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Letters

Survival of Diced Cartilage Grafts: An Experimental Study

Sir:

We read the article entitled “Survival of Diced Cartilage Grafts: An Experimental Study” by Brenner et al. (Plast. Reconstr. Surg. 117: 105, 2006) with great interest. Diced cartilage is known to be excellent for filling, but it is also known to be difficult to introduce and retain in tissue. The authors stated that wrapping diced cartilage with Surgicel (Turkish Delight grafts) produces an inflammatory response and subsequent absorption of the cartilage. We would like to present similar results of our experimental study in rabbits.1

The main aim of our study was to investigate the effects of different degrees of crushing on autogenous cartilage graft viability and outcome in rhinoplasty. We also compared the outcomes for crushed cartilage grafts with outcomes from using Surgicel-wrapped diced cartilage grafts. The cartilage grafts were harvested from the ears of 29 rabbits and inserted into the paraspinal subcutaneous plane. Animals were euthanized at 2, 5, and 10 months, and graft specimens were evaluated for graft viability, new cartilage cell formation, fibrous-collagenous tissue development, and bone metaplasia. While the intact grafts showed significant new cartilage formation at the periphery, the diced/Surgicel-wrapped grafts exhibited almost no peripheral growth activity. While the average proportion of viable graft material in the intact cartilage group was 90 percent, the mean viability in the diced/Surgicel-wrapped group was only 10 percent. Our results indicate that diced/Surgicel-wrapped grafts exhibit massive destruction of the chondroid matrix, extensive loss of viability, and almost no chondrocyte proliferation at the periphery. Our results show that Surgicel not only inhibits cartilage proliferation but also decreases cartilage viability. Owing to these negative effects, we believe that in the clinical setting, wrapping diced cartilage with Surgicel reduces long-term graft predictability.

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Reply

Sir:

We kindly thank Drs. Cakmak and Buyuklu for their interest in our article, “Survival of Diced Cartilage Grafts: An Experimental Study” (Plast. Reconstr. Surg. 117: 105, 2006). We were intrigued to learn of the findings in their study, “Viability of Crushed and Diced Cartilage Grafts: A Study in Rabbits” (Arch. Facial Plast. Surg. 7: 21, 2005). The correlation between degree of

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cartilage crushing and loss of viability with time was important and, indeed, unique. The size (29 animals) and duration (10 months) of their study likewise was valuable. While we acknowledge the difficulty inherent in physically measuring grafts, it is a bit unclear how exactly they determined the extent (percentage) of cartilage viability. Did they calculate the exact area of each graft upon insertion and extraction? Further, the statistical significance of their percentage estimations is not included. We were not surprised to see that they too noted severe loss of chondrocyte viability when wrapping the grafts with Surgicel. This was no doubt the result of Surgicel-induced inflammatory reactions. In addition, we believe that chondrocytes may “turn over” or “regenerate,” but they do not necessarily “proliferate.” Cartilage grafts clearly have been noted to warp and absorb, but they do not really proliferate and increase in size. Instead, we prefer to assess “regeneration potential” with GFAP staining.

While we believe that their findings are similar to ours, there are a few differences that are worth pointing out. Their investigation utilized a rabbit model with rabbit cartilage; our investigation used an athymic rat model with human cartilage. The behavior of rabbit and human cartilage may differ, making extrapolation of results difficult. Their study noted a significant difference among intact cartilage grafts, variably crushed grafts, and diced grafts wrapped with Surgicel. Our study noted a significant viability difference between diced cartilage grafts wrapped with human deep temporal fascia and those wrapped with Surgicel. While both studies are partially corroborative, they are also complementary, and each individually lends credence to the technique of cartilage grafting in rhinoplasty.

With regard to diced cartilage grafts, we strongly believe that fascia serves as a superior envelope. Deep temporal fascia provides softer aesthetic contouring and, more significantly, it helps to preserve chondrocyte viability. Clearly, caution must be exercised for surgeons who prefer using Surgicel, as unpredictability in cartilage viability and, ultimately, graft survival is inevitable.

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A Severe Case of Madelung Syndrome

Sir:

We were very interested in reading the brief communication by Dr. B. Civelek et al. entitled “An Unusual Entity for a Plastic Surgeon: Madelung Syndrome” (Plast. Reconstr. Surg. 117: 693, 2006).

During the past 3 years, we have treated four cases of Madelung syndrome. Two patients had upper extremity localizations that were treated by liposuction only; one patient had upper trunk and neck involvement that was treated by combined liposuction and lipectomy. One patient had major involvement of the neck that was managed with an open approach, and which we present herein. In all three cases treated by liposuction, we encountered disease recurrence to various degrees. Our experience strongly supports Civelek et al.’s conclusion that open excision is the best approach to avoid recurrence in these cases.

A 48-year-old man was referred to our division by the internal medicine department after being diagnosed with progressive symmetrical lipomatosis (Fig. 1, left).
His clinical condition was complicated by liver steatosis, which progressed to hepatic hypocoagulability. The patient’s general condition and his hesitance to undergo surgery delayed any operative management. Six months later, the patient presented in respiratory distress with obstructive symptoms that seriously impaired his ability to sleep, eat, and speak. The patient was taken to the operating room to undergo a bilateral modified radical neck dissection (levels 1 through 6) with preservation of the sternocleidomastoid muscle, internal jugular vein, and spinal nerve. A 2.5-kg mass of dystrophic adipose tissue was resected from the anterior triangles of the neck bilaterally. The patient had an uneventful postoperative course, and after 18 months of follow-up, he is still free of recurrence. All of his respiratory and oral functions were restored (Fig. 1, right).

Madelung syndrome is a rare disease (incidence, one in 25,000) of unknown etiology characterized by multiple symmetric nonencapsulated adipose masses that can affect several parts of the body.2–5 Localization to the head and neck is common. In the reported case, the adipose masses not only were subplatysmal but had spread through all the deep fascial planes of the neck, between muscles and neurovascular structures, although they did not invade them.

**REFERENCES**


**Reply**

**Sir:**

I would like to take the opportunity to congratulate Dr. Baccarani et al. for accomplishing a radical excision in a case of Madelung syndrome. It is worth noting that a cosmetically disturbing look in a patient might cause a functional disability as well. It is also noteworthy that liposuction attempts may turn out to be futile, although they are less traumatic. Therefore, as plastic surgeons, we should not hesitate to resort to conventional surgical methods when it is deemed necessary.

I am surprised, however, to see that Baccarani et al. performed a bilateral neck dissection on their patient in an emergency setting. It should be considered more of a debulking procedure than a radical curative resection. A modified radical neck dissection bilaterally may be a destructive and risky procedure. They deserve appreciation, though, for such a courageous and quick approach in an emergency setting.

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**Surgical Strategy in the Treatment of Globe Protrusion Depending on Its Mechanism**

**Sir:**

I read with great interest the article entitled “Surgical Strategy in the Treatment of Globe Protrusion Depending on Its Mechanism (Graves’ Disease, Nonsyndromic Exorbitism, or High Myopia)” by Krastinova et al. (Plast. Reconstr. Surg. 117: 553, 2006). I have some different opinions and solutions concerning surgical treatment of Graves’ ophthalmopathy.1–3
1. During the expansion on the orbital floor, I remove the whole retrobulbar part of the orbital floor, sawing the infraorbital nerve. If the orbital floor is not removed medially from the infraorbital nerve, then it is impossible to obtain good expansion and decompression. If the whole retrobulbar part of the orbital floor is removed, the expansion is achieved at the most important part concerning the disease, that is, in the narrow apex of the orbit. The anterior, bulbar part of the orbit prevents the eyeball from “falling” into the maxillary sinus.

2. I disagree with the opinion that “fibrous change of the orbital contents in long-standing Graves’ ophthalmopathy more often requires removal of a third [lateral] orbital wall than in recent, less fibrous Graves’ ophthalmopathy” (my brackets). In cases of moderate and severe Graves’ ophthalmopathy, it is necessary to perform three-wall expansion, including the lateral wall and excluding the lateral margin of the orbit. Lateral expansion also prevents superfluous medial globe displacement, which can cause worsening of the diplopia. The openings made in the three orbital walls should be continuous, without bone bridges between the walls, as well as in the region of the infraorbital nerve. In this way, an important symmetrical decompression is achieved that enables the recovery of the optic nerve and balanced decongestion (i.e., recovery of the extraocular muscles and exophthalmos). Unbalanced recovery of the extraocular muscles can worsen diplopia.

3. I also disagree with the opinion that “in emergency cases, the expansion must be minimally invasive to save vision without risking an overcorrection of the proptosis in case of a secondary regression of the ophthalmopathy.” This is a severe case of Graves’ ophthalmopathy, so minimally invasive expansion will not give a good therapeutic result. If the expansion is performed correctly, overcorrection of the proptosis is virtually impossible.

4. I do not think that correction of eyelid deformities and retraction should be performed in a second operation. In my experience, it is possible to perform, via the eyelid approach, three-wall expansion, removal of orbital and retrobulbar fat, and correction of eyelid deformities and retraction in a single operation, with very good, even excellent, results (Fig. 1).

5. I disagree with the decision “not to remove too much posterior orbital fat around the extraocular muscles because of the risk of producing or aggravating strabismus.” Gentle, precise removal of fat does not risk producing or aggravating strabismus. Removal of fat in the apical region, with rough traction, can risk injury to the optic nerve and central artery of the retina with loss of vision.

6. I think orbital enlargement, using calvarial bone grafts, is not indicated in patients with Graves’ ophthalmopathy. Enlargement by grafting on the lateral and superior orbital rim and the nasal bone makes the diameter of the orbits longer. The longer orbital diameter creates stronger pressure in the apical region of the orbits. Exophthalmos in Graves’ ophthalmopathy is a kind of spontaneous decompression.

My approach to orbital expansion on three walls is simpler and less traumatic and allows faster post-

![Fig. 1. (Left) Severe Graves’ ophthalmopathy with severe eyelid retraction. (Right) Three-month postoperative view.](image)
operative recovery. As for upper blepharoplasty, excision of the excessive skin and subcutaneous tissue and eyelid fat is performed. To obtain the correction of upper eyelid retraction, I excise the central part of the levator aponeurosis and Müller’s muscle. Using this procedure, it is possible to achieve excellent correction of upper eyelid retraction. In one case only did I have to use the graft interposition technique. Using a lower blepharoplasty incision, orbital fat, the retrobulbar part of the orbital floor, and the lateral orbital wall, except for a lateral orbital margin, are removed. After this, through an incision made over the medial margin of the orbit, the ethmoidal part of the medial orbital wall and the retrobulbar fat are removed. When it is necessary, lateral canthopexy is performed. I always put the vacuum drain into the retrobulbar space. At the beginning of the operation, temporary blepharorrhaphy should be performed using two single sutures. These sutures are removed 5 to 7 days postoperatively. After such an operation, the definitive result (recovery of all orbital structures) is achieved in 2 to 3 months. If strabismus and double vision are not corrected during this time, a strabismus operation should be performed. This operation has been performed on five out of more than 100 patients.

In cases of globe protrusion (“big eyes”) due to shallow and/or short orbits, using a lower blepharoplasty incision, I remove orbital fat and, if necessary, the retrobulbar part of the orbital floor3 (Fig. 2).

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REFERENCES

Reply
Sir:

We thank Dr. Rončević for his interest in our article and for his interesting comments. Dr. Rončević has good experience of orbital expansion. He uses a lower blepharoplasty approach and an incision made over the medial margin of the orbit to remove the whole retrobulbar part of the orbital floor (including the infraorbital nerve), the lateral and medial orbital walls, and orbital fat. He performs upper blepharoplasties and corrects upper lid retraction by excising the central part of the levator aponeurosis and Müller’s muscle during the same stage. However, we would like to comment on a few points.

Dr. Rončević seems to have misunderstood our surgical strategy. As described in the Methods section of our article, our two-wall expansion procedure always includes removal of the medial and lateral orbital walls. We agree that, otherwise, the medial displacement of the globe could worsen a diplopia. Dr. Rončević states that it is not

Fig. 2. (Left) Patient with malignant Graves’ ophthalmopathy. (Right) Three-week postoperative view.
possible to perform a good three-wall expansion procedure without removing the infraorbital nerve. Our long-term results show that, on the contrary, a nerve-sparing approach is possible and provides good results.

In emergency cases, as described in our Methods section, we use a direct approach. We consider this approach to be minimally invasive compared with a coronal approach. The orbital expansion itself is not minimally invasive, because the entire orbital floor may then have to be removed. Even in this case, we try to spare the infraorbital nerve.

It is our opinion that in terms of eyelid surgery in Grave’s ophthalmopathy, the best results can be achieved through a two-stage approach. This opinion is the result of our experience (we used to perform eyelid surgery and orbital expansion in the same stage) and of close long-term follow-up of our patients. Dr. Rončević should not show early results to illustrate his theory.

Whatever the consequences, we agree with Dr. Rončević that removal of posterior orbital fat is dangerous. However, we do not understand his comment about the risk of increasing the pressure in the apical region of the orbit by using bone grafts on the orbital rims and the nasal bone.

Finally, we agree with Dr. Rončević that his surgical attitude is simpler than ours, but we do not think that a deliberate and systematic sacrifice of the infraorbital nerve is “less traumatic.” We would have appreciated seeing more mature cases to illustrate his comments. Dr. Krastinova, following in the footsteps of Dr. Paul Tessier, used to perform the same technique as Dr. Rončević, although the coronal approach was used most of the time.

The strategy outlined in our article is the consequence of our experience (we used to perform eyelid surgery and orbital expansion in the same stage) and of close long-term follow-up of our patients. Dr. Rončević should not show early results to illustrate his theory.

Mandibular Reconstruction Using a Combination Graft of rhBMP-2 with Bone Marrow Cells Expanded In Vitro

Sir:

We read with interest Seto et al.’s article entitled “Mandibular Reconstruction Using a Combination Graft of rhBMP-2 with Bone Marrow Cells Expanded In Vitro.”1 It is an important article that illustrates the use of bone marrow cells in bone tissue engineering. However, we were not convinced by the authors’ claims that the combination graft of recombinant human bone morphogenetic protein-2 (rh-BMP-2) and culture-expanded bone marrow cells is a reliable method for the reconstruction of segmental bone defects of the mandible. First, the number of animals used in this study was limited, with three animals in the experimental group and just one animal in each control group, to end up with a really conclusive result. Second, there should be at least one more control group to demonstrate that the combination graft of rhBMP-2 and culture-expanded bone marrow cells is a reliable method for reconstructing segmental bone defects. As the authors emphasize in their article, bone tissue engineering depends mainly on the use of three elements—bone-forming cells, tridimensional osteoconductive scaffolds, and osteoinductive growth/differentiation factors—to regenerate new bone tissue.2 To demonstrate the effect of something on bone tissue, two of these main standpoints should be kept constant. In other words, in each step of an experimental study, it is better to change only one variable to demonstrate the effect of it. In the study design of this particular experiment, culture-expanded bone marrow cells were used as bone-forming cells, collagen beads were used as both an osteoinductive scaffold and a carrier system for rhBMP-2 release, and rh-BMP-2 was used as an osteoinductive factor. Control groups were designed as collagen beads only and collagen beads with culture-expanded bone marrow cells. Urist demonstrated that demineralized bone segments and extracts of demineralized bone induce bone formation when implanted subcutaneously or intramuscularly in animals.3 Subsequent purification studies of these bone inductive proteins resulted in the identification of many members of the bone morphogenetic proteins.4–6 Therefore, application of bone morphogenetic proteins in a slow-release system has been demonstrated to be enough to induce new bone formation in vivo.

A control group with rhBMP-2–loaded collagen beads without any culture-expanded bone marrow cells should be included to demonstrate that culture-expanded bone marrow cells really have a significant additional bone-healing effect in a system with a good scaffold and slow-release rhBMP-2 in such an in vivo study.

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REFERENCES

Breast Cancer Local Recurrence after
Mastectomy and TRAM Flap Reconstruction:
Incidence and Treatment Options

Sir:

It is with great interest that we read the article by Howard et al.1 published in the April 15, 2006, issue of Plastic and Reconstructive Surgery. This is a concise and clear account of the incidence and management of local recurrence following transverse rectus abdominis musculocutaneous (TRAM) flap reconstructions. As the authors point out, the study supports the crucial importance of physical examination in the diagnosis of local recurrence. Most previous studies have reported the recurrence presenting as a palpable mass or thickening.2-4 In our experience, the majority of patients presented with local changes in the skin and small palpable nodules adjacent to the mastectomy site and TRAM flap. Also, most recurrences take place at the suture line and other clinical findings may be associated, such as pain, areas of irregularity, or local tenderness.

Despite the importance of the TRAM flap physical examination in the surveillance following initial breast cancer treatment and reconstruction, specific screening modalities for recurrence have not yet been determined.2 Moreover, although there is no consensus, in Brazil, routine mammography of the TRAM flap is advocated even in patients with a normal physical examination. Given the previous evidence demonstrated in large TRAM flap series and the local recurrence patterns observed in the Memorial Sloan-Kettering Cancer Center experience,4 do the authors believe that routine mammographic surveillance of the TRAM flaps is justified in terms of a cost-benefit analysis? As mentioned in their Discussion, and we totally agree, the utility of the mammography as a primary screening tool is not clear. In our series, we advocate that any palpable mass in a reconstructed breast indicates biopsy with an appropriate technique (fine needle, core biopsy, or an open biopsy) to rule out the possibility of recurrent breast cancer.

With this understanding, the authors make another important observation in this study. Few previous studies have addressed completely the management of recurrent breast cancer following TRAM reconstruction. Some studies advocate surgical resection with radiation therapy with minimal effect on the quality of the reconstruction. Others advocate aggressive resection, including the TRAM flap. These different approaches indicate that there is no agreement and also reflect continuing data collection regarding the optimal treatment of recurrent breast cancer in TRAM flap patients.5

Data from the Memorial Sloan-Kettering Cancer Center add support for the concept that the great part of TRAM flaps can be preserved in the situation of local recurrence. This is consistent with our experience demonstrating that, in uncommon cases, the TRAM flap is resected. However, to preserve the TRAM flap, careful and coordinated planning with the oncologic surgeon is advocated. In cases of reexploration, it is reasonable to perform reexploration in conjunction with the plastic surgery team in order to identify the original tumor bed and flap tissue. In addition, surgical margins can be assessed during surgery by histological examination of frozen sections to avoid unnecessary flap tissue resection.

Howard et al. have added to the breast reconstruction literature with this article in many ways. First, the results from the study add to the growing body of clinical evidence that immediate autogenous breast reconstruction has not been shown to adversely affect local recurrence. Second, the authors make a very good case for being less aggressive with partial resections and TRAM flap preservation. Lastly, they remind us of the relevance of the physical examination, a fact not necessarily recognized by many oncologic surgeons and one that will potentially aid the plastic surgeon in the final aesthetic result following treatment of local recurrence.

In conclusion, we believe that breast cancer recurrence after TRAM flap reconstruction is a relatively unusual situation and has not been shown to affect the aesthetic outcome negatively. This is an important text that should be appropriate to many oncological and plastic surgeons.

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Sir:

Reply

Sir:

Drs. Munhoz and Aldrighi have asked a thoughtful and important question regarding monitoring transverse rectus abdominis musculocutaneous (TRAM) flaps for local recurrence. As noted in our manuscript, the majority of local recurrences after mastectomy and TRAM flap reconstruction occur as palpable abnormalities in the native mastectomy skin adjacent to the flap. Rarely will a recurrence present itself within or adjacent to the substance of the flap. The question of whether routine mammography (or TRAMography) for screening purposes is cost effective is an interesting one. During the early 1990s, routine TRAMography was performed at our institution but was abandoned for two main reasons: (1) inadequate penetration of the tissues by the x-ray beam, yielding inadequate images for routine screening; and (2) the fact that, oncologically, the vast majority of local recurrences present in the mastectomy skin flaps or scar thus are accessible by physical examination. Disease presenting deep to the flap on the chest wall is considered stage IV disease and is not normally screened for. Munhoz and Aldrighi raise another important point, specifically, managing the local recurrence while attempting to salvage the TRAM flap. Local recurrence after mastectomy and TRAM reconstruction is a devastating outcome for patients. Routinely removing the flap adds to the emotional and physical insult. A multidisciplinary approach, including oncologic and plastic surgeons and medical and radiation oncologists, to decide on the optimum treatment for local recurrence after mastectomy and TRAM reconstruction, is warranted.

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Determination of the Vascular Configuration in Autogenous Breast Reconstruction Using Abdominal Tissue: Simple but Not Necessarily Easy

Sir:

We applaud the attempt by Dr. Ulusal et al. to quantify perfusion of zones I through IV in abdominal wall flaps in preparation for breast reconstruction in their article, “Breast Reconstruction Using the Entire Transverse Abdominal Adipocutaneous Flap Based on Unilateral Superficial or Deep Inferior Epigastric Vessels” (Plast. Reconstr. Surg. 117: 1395, 2006). The ability to reliably determine the optimal circulatory system (deep inferior versus superficial inferior epigastric) and ensure viability of as many zones as possible based on an easily defined parameter, such as pedicle diameter, would be of great benefit to our practice. We currently dissect the superficial and deep systems bilaterally and then sequentially isolate individual systems to determine which can sustain the most tissue. This is done to determine the side and the system, deep or superficial. If the deep system is selected, it can also be used to determine how many perforators are required. This obviously adds time to the procedure.

We agree with Dr. Nahabedian’s discussion (Plast. Reconstr. Surg. 117: 1404, 2006) that there is no “magical formula” that can determine the optimal ratio between vessel caliber and flap dimension. We appreciated his discussion of the relationship between flap volume and perfusion and agree that even though a thicker flap can have a larger pedicle or perforators, it can still have poor arterial perfusion at its distal edges. This situation requires flap excision to better bleeding or, if volume is required, additional arterial inflow. What Dr. Nahabedian does not discuss in detail, and what we would like to add to his discussion, is an evaluation of venous integrity.

We believe that fat necrosis results not only from inadequate arterial inflow but also from inadequate venous outflow. Because of this, we examine the flap for signs that arterial inflow is greater than venous outflow. These signs include overly brisk bleeding at the tissue edges, darker coloration of the cutaneous surface, and distended outflow veins. Care must be taken to ensure that there is no significant bleeding at the tissue edges when attempting to evaluate venous integrity. If any of

REFERENCES

these signs are present, additional outflow veins can be released to see whether evidence of congestion resolves. Based on the results, additional veins can be repaired. This technique can lead to multiple combinations of arterial and venous circulatory systems, depending on the specific tissue requirement. For instance, a recent case involved a patient with a significant smoking history. Although the flap was isolated on two perforators greater than 1 mm in diameter, there were areas with dark bleeding at the skin edges. Because the bleeding was sluggish in those areas, we diagnosed poor arterial perfusion rather than venous congestion. The superficial inferior epigastric artery was released, which resulted in bright red bleeding. Neither individual system supported the required flap, so a combination of the ipsilateral deep and superficial arterial systems was utilized. Repair of deep and superficial venous outflow can similarly be used in cases of venous insufficiency. Regardless of vessel diameter, the integrity of the system can be evaluated and supplemented, depending on the vascular anatomy. We would embrace any method that can reliably determine the optimal vascular configuration. The best method would make that determination preoperatively.

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Reply

Sir:

I thank Drs. Brooks and Buntic for highlighting this very important point. They are absolutely correct that fat necrosis can occur from insufficient arterial inflow or insufficient venous outflow. In our initial article on breast reconstruction with the free TRAM or DIEP flap, it was mentioned that fat necrosis can occur from arterial or venous insufficiency.1 However, in all the analyses that have been performed, fat necrosis and venous congestion have been evaluated as two distinct variables that appear to be independent of one another. This is in part because their occurrence can be measured and analyzed independently. It is also because the initial investigations that evaluated venous congestion and fat necrosis in the DIEP flap were performed as separate studies and evaluated each as independent variables.2,3 The reality, however, is that fat necrosis can be considered a variable measurement on a scale with arterial insufficiency at one end and venous insufficiency at the other. Any circulatory imbalance can result in fat necrosis.

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REFERENCES


Reply

Sir:

We read the letter from Drs. Brooks and Buntic with interest, and we agree that the abdominal adipocutaneous flap based on unilateral superficial or deep inferior epigastric vessels is determined by not only the pedicle diameter but also the appearance of the flap. We always check the flap’s appearance (color and bleeding) as well as the diameter of vessels (perforators and pedicle) before flap division. Although there is no “magical formula” addressed by Dr. Nahabedian to determine the optimal ratio between vessel caliber and flap dimension, we recommended using the “superficial system larger than 1 mm in diameter” as an objective sign to determine the sufficiency for the entire abdominal flap. Individual arterial insufficiency, of either the superficial or deep system, may occur in particular patients with a history of heavy smoking, as described by Brooks and Buntic. None of the superficial inferior epigastric artery flaps required additional arterial augmentation for the contralateral part of the flap in our series. On the other hand, it is difficult to do the vessels anastomoses if the pedicle diameter is smaller than 1 mm.

Fat necrosis is an important issue for autologous breast reconstruction. We agree that fat necrosis may result from either arterial or venous insufficiency. The etiology of fat necrosis needs more investigation.

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The Incidence of Tuberous Breast Deformity in Asymmetric and Symmetric Mammaplasty Patients

Sir:

We read the article by DeLuca-Pytell et al.1 with great interest. We have dealt with the subject of tuberous breast deformity in depth,2 and we would like to offer our own experience on the subject.

The tuberous breast deformity first described by Rees and Aston in 19763 has been recognized as a rare deformity characterized by deficiency in the vertical and/or horizontal dimensions of the breast due to a constricting fibrous ring at the level of the periphery of the nipple-areola complex.2,4,5 This constricting ring is usually denser at the lower part of the breast and does not allow the developing breast parenchyma to expand during puberty. In a recent publication, we looked at the cause of tuberous breast deformity and suggested a thickening of the superficial fascia combined with the absence of the superficial layer of this superficial fascia under the nipple-areola complex as the underlying anatomical/histopathological cause for the deformity.2

As far as the incidence of the deformity is concerned, in the last 8 years, we have come across 34 cases of true tuberous breast deformity in a pool of approximately 500 patients who presented for breast surgery, although the true incidence of the deformity in the population remains unknown. We were surprised, therefore, to read the article by DeLuca-Pytell et al.,1 in which it was concluded that tuberous breast deformity is a very common problem, identified in as many as 73 percent of the 375 patients studied. This conclusion did not correspond with our own experience over the years, so we looked at the article with a more critical eye. We believe that the authors, although driven by a genuine scientific curiosity, committed a grave error in the design of their study. They used the criteria of tuberous breast deformity too liberally, and therefore included virtually every single breast deformity they came across, as depicted in Figure 3 of their article. We, too, believe that tuberous breast deformity is a deformity that often goes unrecognized by the untrained eye, but on the other hand, the untrained eye can easily classify all deformities as tuberous breasts.

We would like to commend the authors for their effort, but we would also like to draw their attention to our theory on the cause of the deformity,2 which could offer some assistance in understanding its nature.

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REFERENCES


Maternal Cigarette Smoking during Pregnancy and Congenital Digital Anomalies

Sir:

Recently, Man and Chang (Plast. Reconstr. Surg. 117: 301, 2006) utilized National Center for Health Statistics birth certificate data to examine the association between maternal cigarette smoking and “congenital digital anomaly.” While the freely available public use datasets of the National Center for Health Statistics have greatly increased researcher access for analytical purposes, this analysis is an example of the unintended consequences of the widespread availability of these data. Man and Chang devised a case-control study design using a cross-sectional dataset (1999 to 2001), identifying cases as any live birth whose birth certificate checkbox for “polydactyly, syndactyly, adactyly” was marked. Two controls for each case were selected from among those births without any congenital anomalies, matching for maternal and paternal race, sex, county of birth, and month of birth. This study design may appear reasonable on the surface, but when one looks deeper, serious flaws emerge. Congenital disabilities epidemiologists strive to minimize the potential for etiological heterogeneity by avoiding grouping diverse conditions into a single category. While all three conditions, polydactyly, syndactyly, and adactyly, relate to congenital abnormalities of the digits of the hand or foot, these conditions are very different and should not be analyzed as a single group of anomalies. Polydactyly can result from single gene disorders and frequently occurs in isolation from other congenital disabilities. Syndactyly and adactyly frequently occur in conjunction with multiple defects in other organ/body systems. While Man and Chang limited their analysis to those cases with no other congenital disabilities reported, numerous studies have demonstrated the extremely poor sensitivity and positive predictive value of U.S. birth certificate reporting of congenital anomalies.1,2 In addition, limb reduction defects, a broad grouping of congenital abnormalities that includes the conditions referred to by Man and Chang as “congenital digit anomalies,” can involve a single limb, which might be an upper or a lower limb, and can involve laterality as well. No data on these issues are available from the birth certificate records.
The authors argue that their results demonstrate a dose-response relationship between maternal cigarette smoking and congenital digit anomaly. Space does not permit an extensive analysis of the validity and reliability of reporting of maternal smoking in vital records. While most perinatal epidemiologists regard these data as a reasonable proxy measure, they are loath to make generalizations concerning the timing of the exposure on the basis of these data alone. In fact, the 2003 national standard certificates of live birth and fetal death will contain more specific reporting of smoking by trimester, based in part on research conducted by Kharrazi et al.³

There are much better datasets to test the authors’ hypothesis. Several states, including New York, California, and Georgia (metropolitan Atlanta), have comprehensive congenital disabilities surveillance programs with records linked to vital statistics.⁴,⁵ In addition to much more detailed data on specific congenital disabilities, these data sources also permit an analysis of trends in congenital disabilities prevalence. Future research should focus on data sources with richer clinical details, rather than rely primarily on birth certificate data to document both exposure and outcome.

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A New Material for the Standard Burn Model

Sir:

With regard to the letter by Dr. Dinar, “A New Material for the Standard Burn Model: Allevyn Adhesive” (Plast. Reconstr. Surg. 117: 717, 2006), we know that experimental studies on burn therapy are essential before clinical use, but we also know that experimental burn models are very hard to create.

Dr. Dinar’s Allevyn adhesive dressing was placed with a central perforated area in the wound contact layer to produce the thermal lesion on the back of the animal. It was necessary to anesthetize the animal with 50 mg/kg sodium phenobarbital, and on the exposed cutaneous surface, 70°C water was used for 12 seconds.

In our experience, a major problem with this animal model is the kind of dressing used. Allevyn adhesive is not the most appropriate material because it does not adhere to rat skin. We have used another kind of material; our wound cover consists of a foam frame with a central window covered by a transparent film and a four-corner adhesive border to affix the wound cover to the rat skin (Fig. 1). The wound cover is really flexible and conforms to the rat’s movements.

Another advantage is that a disposable warming pad provides warming therapy. It is possible to prepare a warming bag for operating temperatures ranging from 35°C to 42°C or higher (Fig. 2). Considering the advantages of ease of application and resistance to keeping the dressing in situ, it is...
important to have a good animal burn model with no anesthetic problems.

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