a second induction phase, and when daily maintenance therapy is continued long term for the evaluation of any ill effects related to the sequential transplant (i.e., graft rejection, opportunistic infection, immunosuppression-related complications).

In addition, it would have been extremely worthwhile had the authors discussed the second induction immunotherapy phase (which is required with sequential composite tissue allotransplantation) as both a major ethical and immunologic obstacle. This requires exposing fragile patients, who have already had successful outcomes from their life-saving solid organ transplant (i.e., liver, heart), to a variety of potential complications. Should we be risking primary solid organ graft failure with the transplantation of a life-enhancing composite tissue allotransplant (i.e., face, hand, abdominal wall)? In other words, what is the risk-to-benefit ratio of exposing these recipients to additional antigens from a third donor?

In addition, I feel compelled to acknowledge work performed during my year-long surgical research fellowship at Robert Wood Johnson Medical School at Camden/Cooper University Hospital under the direction of Charles Hewitt, Ph.D., in which I also investigated heterotopic heart transplantation for the exact same reasons. In fact, this project received the “Best Overall Resident/Fellow Research Award” at the annual American Society for Reconstructive Microsurgery meeting in 2006.1 It is therefore quite puzzling to me why the authors chose not to include and/or reference our two resulting publications,1,2 one of which described our femoral model used to study acute graft rejection (a study comparable to the one presented here) (Fig. 1), and the other of which illustrated our novel use of transfemoral echocardiography to monitor graft rejection (Fig. 2).

In conclusion, the study by Yang et al. was very interesting and well timed, especially because there are numerous institutions throughout the United States showed 80, 83, 95, and 100 percent).1–6 Considering this homogeneity of the published data, we think that this device has indeed proven useful, particularly in the setting of buried flaps, which were previously not monitored at all.

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interested in performing composite tissue allotransplantation. I very much look forward to reading about more stimulating and well-executed experiments from this group in the near future.

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Sir:

We read with great interest and pleasure the experimental approach of Dr. Levi et al. published in July of 2010, which describes possible co-effects of the cytokines insulin-like growth factor (IGF)-1 and platelet-derived growth factor (PDGF)-α on the osteogenic dif-
ferentiation of human adipose-derived stromal cells. We do believe that developments in the field of human adipose-derived stromal cells make them a crucial pillar for tissue engineering; therefore, we would like to congratulate the authors for their research work in this field.

One hypothesis of this study was the significantly ($p < 0.01$) higher pro-osteogenic effect of recombinant IGF-1 (rIGF-1) compared with recombinant PDGF-α (rPDGF-1) on the human adipose-derived stromal cells. However, using a sample size of $n = 3$ might be misleading because a random statistical error could have possibly been provoked. Figure 4 (which was interpreted throughout the article for a different purpose) showed actually no significant difference between the single application of rPDGF-α and rIGF-1. However, Suga et al.3 indirectly pointed out previously a possibly higher effect of rIGF-1 on human adipose-derived stromal cell proliferation. After 7 days of supplementation with 10 ng/liter rIGF-1, the cell count of human adipose-derived stromal cells increased slightly. The comparison of rIGF-1 with rPDGF-α in the osteogenic differentiation remains a promising approach of Dr. Levi’s work.

Cytokines play a key role in the stem cell microenvironment. After reflecting on this work, we recognize three challenging aspects for the field:

1. Observing the temporal and spatial aspects of cytokines on the microenvironment of stem cells should always be considered. It would give insight into the direct and indirect molecular interactions. Hauner et al.5 demonstrated this previously. In Figure 5 of Dr. Levi’s study, rIGF-1 application after rPDGF-α presupplementation showed a higher expression of RUNX2 and ALP detection on day 3. It is still possible, though, that the effects of rPDGF-α priming are just of an initial nature. A comparison on day 7 with the same method might have possibly answered this question.

2. The effects of the different concentrations should be investigated. Although on day 7 a clear benefit of a higher IGF-1 concentration (20 ng/liter versus 10 ng/liter) did not show any significant effect on the osteogenic differentiation, on day 3 the higher concentration of rIGF-1 showed an even paradoxical effect of reducing osteogenic differentiation (Fig. 1).

3. Distinguishing between differentiation and proliferation is very troublesome. The quantification of the genetic expression markers (ALP and RUNX2) or alizarin red does not clearly rule out whether these results are attributable to a higher proliferation of a few osteogenically differentiated human adipose-derived stromal cells, or whether it is really attributable to a higher osteogenic differentiation. Therefore, we think that the quantification with these methods should be carried out in relation to the absolute cell count.

Finally, we would like to acknowledge the effort behind this study. This work was apparently based on a previous work from Lee et al.,4 who examined the role of several cytokines on the osteogenic differentiation of human adipose-derived stromal cells with a microarray analysis. Recombinant transforming growth factor-β3 showed the highest expression. We are therefore very keen to an even more exciting study in the future in which the effects of recombinant transforming growth factor-β3 are analyzed.

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REFERENCES

Reply: Regulation of Human Adipose-Derived Stromal Cell Osteogenic Differentiation by Insulin-Like Growth Factor-1 and Platelet-Derived Growth Factor-α

Sir:

We thank the authors for their complimentary discussion of the article entitled “Regulation of Human Adipose-Derived Stromal Cell Osteogenic Differentiation by Insulin-Like Growth Factor-1 and Platelet-Derived Growth Factor-α.” We are pleased that our work has stimulated conversation on the usefulness of adipose-derived stromal cells, and agree with the readers that adipose-derived stromal cells have significant potential for future endeavors in skeletal tissue regeneration. We would like to respond to a handful of comments Drs. Mikhail and Steinstraesser have made.

First, we would like to discuss the statistics as mentioned, as we believe that statistical power is of paramount importance for any scientific study. We could have been more clear that $n = 3$ meant that human adipose-derived stromal cells were harvested from three female patients.
This means that we conducted all experiments three times and, as described, each experiment was performed in triplicate, which means there was $n = 9$ for each treatment for every stain, quantification, and qRT experiment. We believe this exceeds what is needed to demonstrate statistical significance. Since this study, we have also repeated this experiment on fat from three other female patients and from other species such as mouse and dog for a different study. Across all species and the additional patients, these results were repeated. Thus, we are very confident of these findings.

We agree that consideration of the timing of cytokine stimulation is important in cell differentiation. This concept in fact prompted the investigation of a short pretreatment of platelet-derived growth factor (PDGF) followed by insulin-like growth factor (IGF)-1 supplementation (Fig. 5). This duration of exposure was chosen empirically, and without a doubt refining is warranted of both the timing and composition of a “cocktail” of pro-osteogenic cytokines for adipose-derived stromal cell differentiation.

Second, we agree that ideally a range of concentrations should be studied. For the present (and initial) study of the effects of IGF/PDGF on the osteogenic differentiation of adipose-derived stromal cells, we narrowed our focus of study to commonly used concentrations of 10 and 20 ng/ml. We do agree that before these in vitro results are applied to in vivo scenarios, comprehensive dose curves should be performed to verify the precise, optimum concentration for the pro-osteogenic effects of the cytokines.

Third, we agree that distinguishing between differentiation and proliferation in any context is important. Does a particular cytokine enhance bone formation by expanding osteoprogenitor cell number, by directly promoting differentiation, or by means of both mechanisms in combination? This is why for the present study we normalized alkaline phosphatase enzyme activity to total protein content, and specific gene expression to housekeeping gene expression. However, there is no doubt that recombinant IGF or PDGF affects cellular proliferation, and future investigation beyond this initial study is warranted to determine the precise mechanism of action of each cytokine’s pro-osteogenic effects.

Finally, we thank the reader for their interest in the results of our previous microarray data and the potential influence of transforming growth factor-β signaling on adipose-derived stromal cell osteodifferentiation. We hope that they will read with equal interest our article entitled “Divergent Modulation of Adipose-Derived Stromal Cell Differentiation by TGF-bet1 Based on Species of Derivation” in an upcoming issue of Plastic and Reconstructive Surgery.

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Correction: Indications for Performing Carpal Tunnel Surgery: Clinical Quality Measures

In the article by Maggard et al. entitled “Indications for Performing Carpal Tunnel Surgery: Clinical Quality Measures,” published in the July 2010 issue of the Journal (Plast Reconstr Surg. 2010;126:169–179), an item in Table 1, Clinical Characteristics That Were Varied to Create Scenarios,” is incorrect. Under “2. Clinical probability that symptoms represent carpal tunnel syndrome,” the authors define a high probability presentation as one that includes a "symptom pattern classic for carpal tunnel syndrome." The correct definition is a presentation that includes a "symptom pattern classic or probable for carpal tunnel syndrome (defined below)" (corrections in italics). DOI: 10.1097/PRS.0b013e31820c039b

REFERENCES

Correction: Early Detection of Complete Vascular Occlusion in a Pedicle Flap Model Using Quantitation Spectral Imaging

In the article by Pharaon et al. entitled “Early Detection of Complete Vascular Occlusion in a Pedicle Flap Model Using Quantitation Spectral Imaging,” published in the December 2010 issue of the Journal (Plast Reconstr Surg. 2010;126:1924–1935), the title of the article and the disclosure statement are incorrect. The title should read “Early Detection of Complete Vascular Occlusion in a Pedicle Flap Model Using Quantitative Spectral Imaging” (correction in bold). The disclosure statement should read “Dr. Cuccia and Dr. Durkin have a financial interest in Modulated Imaging, Inc. The other authors have no financial interests or commercial associations that might pose or create a conflict of interest with the information presented in this article” (corrections in italics). DOI: 10.1097/PRS.0b013e31820c03b9

REFERENCES
Correction: Basic Science Review on Adipose Tissue for Clinicians

In the article by Brown et al. entitled “Basic Science Review on Adipose Tissue for Clinicians,” published in the December 2010 issue of the Journal (Plast Reconstr Surg. 2010;126:1937–1946), the name of the third author is spelled incorrectly. The correct spelling of the third author’s name is Charlotte Lequeux (correction in italics).
DOI: 10.1097/PRS.0b013e31820c03e2

REFERENCE

Correction: Blepharoplasty Customized Marking: A New Technique for Better Results

In the Viewpoint by Razavi and Rajabi entitled “Blepharoplasty Customized Marking: A New Technique for Better Results,” published in the December 2010 issue of the Journal (Plast Reconstr Surg. 2010;126:307e–308e), the affiliation for Dr. Rajabi should read “Eye Research Center, Farabi Eye Hospital, Department of Ophthalmology, School of Medicine, Tehran University of Medical Sciences, Tehran, Iran” (correction in italics).
DOI: 10.1097/PRS.0b013e31820c0401

REFERENCE