GUIDELINES

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

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Letters

No Evidence for Long-Term Effectiveness of Early Osteodistraction in Hemifacial Microsomia

This letter was intended as a Discussion to the article entitled “No Evidence for Long-Term Effectiveness of Early Osteodistraction in Hemifacial Microsomia” by Nagy, Kuijpers-Jagtman, and Mommaerts in the December 2009 issue (Plast Reconstr Surg. 2009;124:2061–2071). It was inadvertently not published with the article. PRS regrets the oversight.

Sir:

When I was first asked to review this article for the Journal, it confirmed my own clinical impression, in my own patients, that following distraction osteogenesis in the treatment of hemifacial microsomia, there is a large degree of relapse that may occur. The original manuscript was somewhat direct and at times argumentative, and the other reviewers universally condemned it as being petty and perhaps argumentative. The original manuscript was rejected despite this reader’s recommendation that it be resubmitted with revision. An appeal was made to the editor that it be published and what has now emerged is a revised rendition, toned down in its rhetoric and tightened in its science. Standing alone, this discourse is an important contribution in this area, and despite its controversial nature, it certainly deserves publication.

In this work, the authors extensively reviewed the surgical literature between 2002 and 2008 for articles addressing treatment of hemifacial microsomia by distraction osteogenesis. The authors then coned down this list to those articles that dealt with early osteodistraction (before skeletal maturity) and had sufficient follow-up to be assessed. Length of follow-up, number of patients, age, classification, methods of analysis, validation, reliability, evidence-based medicine level, surgical outcomes, and complications were then analyzed. The authors concluded that:

1. The study designs were at times poor, with the majority of studies failing to segregate results based on Pruzansky-Kaban type.
2. No validation of the measurements of facial symmetry was forthcoming.
3. None of the studies showed convincing stability, with unpredictability being the norm. Regenerate relapse was seen in the majority of studies that had reliable evaluation.
4. Repeated distraction osteogenesis was found to be necessary to improve facial asymmetry during growth.
5. There were no reports that substantiated the belief that early distraction osteogenesis could significantly reduce the secondary maxillary deformity in hemifacial microsomia.
6. Repeated distraction osteogenesis may result in increased healthcare costs as opposed to waiting for a single-stage surgery completed at maturity.

The article concluded that “there is no convincing evidence supporting the effectiveness of early mandibular osteodistraction in hemifacial microsomia patients. Patients need to be informed that additional distraction procedures or definitive secondary surgery at maturity most likely will be required.”

This article, I believe, is mandatory reading for all surgeons working in this area. Hemifacial microsomia is, after all, the second most common congenital craniofacial deformity after cleft lip and palate, and patients with this disorder make up a large number of those treated in any busy craniofacial center. In addition, distraction osteogenesis has become the procedure of choice for most younger patients afflicted with this deformity. One may endlessly debate the merits of this review and the conclusions reached, and each of us likely has one or more patients in whom early distraction osteogenesis in hemifacial microsomia produced a profound and, at least in the early stages, lasting improvement. Anecdotally, I would hazard that each of us might initially view this work as interesting but not applying to the results he or she is
getting at his or her craniofacial center when treating hemifacial microsomia by distraction osteogenesis. To me, the value of this article is that it not only engenders debate but also forces us to look for ways to improve our own results. Distraction osteogenesis is a remarkable tool. It allows us to “create” bone (and, to a degree, soft tissue) where none or little existed before and, upon completion, to improve facial form, at least in the short term. This is tissue engineering at its finest. Rather than deny the fact that some of this correction may be lost to relapse, we need to think of the next steps to not only preserve these gains but perhaps promote further growth. For example, the application of rigid internal fixation has done much to obviate the relapse one sees with a traditional maxillomandibular advancement. I know of at least one surgeon who has applied this to mandibular distraction osteogenesis and plate fixes the basal bone and regenerate at the time of distractor removal when performed in adolescents and adults. Although this may not be recommended for patients in the younger age group, perhaps there is a role for fixating the regenerate in a shroud of resorbable plate or mesh (especially if one uses the first-generation resorbables, which have a 24-month degradation time) to limit the relapse. To this complex could then be added currently available growth factor, such as transforming growth factor such as TGF beta. On a different tack, undoubtedly there is a much larger role for interceptive orthodontics. The current article mentions these interventions but perhaps does not stress their importance enough. I, for one, remain firmly convinced that the use of bite plates to promote maxillary dental eruption also has a role in preserving mandibular regenerate length and volume. Properly positioned dental appliances and orthodontic bone anchors and judiciously applied splints not only preserve regenerate volume but may also promote regenerate and maxillary growth. The coordinated care of these patients with the orthodontist cannot be overemphasized and represents yet another opportunity for advancement. There are undoubtedly many other tools and techniques that could be applied to these patients.

Perhaps most importantly one should stress that having to do secondary surgery in an evolving (growing) facial deformity should not be looked upon as a negative or as a failure. I recall the days when we were first treating syndromic and nonsyndromic craniosynostoses via fronto-orbital advancement. The early promise was that by complete release and repositioning, we would somehow normalize growth and a single operation in infancy would be all that is required. This is certainly not the case, and many of these patients will need secondary interventions, some that duplicate in extent the original procedure and some of a lesser degree, adding contour and shape at a later date. This is not looked upon as a failure but more as completion procedures at a later date to optimize function and form. This is also true of the midface deformity that one sees in syndromic synostosis. Treatment by conventional methods in the early years often needs to be repeated, as there is a relapse or lack of subsequent sagittal growth. Again, this should not be abandoned and considered a failure, as it provides much improvement in the short term. Adding distraction osteogenesis to the midface advancement and overcorrecting, as the authors have recommended for distraction osteogenesis of the mandible in hemifacial microsomia, seems prudent.

In summary, this article’s value lies not so much in detailing our shortcomings in distraction osteogenesis, which it seems are many, but in pointing us in new directions. Distraction osteogenesis is here to stay. It is up to us as clinicians and researchers alike to make the next refinements that will improve the results of therapy in these patients. Distraction osteogenesis is but one arrow in our quiver, and it is time that we unsheathe the others. The authors are to be commended for their thoughtful and enlightening discourse.

DOI: 10.1097/PRS.0b013e3181dab63e

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Congenital Melanocytic Nevi of the Eyelids and Periorbital Region

Sir:

With regard to “Congenital Melanocytic Nevi of the Eyelids and Periorbital Region,” having treated a few of these patients, this is a very important article, as it gives us guidance and allows us to see other people managing these difficult cases. We agree with the authors that for reconstruction of lesions involving the upper and lower eyelids, a single, large, full-thickness graft is preferable to multiple small grafts to avoid a patchwork appearance. The authors discuss their experience of expanding supraclavicular skin for this purpose. We have found that this technique can also be used to recruit donor skin for full-thickness grafting from the postauricular site, which we feel gives a better color match for the eyelids (Fig. 1). By preexpanding the postauricular site, we have been able to obtain a full-thickness graft that is large enough to reconstruct upper and lower eyelids and can still close the donor site directly for harvesting full-thickness skin. This is one adjunct that does help in the management. Most of these patients at some time need minor revisions to gain and improve a satisfactory appearance, and occasionally there are some recurrences that need excision.

DOI: 10.1097/PRS.0b013e3181d512aa

Congenital Melanocytic Nevi of the Eyelids and Periorbital Region
Sir:

I read with interest the article entitled “Lateral Orbicularis Oculi Muscle Plasty in Conjunction with Face Lifting for Periorbital Rejuvenation” by Cabbabe, Andrades, and Vaconez (Plast Reconstr Surg. 2009;124:1285–1293) and have the following comments. The authors’ purpose was to conduct a study evaluating the lateral orbicularis oculi muscle plasty as an alternative periorbital rejuvenation technique during face lifting in a total of 76 patients with midface lift.

They conclude that lateral orbicularis oculi muscle plasty is a safe technique that may be considered a good alternative for periorbital rejuvenation and may help in avoiding lower lid incisions or extensive face lifting in some cases. It is not an original description, because many authors have reported the use of orbicularis oculi muscle as an adjuvant mechanism in the treatment of periorbital aging. Authors such as Skoog, Aston, and McCord et al.1–3 have demonstrated by several mechanisms an effective treatment of crow’s feet and periorbital fold, changing the position, the innervation, and the volume of orbicularis oculi muscle, with minimal complications.

On June 12, 2009, my article entitled “Periorbital Rejuvenation: A Safe Subcutaneous Approach to Forehead, Eyebrow, and Orbicularis Oculi Muscle Mobilization”4 was published in Aesthetic Plastic Surgery (online). The purpose of the article was to present a treatment of periorbital aging in women of middle age. In this article, I proposed a wide temporal, malar, and frontal subcutaneous undermining of the skin from the temporal incision to the lateral aspects of the eyebrows, lids, and forehead and a superficial and deep dissection of the lateral third of the orbicularis oculi muscle.

Subcutaneous dissection is safe because it avoids injury to the frontal branch and also permits easy reach to the orbicularis oculi muscle and the external aspect of the malar fat. Deep muscular dissection contributes to partial denervation of the orbicularis oculi muscle, decreasing production of crow’s feet. The keys of this technique are three principal suspensions, in the superolateral direction:

1. The orbicularis oculi muscle is suspended to the superficial temporal fascia, which is splayed out with redraping of orbicularis oculi muscle while a face lift is performed when the skin attached on the muscle is also tractioned; this muscular redraping completes the treatment of crow’s feet.
2. The malar fat also is suspended to the superficial temporal fascia, permitting not only its “juvenile” rejuvenation but also a superolateral suspension of the cheek fat, improving the nasolabial fold.
3. Suspension of the skin of the temporal, lids, and periorbital areas contributes to recuperation of the tension in flaccid superficial periorbital, inferior lid, and cheek tissues.

This technique permits a reliable and safe reduction of crow’s feet, skin flaccidity, replacement of the malar fat, light or moderate correction of the nasolabial fold, and a beautiful and rejuvenated upward slant position of the lower lids. This important article by Cabbabe et al. confirms the value of orbicularis oculi manipulation in the treatment of periorbital aging and for minimizing surgical trauma to the lower lid, but I think that rejuvenation of both middle facial and periorbital areas can be enhanced when a more complete and aggressive technique is applied.

DOI: 10.1097/PRS.0b013e3181d5136b0

REFERENCES


Lateral Orbicularis Oculi Muscle Plasty in Conjunction with Face Lifting for Periorbital Rejuvenation

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Fig. 1. Patient showing postauricular tissue expander in situ.
Sir:

We thank Dr. Guerrissi for his letter expressing interest in our article. We will address the points on which he has commented.

Dr. Guerrissi describes an aggressive approach to periorbital rejuvenation, including undermining of the lateral orbicularis oculi, hypothesizing that this helps denervate the lateral orbicularis to eliminate crow’s feet. He also states that “suspension of the orbicularis oculi muscle contributes not only to correction of the malar crescent but also to lifting of malar fat.” A total of 33 percent of his patients had upper and lower blepharoplasties performed incorporating skin and fat resection in conjunction with the orbicularis manipulation. In addition, a total of 10 percent of his patients had a conventional face lift performed concurrently. His technique was applied as a secondary procedure after conventional blepharoplasty for 16 percent of his patients. We had a conventional face lift performed concurrently for 16 percent of his patients.

Our technique is not an original description. Our discussion reviewed several different techniques of orbicularis manipulation, including those described by Skoog, Aston, Connell and Marten, Camirand, Fogli, and Viterbo. Our techniques most resemble those described by Skoog, Aston, and Connell and Marten.

The purpose of our article was to describe a technique that may be used in select patients to eliminate the need for lower blepharoplasty incisions and fat resection to decrease the complications associated with these procedures. We do not agree with Dr. Guerrissi in his belief that orbicularis suspension also corrects the malar crescent and lifts the malar fat. Rather, our technique is performed in conjunction with midface lifting because we believe that any lower lid procedure should include malar fat elevation to recruit descended cheek tissue and support the lower lid. We prefer to resect the lateral portion of the orbicularis as shown by Viterbo or to splay out the orbicularis with two sutures and to resect a triangle of muscle at the 3-o’clock (or 9-o’clock) position as shown by Connell and Marten. In our opinion, there is less need for fat removal from the lower lid because suspension of the orbicularis tightens the septum.

Our complication rate for midface lift with lower blepharoplasty was 46.7 percent. This decreased to 28.6 percent when orbicularis suspension was added to midface lift and lower blepharoplasty, then decreased to 16.2 percent when midface lift and orbicularis suspension were performed without lower blepharoplasty. Lateral orbicularis suspension also improved the aesthetic outcome over lower blepharoplasty. We thank Dr. Guerrissi for his comments and interest in our article.

DOI: 10.1097/PRS.0b013e3181d513d9

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REFERENCE

A Novel Pain-Alleviating Adjunct to Dermal Fillers and Neurotoxins

Sir:

I read with interest the article entitled “Minimizing Pain, Maximizing Comfort: A New Technique for Facial Filler Injections” published recently in the Journal. Drs. Rohrich and Herbig allude to the increasing numbers of patients who have either dermal fillers or neurotoxin injections for restoration of a more youthful facial appearance. They likewise espouse the necessity for a preinjection anesthetic for a more comfortable patient experience. They recommend the off-label addition of 2% lidocaine to a number of fillers. The other viable alternatives to help alleviate the pain of cosmetic injectables include anesthetic creams, vibration devices, ice, cool air, nerve blocks, local infiltration of lidocaine, and contact cooling devices. The anesthetic creams require some 30 to 45 minutes to take effect, and their numbing effect may last long after the injectable procedure. Ice, because it freezes at the freezing point of water, can cause a frostbite-like feeling.

Plastic and Reconstructive Surgery • May 2010

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DISCLOSURE

The author has no financial interest to declare in relation to the content of this communication.

REFERENCES

that can be painful to the patient. The addition of local lidocaine into the area of anticipated injectable placement can distort the area, making the judgment of how much filler to place difficult. The injection of the lidocaine can cause a bruise and can take some time to set up. Vibration devices, cool air–blowing machines, and contact cooling devices can be costly and may require the aid of an assistant for easy application.

I describe a novel, patent-pending adjunct to dermal filler or neurotoxin injections that addresses all of these shortcomings stated above. The Ouchless Needle (Fig. 1) is a syringe-attached vapocoolant dispenser that is activated with one hand just before dermal filler or neurotoxin injections. It sprays a mixture of 1,1,1,3,3-pentafluoropropane and 1,1,1,2-tetrafluoroethane, both of which are nonflammable, chlorofluorocarbon-free vapocoolants. The Ouchless Needle vapocoolant spray mixture numbs instantly, obviating the 45-minute waiting period for topical creams, and has a few-second duration of effect. Because neurotoxin and filler injections are sequential and in multiple noncontiguous locations, the addition of lidocaine to the dermal filler may not produce enough of a numbing effect to help with the subsequent injection. I still add lidocaine to the fillers, as I believe this adds to the decrease in postinjection pain and throbbing.

The efficacy in pain reduction of vapocoolants as compared with topical creams for decreasing pain of intravenous cannulation1,2 in children and immunization pain3 has been described in the literature. Weiss and Lavin4 have recently published a split-face study of botulinum toxin type A patients in which half the face was sprayed with a vapocoolant and no treatment was applied to the other half. Overall, 67 percent of patients reported that the vapocoolant side had less pain and 54 percent preferred vapocoolant for their next treatment.

The addition of a syringe-attached vapocoolant dispenser such as the Ouchless Needle can make for a more comfortable injection experience for our patients, which may allow us to reach out to those patients who would otherwise not consider an injectable cosmetic enhancement secondary to fear of the needle pain. It will also alleviate the patient’s time commitment, allowing for a shorter appointment time.

DOI: 10.1097/PRS.0b013e3181d5136f

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DISCLOSURE

The author is the chief executive officer and founder of the BellaNovus Development Company (Louisville, Ky.), which makes and distributes the Ouchless Needle device.

REFERENCES


Pediatric Nasal Reconstruction for Nasal Tip Hemangioma

Sir:

We read with great interest Dr. Burget’s recent article on pediatric nasal reconstruction. Dr. Burget is well known for his special interest and phenomenal results in nasal reconstruction. We would like to congratulate him for the unique and very detailed case presentation and the excellent result achieved.

We have had a similar case of nasal tip hemangioma extending to the upper lip on a female pediatric patient (Fig. 1). Our approach was completely different from Dr. Burget’s. We chose to follow a “wait-and-see” approach initially, until involution of the hemangioma was evident. Our experience with this patient contrasts with Dr. Burget’s observations, that “when a hemangioma necroses, collagen contraction shrinks the nasal lining skin and displaces the alar cartilages posteriorly and superiorly.”

The patient was followed up until the age of 9 years (Fig. 2), when a very conservative excision of the excess

Fig. 1. The Ouchless Needle on a Juvederm (Allergan, Inc., Irvine, Calif.) syringe.
skin on the nasal tip and dorsum was performed, following the shrinkage of the hemangiomatic tissue (this was probably the reason why tissue shrinkage was minimal).

At the age of 16 years (Fig. 3), a second operation was performed, where the nose was reconstructed using an open rhinoplasty method. We used a columella strut graft, spreader grafts, and lower lateral grafts with the septum as a donor site.

At the age of 24 years (Fig. 4), a third operation was performed for the second stage of the nasal reconstruction. The hump was reduced and lateral crural strut grafts were used. We believe our approach has achieved acceptable results, and we would like to add our experience to the excellent and fascinating case presented by Dr. Burget.

DOI: 10.1097/PRS.0b013e3181d51384

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Reply

Sir:

Congratulations to Dr. Mandrekas and associates on the successful example of nasal reconstruction in a child. The beautiful result is testimony to having made all the right decisions. I am especially pleased that techniques of open rhinoplasty were used, as I have also used these in my practice. These techniques are useful when the anterior nasal septum is present. In patients where the anterior septum is missing, other more complex techniques for support of the reconstructed nose are required.

The congenital hemangioma seen in Dr. Mandrekas’ patient had undergone involution, not necrosis, so that the entire integument of the nose was still present. The patient possessed a slightly bulky nose, but not a horribly deformed one. Therefore, her operations were rightly postponed until ages 9 and 16 years. In many such cases, the congenital hemangioma has undergone frank necrosis and fallen away as a result of autolytic débridement. Alternatively, a surgeon has, with good intention, excised the tumor mechanically and with it a part of the underlying normal, natural nose. The resulting nasal deformity can be quite severe, which may in turn have negative and lasting psychological and social repercussions. In these instances, it is a mistake to allow a child to grow up without correction of the nasal deformity. Reconstruction performed after a child has grown is done too late to avoid the social isolation and stigma of a facial deformity when school attendance begins. It is my firm belief that a severe nasal deformity should be corrected through staged procedures beginning at age 3.5 years. The child can then enter school at age 5 years appearing normal and possessing the confidence that such an appearance bestows. Skin grafts, composite grafts, and free microvascular flaps may temper or mute a severe deformity. However, only a vertical forehead flap with a cartilage graft framework eradicates it. Had I seen the 9-year-old patient presented in my article at age 3.5 years, I would have performed a forehead flap reconstruction at that time.

Each patient is an individual who presents with unique challenges. In children especially, one must consider the negative social impact a marked deformity may have on the child and their parents and siblings. Patients often suffer from an overwhelming sense of inferiority and shame, and the formation of a well-adjusted personality and appropriate social skills may be inhibited. In an early article on the subject, Dr. Caspar M. Epsteen expresses the core of the problem:

“There are three important psychological effects of deformity: (1) inferiority and shame, (2) modification of self-expression, and (3) antisocial tendencies. Children are notoriously observant of the unusual, and are cruel; they have no inhibitions. A great deal of undue attention is invariably directed to any cosmetic abnormality. There is no attempt on the part of a child to... refrain from ridiculing.”

Dr. Epsteen goes on to state that children with facial deformities are often shunned or forced into an inferior social status. Thus, in the pediatric patient population, appropriately applied and well-timed aesthetic reconstructive surgical intervention is of paramount importance. It is our professional duty to familiarize ourselves with and use all of the tools at our disposal to, as Gaspare Tagliacozzi noted in 1597, “bring back, refashion, and restore to wholeness the features that nature gave but chance destroyed.”

DOI: 10.1097/PRS.0b013e3181d51463

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REFERENCE


Reducing Scar Formation after Lip Repair by Injecting Botulinum Toxin

Sir:

We read with great interest the recent article “Intracutaneous Botulinum Toxin Type A Injection as a New Treatment Measure for Keloids.” We would like to take the opportunity to further expand on the topic, with injection of botulinum toxin to reduce scar formation after lip repair for cleft lip.

One of 500 to 1000 babies is born with a cleft. 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, surgical intervention leads inevitably to scar formation. Scars are usually termed hypertrophic when they develop excessive redness, elevation, widening, and stiffness. The presence of serious scarring limits social interaction and affects self-esteem after lip repair. Surgeons have been focusing on how to make this scar less conspicuous and how to obtain a straighter and more natural-appearing suture line. Carefully designed incision lines, anatomically appropriate muscle reconstruction, and atraumatic suturing are essential for optimal scars. However, because of continuous muscle contraction and skin tension caused by infant crying and feeding, we have difficulty in achieving a satisfactory effect after lip repair. Surgeons have had to use a lip bumper to protect the wound and reduce continuous muscle contraction and skin tension. How can we further reduce scar formation after lip repair?

Botulinum toxin is the universally accepted standard treatment for upper facial rejuvenation. Botulinum toxin use in the mid and lower face and neck has become an increasingly popular indication for facial aesthetic treatment. Botulinum toxin acts by inhibiting...
acetylcholine release at the neuromuscular junction. The toxin has an influence on muscular activity alone. When injected into hyperactive corrugator and/or procerus muscles that predominantly control frowning, botulinum toxin produces a localized muscle weakness, resulting in a temporary improvement in the glabellar frown line. On the basis of the above facts, we hypothesized a possible method of reducing scar formation after lip repair by injecting botulinum toxin.

When skin and organs are damaged, the body naturally wants to heal itself. When the wound heals completely, it is known as a scar. Although scar tissue takes the place of damaged or destroyed tissue, it is limited in function, including movement, circulation, and sensation. Scars can never be completely removed. Cleft lip scars have a particularly serious impact on appearance. Many factors affect the formation of scar. However, continuous muscle contraction plays a key role in the formation of lip scar. The surrounding tissue is compressed because of muscle contraction. The skin tension will increase. In patients with reconstructed cleft lips, the expanding muscle is interrupted in the region where scar tissue is supposed to be present. After injection of botulinum toxin locally into lip muscles, the toxin enters the nerve terminal by means of endocytosis, interacts with intracellular proteins, and inhibits the vesicular release of the acetylcholine neurotransmitter at the neuromuscular junction. Inhibition of acetylcholine produces chemical denervation and paralysis of the lip muscles. Botulinum toxin will produce a localized muscle weakness. The method of local injection of botulinum toxin into lip muscles may reduce the continuous muscle contraction and skin tension and create good conditions for wound healing and reduction of the formation of lip scars after lip repair. Many surgeons prefer removing scars by means of surgery, but any operation will always create a new scar. The former scar may be less obvious, but there is the risk of recurrence and worse scarring. Botulinum toxin has been used for depressor upper lip wrinkles. It may have effective clinical applications for reducing scar formation after lip repair.

DOI: 10.1097/PRS.0b013e3181d51404

REFERENCES


Reply

Sir:

I carefully read the letter entitled “Reduce Scar Formation after Lip Repair by Injecting Botulinum Toxin” with great interest and am pleased to reply here. The main content of this letter was to reduce hypertrophic scar after operation to repair cleft lip by using botulinum toxin type A. Because the cause of hypertrophic scar formation is not fully understood, the clinical management of hypertrophic scar remains problematic. Although numerous treatments are currently available, including surgical excision, steroid injection, radiation therapy, laser, and pressure therapy, few can achieve excellent therapeutic results. Thus, I firmly...
believe that it is necessary to find a better alternative method for treating hypertrophic scar.

On the basis of the above background, some scholars, including Professor Shu-juan Zou, the corresponding author of this letter, are paying more attention to the application of botulinum toxin type A in treating hypertrophic scar. In fact, some clinical observations about improving the eventual appearance of hypertrophic scar by using botulinum toxin type A had been reported several years previously.1,2 Compared with the articles published previously, the purpose of this letter is to give the reader some explanation regarding the reasons for using botulinum toxin type A in treating hypertrophic scar, and their explanation is reasonable and logical. I think that the greatest obstacle to the wide application of botulinum toxin type A in controlling hypertrophic scar is the lack of good understanding of the molecular mechanism of action of botulinum toxin type A on hypertrophic scar. In other words, the molecular mechanism has not been clearly elucidated, with botulinum toxin type A never having been used widely in treating hypertrophic scar. Therefore, it is very important to explain the molecular mechanism of action of botulinum toxin type A on hypertrophic scar. Apart from Professor Shu-juan Zou’s viewpoint that botulinum toxin type A improves hypertrophic scar by decreasing tensile force during the course of hypertrophic scar formation, I raised three new points regarding this problem. First, I reviewed some articles published previously. Some scholars have reported that botulinum toxin type A could promote the atrophy of benign prostatic hyperplasia by inducing apoptosis of prostatic epithelium and inhibiting the growth of prostate cancer by inducing apoptotic cancer cells.3,4 Botulinum toxin type A could also induce the temporary apoptosis of nasal glandular cells.5 These articles led me to consider the relationship between botulinum toxin type A and cellular dynamics of fibroblasts derived from hypertrophic scar. My colleagues and I have carried out some experimental research. We found that botulinum toxin type A could influence the cell cycle of fibroblasts derived from hypertrophic scar, and botulinum toxin type A can inhibit the proliferation and promote apoptosis of fibroblasts derived from hypertrophic scar.6,7 Second, botulinum toxin type A could be closely associated with transforming growth factor (TGF)-β1. As is known, TGF-β1 plays an important role in the formation of hypertrophic scar. The high expression of TGF-β1 has obviously promoted the formation and growth of hypertrophic scar. Recent reports have shown that botulinum toxin type A can reduce the expression of TGF-β1 protein in fibroblasts derived from an in vitro experiment.8 The latest advancement I mentioned may partly explain the molecular mechanism of action of botulinum toxin type A on hypertrophic scar. Of course, I have believed that the limitations of the above finding cannot be ignored. The findings were obtained only in fibroblasts cultured in vitro. The in vitro environment may cause some experimental error. Thus, the in vivo experiment should be performed to strengthen the finding. Third, according to my clinical and experimental experience, I believe that botulinum toxin type A can exert better effects on hypertrophic scar during the earlier period of formation of hypertrophic scar because the fibroblasts in the earlier period have stronger activities of proliferation and apoptosis, which is favorable for the influence of botulinum toxin type A on cellular dynamics.

In treating hypertrophic scar, the generalization of use of botulinum toxin type A needs more potent support from basic experimental and clinical observations on a large scale. We must be aware that much work needs to be done regarding this problem. The reasonable use of botulinum toxin type A depends on good understanding of its pharmacologic action and deeper comprehension of the molecular processes regarding the formation of hypertrophic scar. Although many problems need to be solved in this area, I still believe that botulinum toxin type A is promising for treating hypertrophic scar.

DOI: 10.1097/PRS.0b013e3181d51419

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REFERENCES

The Price of Pathology: Is Screening All Breast Reduction Specimens Cost Effective?

Sir:

We read with interest the article entitled “Incidence of Precancerous Lesions in Breast Reduction Tissue: A Pathologic Review of 562 Consecutive Patients” in the October of 2009 issue of Plastic and Reconstructive
In their article, the authors present the first study to assess a patient's individual breast cancer risk on the basis of breast reduction specimen findings by applying the College of American Pathologists consensus statement. The group included 562 consecutive patients seeking breast reduction at a single institution; the mean age of the study group was 43 years. This included patients with a history of (or current) breast cancer seeking balancing reductions with concomitant breast reconstruction. They state that 11.2 percent of the group had a personal history of breast cancer before reduction, which is not typical of the macromastia population. They conclude from their study that no invasive carcinoma was found in the reduction specimens; however, over 50 percent had benign or precancerous breast lesions. They found, as expected, that patients with a personal history of breast cancer were more likely to have ductal or lobular carcinoma findings in reduction specimens, and younger patients were more likely to have normal findings.

Our review of the article yielded the following few comments. First, this is not a homogeneous study population. To extrapolate pathologic findings in this group to the typical population seeking reduction mammoplasty would not be entirely accurate. Second, it is unclear what perioperative guidance the authors recommend for patients found to have an increased risk of breast cancer based on this study. Third, in this era of cost containment and population-based health care reform, the cost of routine pathologic sampling must be considered.

In 2007, at the 24th Annual Meeting of the Northeastern Society of Plastic Surgeons, our group presented a study on the cost of sending all reduction specimens for pathologic evaluation. As the current study documents, we found no cases of occult carcinoma in our series. Benign pathologic findings were common, being present in 86.4 percent. This routine pathologic examination is costly. The internal institutional cost averaged $65 per breast reduction specimen. The external costs averaged $118 per specimen as measured by Medicare data. The contribution margin per specimen to the Department of Pathology was approximately $50. The American Society of Plastic Surgeons reports that 212,358 breast reductions were performed in the United States in 2007. Therefore, the total cost to the healthcare system for microscopic examination of breast reduction specimens approaches $25,058,244 annually. Given the estimated incidence of occult carcinoma referenced by the author, it costs $236,000 to diagnose one breast cancer.

We have subsequently looked more closely at the adolescent breast reduction population (younger than 18 years) in our institution over the past 10 years. Pathologic examination yielded entirely normal or mildly fibrotic histologic findings in 80 percent of specimens. Benign pathologic findings were observed in the remaining 20 percent, including fibroadenoma, fibrocystic change, ductal hyperplasia (without atypia), and other benign pathologic findings. No cancers or precancerous lesions were identified. Using the same cost data as above, and realizing the incidence of adolescent breast cancer in 0.08 cases per 100,000, the cost of one cancer diagnosis is $147 million to the healthcare system. These results are in their final stage of write-up and will be accompanied by indications, techniques, and outcomes in a future submission for publication.

The incidence of occult carcinoma identified in the reduction mammoplasty samples of the macromastia population is low. Serendipitous identification of such cancer is expensive. The cost effectiveness of routine pathologic examination following reduction mammoplasty is questionable and should be reconsidered. This is especially true in the younger patient population and the patient without prior personal history of breast cancer.

DOI: 10.1097/PRS.0b013e3181d5139e

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REFERENCES

Reply
Sir:

We appreciate the insightful comments provided by Drs. Koltz and Girotto regarding our article on the incidence of precancerous breast lesions in breast reduction tissue and look forward to their contribution to this topic in the near future. In response to specific comments, we recognized that breast cancer patients may be overrepresented in our patient group, but we felt it to be important to include this subgroup in our analysis because a history of breast cancer would likely increase one’s incidence of contralateral breast disease.

Second, this study emerged as a result of patients questioning our surgeons on their risk of finding abnormal breast tissue at the time of reduction. As a result of this study, we have made concerted efforts to counsel our patients on the likelihood of identifying atypical ductal hyperplasia, atypical lobular hyperplasia, lobular carcinoma in situ, or ductal carcinoma in situ and the
subsequent follow-up with an oncologist if indicated. Our report was not a longitudinal study designed to define a patient’s risk of breast cancer as a result of incidental precancerous lesions identified at the time of reduction mammoplasty. The clinical relevance of precancerous breast lesions discovered in breast resection specimens remains to be defined. However, in a Danish population-based study, Baasch et al. reported that reduction mammoplasty can reduce one’s risk of breast cancer.1

In light of recommendations by the U.S. Preventive Services Task Force that only women older than 50 years should undergo biennial screening mammography, patients found to have incidental precancerous lesions that are younger than 50 years may constitute a subgroup of the population that could benefit from earlier screening.2 In addition to postoperative screening mammography, this subgroup may benefit from treatment with a selective estrogen receptor modulator (tamoxifen or raloxifene) as demonstrated in the National Surgical Adjuvant Breast and Bowel Project Study (tamoxifen or raloxifene) as demonstrated in the National Surgical Adjuvant Breast and Bowel Project Study—A prospective study.

Lastly, the cost of processing pathology specimens is a serious concern. We reported that 1.1 percent of patients will have incidental ductal carcinoma in situ in their reduction specimen, and recently, Ambaye et al. reported that with extensive tissue sampling, 2.5 percent of patients will have incidental ductal carcinoma in situ or invasive adenocarcinoma of the breast.3,4 Thus, based on crude calculations, routine pathologic sampling of reduction mammaplasty specimens costs $2250 to $10,719 per diagnosis of ductal carcinoma in situ or breast cancer. Although this seems to be cost-effective and is similar to mammographic screening costs, formal analyses should be performed that factor a patient’s actual risk of breast cancer and quality-adjusted life-years.

DOI: 10.1097/PRS.0b013e3181d5142b

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REFERENCES

Surgical Correction of Symmastia

Sir:

In 1990, we reported our technique of surgical correction of inferior capsular ptosis after breast augmentation using a posterior capsular flap for correction of inferior displacement of breast implants.1 Because of recent interest in surgical management of symmastia,2–4 we would like to report our approach to this challenging problem with the use of posterior capsular flaps as shown in Figure 1.

Most recently, in 2009, Spear et al. reported their technique of symmastia correction using a “neosubpectoral” pocket.3 Their technique consists of creating a subpectoral pocket between the anterior capsule and the muscle. The authors state that although the plane of dissection is most distinct, the capsule often thins and can be very adherent to the deep surface of the pectoralis muscle as one proceeds cephalad.3 In our experience, the dissection of the anterior capsule off the overlying pectoralis muscle has been fraught with difficulty in a relatively large percentage of patients who undergo routine capsulectomies because of the thinness of this structure and its dense adherence to the overlying muscle. In these situations, we find that the separation is difficult, if not impossible at times, and if one perseveres, it is bloody and time consuming. In such instances, we opt against total capsulectomy and proceed instead with partial capsulectomy, leaving behind the very thin capsule that coats the undersurface of the pectoralis muscle. Obviously, when a thick and mature capsule is present, these layers may be dissected easily, quickly, and relatively bloodlessly. Spear et al. also report on the necessity to “obliterate” the midline symmastia space with a medial capsulorrhaphy using as many interrupted and running sutures as seem necessary.3 They state that these sutures “serve more to stabilize the anterior and posterior leaves of the old capsular space and potentially obliterate it.”5 However, it seems to us that medial capsulorrhaphy as described by these authors plays an important role in defining the medial borders of the breasts, perhaps more so than the neosubpectoral placement of the implants.

When the posterior capsule lends itself to flap elevation, we correct symmastia as follows. We excise an approximately 2-cm-wide strip of anterior and posterior capsular layers from the presternal region, to create adhesions between the skin and the sternum (Fig. 1).
Then, a medially based posterior capsular flap is raised and the edges are approximated to the anterior capsule with a few interrupted 4-0 monofilament absorbable sutures. Drains are placed and this is followed by the insertion of implants. This technique lends itself to both subglandular and subpectoral symmastia (Fig. 1). Figure 2 shows the preoperative and postoperative photographs of a patient with subpectoral symmastia. Although our experience with correction of symmastia is limited, we have found this technique to be reliable, quick, and easy to perform, provided that a dependable posterior capsule is present. Otherwise, we prefer a two-stage operation with medial capsulectomies as the first stage to obliterate the presternal space followed by reaugmentation at least 6 months later.

DOI: 10.1097/PRS.0b013e3181d513f0

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REFERENCES

A Unique Abdominoplasty Approach in Management of “Buried” Penis in Adulthood

Sir:

Our recently published article, “Management of ‘Buried’ Penis in Adulthood: An Overview,” provided operative strategies in the management of the buried penis. In the article, we describe operative approaches to the penis and adjunctive procedures, such as panniculectomy/abdominoplasty and lipectomy of the pubis if necessary. The lipectomies described are generally similar to standard techniques; however, we would like to further describe our approach (Fig. 1).

In addressing the features of the abdominoplasty/panniculectomy, we preoperatively mark the patient in a way that resembles a W excision (Fig. 2). The mid-infraumbilical region is marked resembling a W or bullhorn pattern that will fit into the mons region resection. This curvilinear mons resection allows adequate fat removal and creates a central line of tension, further exposing the penis (Fig. 3). The skin above the penis base preferably remains intact. The lateral abdominoplasty resection extends to the anterior superior iliac spine, resulting in minimal dog-ear deformity. The Scarpa fascia and subcutaneous fat are reapproximated with absorbable suture. The lateral skin is reapproximated with absorbable monofilament suture followed by surgical glue. The medial skin W-shape region is closed with interrupted 3-0 permanent vertical mattress suture (Fig. 4). Bilateral 19-French drains are placed and secured. An abdominal binder is used at all times.

DOI: 10.1097/PRS.0b013e3181d5166b
Alternative Vascular Pedicle of the Anterolateral Thigh Flap: Does an Oblique Branch Really Exist?

Sir:

We read with interest the article by Wong et al.1 and the recently published letter to the editor by da Costa and Lancellotti2 concerning the oblique branch of the lateral circumflex femoral artery. Wong et al. described a distinct vascular pedicle lateral to the descending branch of the lateral circumflex femoral artery (which was present in 35 percent of their clinical cases) and stated that this vessel was as yet unnamed.

A claim to have identified a completely new branch in the well-known vascular system of the lateral circumflex femoral artery should, however, be made with caution. It implies of course a thorough understanding of the valid international terminology and the anatomical literature. Moreover, this finding should be based on an anatomical study with a representative number of specimens. According to the description of Wong et al. and the diagram in Figure 1, the lateral circumflex femoral artery splits into two main branches, a transverse and a descending one. The branches between these main branches are designated as the new “oblique branch.” This is in contrast to international anatomical terminology3 and the German and English literature, which do not list an oblique branch of the lateral circumflex femoral artery.3,5 This, of course, would not exclude the existence of such a branch. However, following the “classic” descriptions, the lateral circumflex femoral artery splits into three branches, the ascending, the transverse, and the descending branches. According to the description of Thiel, the ascending branch crosses underneath the rectus femoris muscle to supply the tensor fasciae latae muscle. The transverse branch generally arises from this ascending branch and runs laterally and ventral to the vastus intermedius muscle. Most of its branches enter the vastus lateralis muscle. The descending branch, which may arise from the femoral artery directly, runs distally between the rectus femoris and vastus intermedius muscles. The same description can be found in Gray’s Anatomy6 and in the corresponding author’s recently published book.6,7 By applying these descriptions to the work of Wong et al. and the diagram, the so-called oblique branch could in fact be the transverse branch of the lateral circumflex femoral artery.

In contrast, in this study, the “oblique branch” was only present in 35 percent of the cases, which is in contrast to the constant presence of this branch in the anatomical literature. This difference can be explained by the design of the study. It was performed on clinical cases, and the “oblique branch” was only dissected when present during the harvest of an anterolateral thigh flap. It is not clear and not mentioned by the author whether the dissection of the remaining flaps was carried out in all clinical cases to a point where an “oblique branch” could have been detected. This would mean that the remaining 65 percent of the cases were not included in this study—and perhaps more proximally located transverse branches were simply missed.

The possibility of harvesting an anterolateral thigh flap based on an alternative pedicle is helpful, and the efficiency of this procedure has been proved by the authors. Nevertheless, the designation of a new branch of a well-described vascular system should be made on the basis of the international anatomical nomenclature and literature, and it should be made on the basis of an anatomical study rather than on clinical cases.

DOI: 10.1097/PRS.0b013e3181d51655

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REFERENCES


Reply

Sir:

We thank Drs. Hubmer and Feigl for their comments. We are well aware of the classic description of branches of the lateral circumflex femoral artery system. We agree that, based on these descriptions, the oblique branch could perhaps have been a low-lying transverse branch. In fact, that was our initial conclusion when we first noticed this vessel many years ago. However, with careful analysis and experience, it is quite clear that this vessel is not the transverse branch but a new vessel that we have called the oblique branch of the lateral circumflex femoral artery.1 As demonstrated in our article, from the standpoint of the use of the anterolateral thigh fasciocutaneous or myocutaneous flap, it is a vessel of profound clinical significance.2

The description of a “new” branch of the lateral circumflex femoral system is a responsibility that we do not take lightly. Accordingly, we have performed cadaver dissections to meticulously study the lateral circumflex femoral system before performing the clinical series (Fig. 1). Having performed the anatomical study and with the benefit of an extensive experience with the anterolateral thigh flap, a prospective clinical study was performed. The benefits of a clinical study over a cadaver study are that in addition to noting its prevalence, the reliability of the oblique branch as the flap pedicle and adequacy for microsurgical anastomoses can be evaluated conclusively. We found in our study that the oblique branch is present in 35 percent of cases and originates most commonly from the descending or the transverse branch (88 percent). The transverse branch, in contrast, is always present and identified when we traced the flap pedicle to its origin at the lateral circumflex femoral artery (i.e., it is never missed as the authors erroneously suggested).1 Finally, one only has to note the occasional “extreme” cases encountered to be convinced that the oblique branch exists (Figs. 2 and 5).1

Multiple types of the so-called anatomical variation of the anterolateral thigh flap have been described.3–5 Much of the past confusion regarding the surgical anatomy and failure of the anterolateral thigh flap could now be attributed to the unrecognized presence of the oblique branch of the lateral circumflex femoral artery. The description of the oblique branch is a certainly a step forward in our understanding of the surgical anatomy of the anterolateral thigh flap. To question its existence would take us back into the confusion that was so pervasive before. From a surgical standpoint,

Fig. 1. Photograph of a cadaver specimen showing an oblique branch arising from the descending branch of the lateral circumflex femoral artery. As demonstrated here, the oblique branch is an additional vessel that may be present in the lateral circumflex femoral system. The transverse branch, which is always present, is limited to the proximal thigh and runs in a more transverse and cephalic direction. LCFA, lateral circumflex femoral artery; TFL, tensor fasciae latae.
knowing that either the descending or the oblique branch of the lateral circumflex femoral artery can be used as the flap pedicle is a psychological breakthrough and liberates one from the need to base the flap on the descending branch in every case. It encourages the move away from the conventional approach to flap harvest to one that embraces the free-style approach.6

DOI: 10.1097/PRS.0b013e3181d51682

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DISCLOSURE

Neither of the authors has a financial interest in any of the products, devices, or drugs mentioned in this reply or the article being discussed.

REFERENCES


Comparison of the Vascularity of Fasciocutaneous Tissue and Muscle for Coverage of Open Tibial Fractures

Sir:  

We have read the article entitled “Comparison of the Vascularity of Fasciocutaneous Tissue and Muscle for Coverage of Open Tibial Fractures” by Lorraine Harry et al. published in Plastic and Reconstructive Surgery (2009;124:1211–1212). In that experimental study, the article by Calderon et al. entitled “Comparison of the Effect of Bacterial Inoculation in Musculocutaneous and Fasciocutaneous Flap” published in Plastic and Reconstructive Surgery (1986;77:785–754) is listed as a reference. This article was awarded first prize in the basic science category in the 1984 Plastic Surgery Educational Foundation contest, and it has been a reference in many others articles regarding these flaps.

Their study is well designed and the conclusions are valid. Since our study in 1986, we have operated on patients with open fracture with osteomyelitis using muscle, mainly rectus abdominis muscle free flaps in the distal third of the leg, with good results. In the open fractures without osteomyelitis and with extension no more than 5 cm in diameter, in the same area, we have published an article regarding use of the fasciocutaneous cone flap, Plastic and Reconstructive Surgery (2005;115:1582–1590), also with very good results. Thus, our clinical experience confirms the experimental study of Harry et al.

DOI: 10.1097/PRS.0b013e3181d62cbf

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Reply

Sir:  

With reference to the letter from Dr. Calderon and Dr. Leniz, I am pleased to acknowledge Dr. Calderon’s contribution to this field of work, which has been extensively quoted, and which was included in my original publication as reference 16.

The article entitled “Comparison of the Effect of Bacterial Inoculation in Musculocutaneous and Fasciocutaneous Flaps” published in 1986 addressed a critical clinical problem, using a well-designed, novel canine model. This study enabled the microenvironment at the flap interface to be investigated, and the findings demonstrated that muscle had an increased ability to reduce the bacterial count at the wound surface and enhanced the indices of wound repair.

My coauthors and I attempted to refine the comparison of muscle and fasciocutaneous tissue with
respect to coverage of open tibial fractures. This is a complex reconstructive problem, and our novel murine model investigated healing beneath these soft tissues and, in particular, their vascularity. Further work has been undertaken to investigate soft-tissue angiogenesis, and this work is due to be submitted.

I would like to thank Dr. Calderon and Dr. Leniz for their comments and critique of our work, in particular for the recognition that these experimental findings translate to clinical practice.

DOI: 10.1097/PRS.0b013e3181d516a8

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