Letters and Viewpoints

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**Viewpoints** pertain to issues of general interest, even if they are not related to items previously published (such as unique techniques, brief technology updates, technical notes, and so on). Please note the following criteria for Letters and Viewpoints:

- **Text**—maximum of 500 words (not including references)
- **References**—maximum of five
- **Authors**—no more than five
- **Figures/Tables**—no more than two figures and/or one table

Authors will be listed in the order in which they appear in the submission. Letters and Viewpoints should be submitted electronically via PRS@mkwell, at www.editorialmanager.com/prs/. We strongly encourage authors to submit figures in color for clarity.

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**LETTERS**

**BRODY’S ARTICLE ON “THE PERFECT BREAST”**

Sir:

I want to commend the *Journal* on Dr. Brody’s editorial “The Perfect Breast: Is It Attainable? Does It Exist?” (*Plast. Reconstr. Surg.* 113: 1500, 2004). Open discussion of this topic is long overdue and most welcome. I have found myself under strong criticism at plastic surgery meetings for suggesting that the patient’s desires should be given priority over the surgeon’s preferences. One of my critics even went so far as to suggest that we plastic surgeons should serve as “guardians of the normal” where breasts are concerned, refusing to achieve patients’ objectives if they conflict with our own preferences, a concept with which I emphatically disagree.

It is highly gratifying to read that such a highly esteemed and respected colleague as Dr. Brody believes that we ought to be trying to give the patient what she wants. Many of our colleagues are adamantly opposed to that idea, some on the grounds that we, possessing an artistic temperament and eye, are in the best position to educate the patient about what it is she should desire for herself. Others are opposed to the patient-oriented approach because putting it into practice requires not only that we take the time to learn what our patients want but also that we take even more time to educate our patients about the implications and consequences of giving them what they want. At a recent meeting one colleague told me pointedly that he had no intention of spending that kind of time with patients and that he already knows how to give patients the look he prefers. Another objected to this approach, saying that patients choose a plastic surgeon because they trust that the surgeon will make them look good, and that should take precedence over the patient’s preference.

Although I use the breast and chest dimensions when finalizing decisions, for the past 20 years I also have relied heavily on the water-bag-in-the-brassiere approach, and I too have seen the average implant size rise to the 450 cc noted by Dr. Brody. Most significantly, I have seen a concomitant reduction in the frequency of repeated operations to increase the implant size a year or two later, formerly the most frequent reason for my reoperation on my own patients and still the number one reason for my revisions of other surgeons’ patients. I suspect Dr. Brody’s experience has been the same.

A wake-up call for me was such a revisional patient whose surgeon had refused to put in the very much larger implant that would increase her to the size she wanted. Although I was also uneasy about that size, I proceeded to use the larger implant. Several years later, at one of her routine follow-up visits (at which times I have always cringed slightly because she looks far larger than my personal taste encompasses), she told me that over these years, for the first time in her life, she has loved the way her breasts look. When I have patiently reviewed with her the long-term future drawbacks she will face due to the large size, she has replied, equally patiently, thank you very much, but what is important to her is her appearance in the present.

My hope is that Dr. Brody’s philosophy is being taught to residents, so that they can approach each patient’s desires with some objectivity, giving great weight to her self-image goals. Information is what our patients need and, in this current age, deserve. If after explaining the pros and cons to the patient she still seeks to pursue a course that we as professionals are unwilling to assist her to achieve, our final duty to that patient is to refer her to another surgeon who might be so willing.

I have no doubt that the *Journal* will receive letters critical of Dr. Brody’s philosophy citing lofty aesthetic standards, objecting to yielding to societal pressures, expressing mathematical formulae, and putting forth various other complex objections, both theoretical and substantive. Perhaps there will always be two separate camps with no common ground. If so, I can be counted as firmly in Dr. Brody’s camp, and I look forward to the debate.

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BRODY’S ARTICLE ON “THE PERFECT BREAST”

Sir:

I am writing to comment on portions of the editorial entitled “The Perfect Breast: Is It Attainable? Does It Exist?” by Dr. Garry Brody in the April 15, 2004, issue of the Journal. Let me be sure I’m getting this right: If we can conclude based on subjective generalizations and no data whatever that something doesn’t exist and therefore is not attainable, then certainly surgeons should have no reason to try to advance patient outcomes and the patient experience in breast augmentation. If we can simply resist the temptation to try to quantify parameters to affect and improve decision making in augmentation, relying on the three-decades-old generalizations and rationalizations in this editorial, we can avoid being “impertinent . . . Henry Higgins” (who is he?!) and rest comfortably on our current 20 percent reoperation rates within just 3 years following augmentation1 (a rate that, interestingly, hasn’t changed appreciably over the past 15 or more years2–4). If we can make generalizations and inaccurate statements regarding a surgeon’s peer-reviewed and published information without ever referencing a single article written by the surgeon,5–9 then perhaps the newspaper is on equal footing with the Journal for exchange of professional information.

According to Dr. Brody, “A successful cosmetic operation should be marked by the patient’s satisfaction with his or her own self-image. It is impertinent for us plastic surgeons to play Henry Higgins to our Eliza Doolittle patients by applying our own sense of aesthetics to the final outcome, even if we had the ability to accurately sculpt the final outcome.” I question whether it may also be illogical, incompetent, potentially damaging to patients, and medicolegally indefensible to give patients “what they want” without ensuring that they simultaneously understand “what they are likely to be getting,” especially long term. If it is impertinent for surgeons to be aware and inform patients of potential long-term consequences of their preference of breast size, perhaps responsible surgeons should be impertinent.

If readers are to believe the content of this editorial, surgeons should simply attempt to produce whatever a patient desires according to the patient’s aesthetic tastes, using implants averaging 450 cc! Perhaps readers should ignore the fact that over the past 20 years, some surgeons have actually learned that a brassiere never accurately simulates the stretch characteristics of human tissue, and that three simple measurements can help produce reoperation rates of 3 percent at up to 7 years of follow-up.4–7 Instead of attempting to quantify even three simple, measurable parameters, the author would have surgeons continue to have patients stuff brassieres, assuming that the compliance of their tissues and the potential tissue consequences are acceptable regardless of the implant size the patient chooses. The author states, “I share with the patient the consequences of chosen size as best I can, but the final decision is hers.” What does “carefully” mean? What consequences? The author obviously believes that the consequences of an average 450-cc implant on the average woman’s breasts are acceptable. That belief certainly clarifies the need to “carefully” explain the potential consequences to the patient. Our level of “careful” education and informed consent regarding breast size and potential tissue consequences is well documented in the Journal and online.8

According to the author, “Using the Tebbetts formula, our volume operations are limited by our patient’s preoperative measurements” (what a novel concept). He has declared that he will never insert an implant larger than 350 cc (no reference provided). But if Hsia and Thomson9 are correct, is this not “playing Pygmalion?” Am I missing something here? Are readers to believe that implant size is unrestricted by the amount of available tissue for cover? Is it logical and acceptable to force patients’ tissues to accommodate volumes that will inevitably produce parenchymal atrophy and skin stretch and thinning, sacrificing soft-tissue coverage and potentially creating irreversible and uncorrectable deformities, including stretching and thinning, implant palpability and edge visibility, and traction ripping? Common sense, observation, and data about what happens to the best of naturally larger breasts as a woman ages, and simple logic applied to decision making have nothing to do with playing Pygmalion, but have everything to do with being a breast augmentation surgeon who prioritizes long-term patient outcomes. Good decisions are not arbitrary and subjective. Instead, good implant size decisions are based not only on what the patient may want but also on what her tissues will realistically allow her to have while minimizing potentially negative or uncorrectable consequences. The best decisions rise above subjectivity and are based on quantifiable parameters of each individual patient’s breasts. In patients with wide breasts and marked skin compliance, larger implants are required to achieve optimal aesthetic results. But the larger the implant, the greater the effects on tissues that have already proved they will stretch and thin, and the more important the surgeon’s “careful” explanation to the patient that the result is temporary at best and that negative tissue consequences may very likely occur.

Patient satisfaction can vary widely according to when it is measured. Experienced surgeons have seen numerous patients who were ecstatic about their larger breasts (“just give me what I want!”) until several years later when they presented furious about their ptosis, visible traction ripping, and visible and palpable implant edges (“but carefully avoid telling me what I’m likely to get”).

Since the first breast augmentation, surgeons have been able to guarantee patients a size increase. How far have we come since then? According to the author, “How much control do we really have over the outcome of an augmentation? In truth, I believe, very little . . . The only thing that we can guarantee is a volume increase.” I suppose some readers are happy that they can still deliver what they could guarantee three decades ago. Fortunately for today’s emerging plastic surgeon, other alternatives exist. Instead of relying on subjective generalizations, today’s young surgeons have tools available that can quantify simple parameters that were previously totally subjective.10 Surgeon educators can provide information and a curriculum that are based on more than subjectivity. Today’s surgeon can apply quantified parameters to decision making (more like science than subjectivity) to achieve more predictable results and better long-term patient outcomes.4–8,10 Surgeons can actually compare and assess re-
sults based on quantifiable data instead of age-old platitudes, generalizations, and selected results.

Dr. Brody’s editorial is not really about “the perfect breast” or whether it is attainable. This piece promotes decades’ old subjectivity, generalizations, and rationalizations that have contributed to a 20 percent reoperation rate within 3 years. Decisions based on quantifiable parameters that a surgeon can measure in less than 5 minutes can help reduce reoperation rates to 3 percent with up to 7 years of follow-up. As to whether advancements in breast augmentation (much less the “perfect” breast) are possible, the author has it right with his words in the last sentence of the editorial: “It depends.” Advancement of the patient experience in breast augmentation depends on whether surgeons embrace what has been or what can be.

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REFERENCES
approaches do not allow patients to select their breast implants, this advance does not limit the ability of patients to be fully responsible for ultimately selecting their breast implant size, but rather it allows them to be fully informed and educated about the current state of the art in breast augmentation before they make their decision.

The opinion that quantifiable breast analysis is nonproductive is not only false but will likely produce a multitude of problems in the next 1 to 2 years, when a variety of new shaped devices become available for use. Optimal outcomes with more sophisticated devices require strict, dimensionally based preoperative analysis and planning and meticulous intraoperative technique.

And what about patient recovery, outcomes, and reoperation rates? How many patients would rather return to full normal activities within 24 hours compared with the recovery experienced by the average augmentation patient? Do we direct residents to the peer-reviewed and published tools to deliver these outcomes, or do we ignore significant advances to the patient experience while restating the principles that produced a 20 percent reoperation rate in the latest premarket approval submission?

The future of plastic surgery is in the hands of our young and developing surgeons. Dr. Brody’s expertise and contributions in breast surgery are significant; however, this editorial sends the wrong message, especially to the young plastic surgeon. Advances in breast augmentation have been significant in the past 5 to 10 years, and a dramatically better experience is available for all patients. Delivering improved patient outcomes and a vastly improved patient experience requires forward-thinking and appropriate mentors, open minds, and effort. The last two ingredients are undoubtedly present in today’s young plastic surgeons. The first ingredient depends on the messages we send them and the tools we provide to them.

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BRODY’S ARTICLE ON “THE PERFECT BREAST”

Sir:


It is an interesting choice of words to write that a colleague “has extensively proselyitized the use of chest/breast ratios.” Proselytization is imposing your faith upon another. However, this surgeon has published extensive quantitative analysis of breast augmentation surgery. Dr. Brody uses this word to pejoratively dismiss conclusions that were based on quantified empirical data. The triumph of data over faith is what distinguishes science from all other intellectual endeavors. If we are to even pretend we are scientists, we must avoid using our own beliefs to condemn empirical data.

Dr. Brody argues that since we cannot define the perfect breast, and since we have no meaningful control over breast shape, we should focus only on volume. But even though we might not be able to define the standard perfect breast shape, we can look at an individual patient’s breasts and identify what would make a better breast shape for her. And even if we cannot all agree about what would constitute a perfect or better breast shape, we should agree about unattractive breast shapes (e.g., visible edges, disproportion, and stretch deformities). Avoiding these (often uncorrectable) iatrogenic deformities should be as important as striving for the perfect shape.

I have seen that my operative choices do profoundly affect long-term breast shape (e.g., implant shape, size, fill, and projection; pocket location; parenchymal scoring; and so on), as well as the need for secondary surgery. I have also observed that when tissue characteristics are ignored for the sake of size, unattractive deformities are more likely to occur. My experience is different from that of Dr. Brody. I believe we can improve breast shape (and damage it as well). Dr. Brody is correct that this shaping power is limited, but it is sufficient to allow us to be breast shapers and not just breast stuffers.

He is correct that existing implants give limited control over shape. As an investigator in both the Inamed and Mentor core trials of form-stable cohesive silicone gel-filled implants (the 410 and the CPG), I can report that rather than being subject to deformation by gravity and the forces of the breast, these implants maintain their shape and impart it upon the breast. We are at the dawn of an era in which we will have an unprecedented ability to control shape. That is why Dr. Brody’s editorial is so timely.

Dr. Brody says that we should not be "playing Pygmalion" when it comes to choosing size, but instead allow our patients to pick the volume that they wish to have. I disagree. I feel it is not merely appropriate but ethically mandatory for us to control shape. That is why Dr. Brody’s editorial is so timely. Dr. Brody says that we should not be "playing Pygmalion" when it comes to choosing size, but instead allow our patients to pick the volume that they wish to have. I disagree. I feel it is not merely appropriate but ethically mandatory for us to control shape. That is why Dr. Brody’s editorial is so timely.
patients. However, his comments imply that his results are applicable to all comers. Most of us are willing to treat the complicated problems as well, customizing the procedure to the patient’s individual problems and concerns, many of which do not lend themselves to ideal outcomes or to standardized measurements and ratios.

Drs. Adams and Teitelbaum suggest that the three simple tape measurements introduced by Tebbetts are scientific while a volume measurement is not. Dr. Teitelbaum corrected my English where I used proselytized when a better word would have been popularized. I will return the favor. Where he states that “[Dr. Tebbetts’] conclusions were based on quantified empiric data” I think I know what he means, but I would remind him that Webster’s defines empiric as “relying on practical experience.” I claim no more or less. In fact it takes very complicated math to convert surface area to volume and is impossible from three surface measurements of the uniquely complex surface of an individual breast. I would remind them that while the volume enlarges by the cube of a perfect (emphasis intended) hemisphere, the surface area enlarges only by the square. Calculating the volume or area of the multiple variable contours of different sizes of shaped implants would require a separate formula or direct three-dimensional measurement scans of each one and cannot be calculated from three simple measurements (Fig. 1).

Dr. Adams also referred to “having patients stuff their brassieres with rice bags or water-filled Ziploc bags.” This is a misquote, suggesting that he has not read my article on the subject. The “stuff” in my editorial refers to what the patient uses at home to get the look she wants. That, of course, does not provide any usable volume measurement. To reiterate, I recommend underfilled, soft, “baggie”-like bags of water that can fill every nook and cranny of the brassiere for accuracy. Rice, Ziploc bags, and implant sizers leave too much dead space for precision. The patient is also instructed to purchase a brassiere that has a full cup, with no padding, and that is made of nonstretch material. Of course, even these do not provide complete accuracy, so that the final measurements should be taken by the surgeon with the

FIG. 1. Rate of change of surface area compared with volume with increasing implant size (solid line, surface area; broken line, volume).
patient in front of a mirror. She can then again try different volumes within a range that the brassiere provides with her clothes on before giving final acceptance, thus ensuring accuracy. Adherence to these parameters results in very precise measurements. Fewer than 1 percent of my patients later request a size change. (It is interesting to note that both manufacturers report that reoperation for size change is approximately the same for up and down sizing.)

I am curious as to how Drs. Tebbetts and Adams can “scientifically” evaluate tissue characteristics. I have in the past spent more than 400,000 federal dollars in collaboration with the best materials scientists at Jet Propulsion Laboratories to study the viscoelastic characteristics of in vivo human skin. We found that both skin and scar have more complex elastic characteristics than any known material. These properties vary with posture, age, direction of stretch or compression, time, Langer’s lines, and anatomic location from millimeter to millimeter. This makes analysis challenging, interpretation questionable, and prediction impossible. 

There is a property of viscoelastic materials such as skin and silicone elastomers called thixotropia. This is the phenomenon by which these materials stretch under tension or load and then either reset at a new length if the pressure is relieved or continue to creep if the pressure is maintained. Thus, the shell that is inflated to the “ideal” volume at surgery may actually enlarge in relation to the saline volume and thus feel softer than when inserted and/or present with wrinkles.

Yes, Dr. Adams, added weight may indeed accelerate sag, but who can predict whether the aging implanted breast will end up as a “rock in a sock” from the implant weight and lack of fixation to the chest wall or as a “Snoopy” deformity due to the natural tendency to ptosis of the original breast over a fixed device? Dr. Teitelbaum, only time will tell if this will happen to the cohesive product, but I predict that it will as it certainly did for the foam-covered devices. Should these women not get pregnant or nurse for fear of distorting our plastic surgical masterpieces? Perhaps we shouldn’t offer face lifts to patients because the wrinkles will return with time? Certainly we see young women who are willing to have surgery and resultant scarring to reduce their large breasts, but what we do not see in our offices are the silent majority who accept and even delight in their ample or full hips rather than the slim lines of the classic fashion model.

I am disappointed that the evidence of history seemingly does not impress Drs. Tebbetts and Adams. What will we do if fashion, changes and the breasts of the A-cup pseudoptotic tendency to ptosis of the original breast over a fixed device? Dr. Teitelbaum, please reread my comments on surgical shaping. I never denied you permission to improve the contour, but only stated as you did that our ability to control shape is limited. Also, remember that Eliza Doolittle was unhappy with her transformation until love was added to the equation.

Dr. Adams, I am not concerned about our young plastic surgeons. They are, by virtue of our very specialty, independent thinkers like yourself and Dr. Teitelbaum who, if we train them well, will make up their own minds from their own experiences no matter what we write here today.

Finally, yes, subjectivity and empiricism are promoted in my piece because that is what my editorial was about. There is no quantifiable measure of breast beauty. It is all in the eyes of the beholder (and holder), whose views we must respect.

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REFERENCES


VIEWPOINTS

A SIMPLE DEVICE FOR CLEANING THE HAIR AFTER SCALP SURGERY

Sir:

The scalp is an extremely well-vascularized structure. Both surgery and trauma can cause severe bleeding. Although this bleeding can be arrested intraoperatively, the clotted blood remains in the hair, matting it together. This results in unsightly dried blood in the hair, which can cause irritation to the patient almost never seen. Only when the polyurethane and the textured devices became popular did ripples surface as a concern.

We live in a multicultural, multiethnic society in which there is no single standard of beauty. Tattoos, fluorescent hair, shaved heads, studs in every bodily projection and orifice, and scarification are the norm in some subcultures. More implants are sold in coastal America than in the Midwest. Many African Americans, at least in Los Angeles, prefer full hips rather than the slim lines of the classic fashion model. We cannot put our patients in the single mold of our personal mind’s eye, but must be conscious and understanding of their own goals and desires. We must also, of course, evaluate as best we can the maturity of the woman’s decision-making process to maximize the quality of her outcome.

Dr. Teitelbaum, please reread my comments on surgical shaping. I never denied you permission to improve the contour, but only stated as you did that our ability to control shape is limited. Also, remember that Eliza Doolittle was unhappy with her transformation until love was added to the equation.

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and difficulties to the nursing staff in the dressing clinic when the time comes to remove sutures or tie-overs.

Sterile combs are rarely, if ever, available in the operating theater. We recommend using a hand-scrubbing brush for cleaning the hair before or after surgery, as required (Fig. 1). This brush is readily available in operating theaters, comes sterilized, and is an inexpensive substitute for the sterilized comb. These brushes are offered plain or impregnated with Hibiscrub (Astra Zeneca, London, England) or Betadine (Seton Scholl, London, England) scrub solutions, which produce lather and can be used with or without dilute hydrogen peroxide solution to gently comb blood and clots out of the hair. The soft nature of the bristles also avoids injury to the scalp and the wound. DOI: 10.1097/01.PRS.0000157506.59630.29

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TABLE I
Frequency of Cleft Palate and Resorption in Mice

<table>
<thead>
<tr>
<th>Group/Mouse Strain</th>
<th>Dexamethasone</th>
<th>No. of Dams</th>
<th>Cleft Palate/Live Fetuses</th>
<th>Resorption/Implantation</th>
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<tr>
<td></td>
<td></td>
<td>No.</td>
<td>%</td>
<td>No.</td>
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<tr>
<td>Blind</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>ddY</td>
<td>+</td>
<td>11</td>
<td>106/110</td>
<td>96*</td>
</tr>
<tr>
<td>C57BL/6j</td>
<td>+</td>
<td>14</td>
<td>19/97</td>
<td>20*</td>
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</tr>
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* p < 0.0001.

Fig. 1. Appearance of scalp after “brushing.”

Sir:

Differences in strains have been shown to affect the rate of glucocorticoid-induced cleft palate in the mouse. Although fetal genetic factors are pointed to as the causes for these differences, some reports have also suggested that maternal factors may be involved. The previous study used normally mating mice, so fetal genetic factors and maternal factors were mixed, making it difficult to examine maternal factors among the different strains. At the twenty-sixth meeting of the Japanese Society of Cleft Palate, we reported that genetic factors and maternal factors could be examined separately. There is a chance for cleft palate to occur based on the load of fertilized ova transferred. In this study, we added a glucocorticoid-untreated group to our previous study and examined the results. We used ddY mice as donors of fertilized ova because this strain has a high rate of glucocorticoid-induced cleft palate. For recipients, we used ddY mice and C57BL/6j mice, which have a lower rate of glucocorticoid-induced cleft palate. For blind groups, we used normally mating ddY mice and C57BL/6j mice. For control groups, we used glucocorticoid-untreated groups. The glucocorticoid drug used was dexamethasone. The fertilized ovum transfer procedure was the same as that reported by Tsunoda,1 Ueno,2 and Kuno et al.3 Dexamethasone was injected on gestation day 11 of the pregnancy. Each group was observed on gestation day 18 of the pregnancy and analyzed for the occurrence of cleft palate. The data were analyzed by Fisher’s exact test (Fig. 1).

In blind groups, the occurrence rate of cleft palate was 96 percent in ddY mice and 20 percent in C57BL/6j mice. In control groups, the occurrence rate was 0 percent in both groups. In experimental groups, the occurrence rate in the ddY mice with fertilized ova transferred from the same strain was 95 percent, and in the C57BL/6j mice with fertilized ova from ddY mice, the rate was 29 percent (Table I).

The occurrence rate of dexamethasone-induced cleft palate between ddY and C57BL/6j mice was found to be significantly different in blind groups. Transferred fertilized ova were used in all ddY mice in the control groups and the
experimental groups. The placenta was of ddY mice origin, and the dexamethasone-passing ability was considered to be of the same degree. Cleft palate in the experimental groups was presumed to have been induced by dexamethasone because cleft palate did not occur in the control groups. The occurrence rate of dexamethasone-induced cleft palate was found to be significantly different in the experimental group, despite the same fertilized ova being transferred. Glucocorticoid-induced cleft palate occurred because of the difference in the metabolic ability of the mother’s body and was considered to be strongly influenced by maternal factors. DOI: 10.1097/01.PRS.0000157507.38210.F2
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AMBULATORY CLEFT LIP AND PALATE SURGERY BY TUMESCENT TECHNIQUE

Sir:

We have used the tumescent technique to minimize intraoperative blood loss in our patients with cleft lip and palate deformity, operating on and discharging patients in 1 day. This is a report of our experience using this technique in 58 consecutive patients.

Fifty-eight patients with cleft lip and palate were operated on using the tumescent technique over a period of 1 year, from January of 2003 to December of 2003. Among these patients, 33 were operated on for cleft lip, 14 for cleft palate, and four for secondary palatal fistula. In seven patients over the age 2 years, combined lip and palate repair was performed as a single-stage procedure. Most of the patients in the study group were from a lower socioeconomic group; their average hemoglobin level was 10.8 g percent (range, 8 to 13.8 g percent). We did not operate on patients with a hemoglobin level of less than 8 g percent. The age of the patients ranged from 10 weeks to 20 years (mean age, 5.1 years).

Tumescent solution was prepared by adding 30 ml of 2% injectible lidocaine, 1 ml of 1:1000 injectible adrenalin, and 1 ml of 8.4% injectible sodium bicarbonate to 500 ml of normal saline. After markings were made, the tumescent solution was infiltrated into the operative site until blanching occurred. An average amount of 10.8 ml of solution was infiltrated in lip repair, 15.6 ml in palate repair, and 28.6 ml in combined lip and palate repair. Heart rate, cardiac rhythm, and oxygen saturation were monitored and recorded preop-

FIG. 1. A patient with cleft lip. (Above) Preoperative view. (Center) Intraoperative view after infiltration of 9 ml of tumescent solution. (Below) Postoperative view after 6 months.
therapeutically, at the time of induction, and at the time of injection and then every minute for 5 minutes, every 2 minutes for the next 10 minutes, every 5 minutes until the completion of surgery, and then every 15 minutes for 3 hours postoperatively. Sevoflurane was used as inhalation agent, as it is more cardiostable with less chance of arrhythmia (Figs. 1 and 2).

Intraoperative blood loss was measured by the difference in the weight of soaked and unsoaked gauze pieces used during the procedure. A suction apparatus was not used during the surgery.

We used the tumescent technique for cleft lip and palate repair and found several advantages of this technique. The perioperative tissue dissection was much easier and faster, reducing operative time significantly. The average blood loss was 5.6 ml in cleft lip repair, 13.2 ml in palate repair, and 23.6 ml in combined lip and palate repair (Table I). These blood loss values are significantly lower than those reported in the literature (54 ml in cleft lip repair and 112 ml in cleft palate repair). Because of the reduced perioperative bleeding, we were able to repair the lip and palate in seven older patients as a single-stage procedure. These patients did not require blood transfusion postoperatively either.

Postoperatively, there was no bleeding in any case and the patients were more comfortable and pain-free. Parenteral analgesic drugs were not required in any patient in the postoperative period, and patients were able to tolerate oral feedings earlier and better. For these reasons, we were able to send these patients home on the same day or within 24 hours of surgery. Suture lines healed uneventfully in all patients, and there was no wound dehiscence, prolonged healing, or unexpected scarring in any patient.

We conclude that the tumescent technique for cleft lip and palate repair keeps the patient safe, comfortable, and pain-free during and after surgery without any essential disadvantage. This technique has made 1-day cleft lip and palate surgery possible.

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**TABLE I**

<table>
<thead>
<tr>
<th>Procedure</th>
<th>No. of Patients</th>
<th>Average Age (yrs)</th>
<th>Average Hemoglobin Level (g %)</th>
<th>Average Volume Infiltrated (ml)</th>
<th>Average Blood Loss (ml)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lip repair</td>
<td>33</td>
<td>3.9</td>
<td>10.6</td>
<td>10.8</td>
<td>5.6</td>
</tr>
<tr>
<td>Palate repair</td>
<td>18</td>
<td>5.9</td>
<td>11.1</td>
<td>15.6</td>
<td>13.2</td>
</tr>
<tr>
<td>Lip and palate repair</td>
<td>7</td>
<td>8.1</td>
<td>11.4</td>
<td>28.6</td>
<td>25.6</td>
</tr>
</tbody>
</table>
CLEFT PALATE AND CONGENITAL ALVEOLAR SYNECHIAE SYNDROME

Sir:

An 8-month-old male infant was referred to our plastic surgery department because of difficulty in feeding and restricted mouth opening. The infant had had a normal vaginal delivery at 40 weeks’ gestation and weighed about 3 kg at birth; he had an Apgar score of 9 at 1 and 5 minutes. The parents’ medical histories were noncontributory, and they were blood relatives. The couple’s first child had no medical problems. The mother was rubella immune, gonorrhea/chlamydia negative, and syphilis-screen nonreactive.

A physical examination revealed incomplete cleft palate with bands of tissue joining the upper and lower jaws bilaterally and restricting mouth opening to approximately 10 mm. The fibrous bands spanned symmetrically from the posterior alveolar ridge of the maxilla to the posterior alveolar ridge of the mandible bilaterally (Fig. 1). Results of chromosome studies and radiographs of the mandible, cervical region, and chest were all within normal limits.

The infant was operated on under general anesthesia for division of the lateral synechiae. The bands were released with small iris scissors, and the tissues were then touched with an electrocautery device (Fig. 2). This released the mandible immediately; however, some restriction of temporomandibular joint movement persisted. We recommended physical therapy and exercises to be performed during the follow-up period until the patient could undergo surgery for the cleft palate. On subsequent follow-up, restriction of temporomandibular joint movement had resolved, and at 1 year of age the patient had correction of the cleft palate. The patient is growing and maturing as expected with no complications (Fig. 3).

There have been more than 50 reports of congenital intraoral synechiae, with interalveolar synechiae representing less than 30 percent of cases. A review of the literature demonstrates that most cases involved additional anomalies, such as cleft lip with other facial or extremity malformations. The isolated cleft palate with interalveolar synechiae has been described in the literature in only seven patients. Verdi and O’Neal referred to this condition as “cleft palate and congenital alveolar synechiae syndrome”; their case involved a male infant. Mathis also described a male patient with synechiae and cleft palate. Fish2 described two male patients with congenital interalveolar bands, but only one had an isolated cleft palate. Haramis
and Apesos\(^3\) described a female infant with cleft palate and interalveolar synechiae, and Dalal and Davison\(^7\) described two siblings with interalveolar synechiae and cleft plate. Lastly, Murphy et al.\(^5\) described a 1-year-old girl with cleft palate and intra-alveolar synechiae. Thus, only nine cases of lateral interalveolar synechiae with isolated cleft palate have been reported, including our present study.

The cause of congenital alveolar synechiae is still unknown. Many theories have been offered. All the reported cases of alveolar synechiae involve cleft palate with or without other facial anomalies. Thus, there may be a relationship between synechiae and cleft palate. It has been suggested that the synechiae cause restriction of the movement of the maxilla and mandible and results in the interposition of the tongue and formation of a cleft palate.\(^5\) This theory is supported by the report of palatomandibular and maxilomandibular fusion with partial aglossia.\(^8\) Also, local ischemia and amniotic bands causing pressure on the first branchial arch, persistence of the buc- copharyngeal membrane, hypervitaminosis A, and some drugs have been postulated as etiological factors.\(^3,7-9\)

Other syndromes associated with congenital synechiae and cleft palate include van der Woude’s syndrome, popliteal pterygium syndrome, orofaciodigital syndrome, and oromandibular limb hypogenesis syndrome.\(^10\) Popliteal pterygium syndrome includes popliteal pterygia, cleft palate with or without cleft lip, lower lip sinuses, syndactyly of toes, and genital anomalies.\(^9,10\) Van der Woude’s syndrome seems to be a variant of popliteal pterygium syndrome with a genetic etiological relationship, and it is rarely associated with maxilomandibular synechiae.\(^15\) Orofaciodigital syndrome is also rare and includes cleft lip and cleft palate, multilobulated tongue, multiple buccal frenae, and digital and neural anomalies.\(^14\)

The fibrous interalveolar bands are treated by surgical excision, but the timing of the surgery depends on the clinical situation. If the neonate experiences breathing difficulties, immediate release of the bands must be performed in the nursery. In patients with no breathing or feeding problems, the procedure can be postponed some weeks so the neonate can grow and gain strength before surgery. As in our case, after 8 months had elapsed, mild temporomandibular joint ankylosis had occurred and resolved after release of the alveolar bands and a short period of physical therapy.

**REFERENCES**


**MEDIAN PALATINE CYST: REPORT OF AN UNUSUAL ENTITY**

Sir:

The median palatine cyst is a rare fissural cyst of nonodontogenic origin located in the midline of the hard
palate, posterior to the palatine papilla.\textsuperscript{1,2} To this date, only 19 cases have been reported.\textsuperscript{3,4} The duration of the lesions ranged from a few days to 5 years; the majority of the cases had been noticed for several months. Although the lesions are benign, they must be differentiated from other maxillary masses.\textsuperscript{5,6} Several authors have pointed out that nasopalatine and other anterior cysts usually show the presence of mucous glands histologically, as well as large vascular spaces, nerve trunks, and occasionally hyaline cartilage.\textsuperscript{1,7,8} These structures have not been observed in median palatine cysts.

A 23-year-old man was referred to our clinic for evaluation and treatment of an asymptomatic palatal swelling distal to the palatine papilla that had been noticed for 1 year (Fig. 1). On examination, the lesion was fluctuant and painless. There were no carious teeth in the area and no divergence of the incisors. All teeth were vital.

A computed tomography scan showed a large, well-circumscribed opacity extending from just distal of the anterior maxillary ridge laterally and posteriorly to a point adjacent to the first molar region; it measured approximately $2 \times 2$ cm. A clinical diagnosis of fissural cyst was made. The lesion was visualized by reflecting the palatal mucosa and was carefully and totally enucleated. No communication with the incisive canal was discovered.

Microscopic examination of the surgical specimen revealed a thick cyst wall composed of dense collagenous tissue with uniformly spaced fibroblasts and a moderate degree of vascularization. Interspersed throughout were considerable numbers of plasma cells and lymphocytes. The lesion was lined equally by both stratified squamous epithelium and pseudostratified ciliated columnar or respiratory epithelium with goblet cells. There was no evidence of any major vascular channels, nerve tissue, mucous glandular structures, or hyaline cartilage.

Median palatine cyst is a rare, nonodontogenic lesion of the hard palate that does not involve the palatine papilla or incisive canal and is often discovered on routine clinical or radiographic examinations. It develops as a circular or ovoid lesion, producing an asymptomatic, fluctuant, symmetrical swelling that may extend as far distally as the molar region.\textsuperscript{9,10} Its etiology is generally attributed to the enclavement of remnants of epithelium surrounding the two lateral maxillary processes that fuse to form the hard palate.\textsuperscript{11,12} The lesions are composed histologically of a fibrous collagenous tissue wall, with infiltration of chronic inflammatory cells, and are lined with stratified squamous and/or respiratory epithelium. They are differentiated from the nasopalatine and other anterior maxillary cysts by the following criteria: (1) they appear grossly to be symmetrical along the midline of the hard palate; (2) they are located posterior to palatine papilla; (3) they are radiographically ovoid or circular in appearance; (4) they are not intimately associated with a nonvital tooth or found to have any communication with the incisive canal; and (5) they show no histologic evidence of nerve trunks, large vascular spaces, hyaline cartilage, or accessory salivary gland tissue in the cyst wall.

Fig. 1. Preoperative view of the lesion.
A 60-year-old patient with left breast cancer was scheduled for a left total mastectomy and pedicle TRAM reconstruction. The patient was counseled preoperatively that staged reduction mammaplasty of the opposite breast to achieve symmetry would be necessary. The patient’s history included a hysterectomy through a lower midline incision and a hernia repair. It was not stated preoperatively that the hernia repair had been performed laparoscopically. During the operation, upon dissection of the rectus from the posterior sheath, 15 spiral tacking coils were encountered embedded into the rectus muscle. Many of these coils were located above the level of the umbilicus, potentially putting the viability of the flap at risk. For this reason, the flap was delayed and not transferred to the breast at the time. At 1 week, the nonviable areas across the midline and a small area ipsilaterally had demarcated. The nonviable areas were débrided and the flap was transferred. Postoperatively, the flap remained viable. The donor site healed primarily.

Laparoscopic ventral hernia repairs are frequently performed by general surgeons. The tacking coils penetrate the rectus muscle and can affect the pedicle TRAM reconstruction. At a minimum it makes the dissection more difficult and may put the flap at risk for failure. Depending on the number of coils and their location, it may be necessary to delay the flap or convert to a free tissue transfer. Therefore, it is vital to determine the exact type of hernia repair that was performed before operating on patients considering TRAM breast reconstruction.

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THE POSTERIOR INTEROSSEOUS NERVE AS A DONOR SOURCE FOR NERVE GRAFTING: A CONCEPT VISITED

Sir:

Over the past 8 years, when faced with the need for a smaller length of donor nerve graft, we have often turned to the posterior interosseous nerve. This has been particularly true for digital nerve repair. This nerve graft affords minimal donor-site morbidity, because it functions at the wrist level as an intra-articular sensory branch. The nerve is of satisfactory size match for digital nerve replacement distal to the proximal phalanx, and the location of the operative field for both donor and recipient is limited and easily accessible.

Our experience with this nerve was derived from the ablative procedures performed on patients with chronic and recalcitrant wrist pain. The loss of this nerve does not seem to cause any significant morbidity or dysesthesia, and to date we have seen no evidence of neuroma formation at the donor location.

This experience is in contradistinction to loss of the sural nerve. While usually not disabling, the resultant loss of sensation to the lateral aspect of the foot has been a bit problematic for several of our patients, and the requisite dissection requires a second surgical field with some positional constraint limitations. Similarly, with the antebrachial cutaneous nerves, the volar cutaneous forearm distribution is made insensate by their removal. Nerve repair conduits, while certainly attractive in many settings, are often not well suited to the repair of mid- to distal digital repairs as a result of their intrinsic physical dimensional constraints.

Dissection of the dorsal interosseous nerve is quite easy. The hand, fingers, wrist, and forearm are all sterilely prepared and draped into the surgical field. A sterile tourniquet is used. An axially directed incision is marked approximately two fingerbreadths proximal to the dorsal wrist crease. Should more exposure be required, we gently curve the incision distally, though in our experience this is rarely required. We try never to cross into or beyond the wrist crease itself. The total length of the incision is primarily dependent on the length of nerve graft required for the planned reconstruction. Usually only an inch (or at most 2 inches) of total incisional length is required, as longer segments of nerve graft are easily visualized andatraumatically accessed through this approach. The tendinous portion of the extensor pollicis longus is easily identified and gently retracted radially. The nerve is identified just ulnar to the extensor pollicis longus at the level of the interosseous membrane, and there is no need to sever the extensor retinaculum. Proximally, the nerve trunk is a bit thicker and we usually circumscribe it here with a vessel loop before it divides into several fine branches at the level of the wrist. For most segmental digital applications, we tend to take the nerve as it enters the wrist carpus. The maximum available length is about 6 to 7 cm.1 The dissected proximal end is maintained well invested within the deeper soft tissues of the forearm. The donor site is closed in a tension-free, layered anatomic fashion. We do not specifically splint the wrist per se, though often it is incorporated for control of the digital nerve repair.

As is often the case in surgery, so many ideas that we at first believe to be novel turn out to be nothing new; we have identified a nice description of this procedure by the notable Dellon and Seif.2 In an article published in the Journal of Hand Surgery in 1978, the location of the nerve and the surgical technique are nicely described. There have been other articles describing the detailed anatomy of the nerve and its branches, as well as fiber and nerve thickness studies.3-4 This fact notwithstanding, in a poll of our local hand surgeons as well as plastic and reconstructive surgeons, this particular source for nerve grafts had either fallen out of favor or been forgotten.

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A VERY SEVERE HAND PATHOLOGY WITH A POSSIBLE DIAGNOSIS OF ONYCHOGYPOSIS

Sir:

Various types of congenital and acquired conditions of the hand are encountered in the daily practice of plastic surgery. In fact, some of these pathologies are too complex and extreme for a certain diagnosis to be made at the very early stage. Furthermore, it may become necessary to plan and undertake a surgical procedure for symptomatic relief and functional gain for this kind of severe lesion. This can usually occur in a medical aid organization carried out under very limited circumstances. In this brief correspondence, we report a very severe and massive type of hand pathology with a possible diagnosis of onychogyposis that we saw and operated on at a regional aid organization clinic in northern Turkey.

A 45-year-old woman was brought to the outpatient clinic from a very remote forest village by local health authorities. She presented with severe mental retardation and limited verbal communication, so the required history was only partially obtained. We were told that she had been constantly kept away from social interaction and isolated in a cottage from the well-known risks of the surrounding natural environment.

Briefly, the patient had gigantic, grotesque nail-like structures of different sizes and shapes located distally on both hands and fingers. These structures reached a maximum length of 12 cm and width of 3 cm (Fig. 1). These distal elongations from the fingers were mostly twisted in shape and all were very malodorous. They were extremely solid and had various colors, changing mainly from black to yellow (Fig. 2). Lesions extended proximally up to the level of metacarpophalangeal joints on some of the fingers, and the web spaces were obliterated with debris and lesions. Different lesions located on the dorsum of the hands and fingers and in some of the web spaces showed features of severe parakeratosis or hyperkeratosis. There were additional skin lesions that looked like cutaneous horns on the wrists (Fig. 3).

The metacarpal bones were significantly deviated to the ulnar side, with limited flexion and extension. Her wrist...
bones were grossly deformed and her joints were rather stiff. Her fingers presented various degrees of contracture of flexion or extension. Flexion function at either the proximal or distal interphalangeal level was absent or very limited for most of the fingers. Proximal interphalangeal joints were predominantly hyperextended, while some of the distal interphalangeal joints were in flexion contracture. She was unable to use her fingers and hands properly to meet her daily needs.

No sensory loss was recorded and circulation was not compromised in the distal portion of the hands. No other lesions were observed on the patient’s trunk or lower extremities, except for a typical cutaneous horn with a height of 2 cm localized on her right lower eyelid. Her skin was fairly dry, scaly, and erythematous, especially on her hands. Moreover, she had chelitis and dry oral mucosa.

The team members could not agree on a firm diagnosis, but we all unanimously shared the same opinion of total excision of these structures just proximal to their originating site as a symptomatic surgical treatment. These structures had to be amputated mostly at the distal interphalangeal joint and a few at the proximal interphalangeal joint levels, where they had totally destroyed the underlying structures. Amputation stumps were closed primarily. Hyperkeratotic lesions located on the dorsum of the web spaces or hands were totally excised or shaved. No specific finding at the surrounding tissues was noted during the surgical procedures.

Since we worked under very limited conditions, we were not able to take a sample with us for a histopathological diagnosis or any further diagnostic study, such as radiological evaluation of the involved bones. We left the region a couple of days after the operation, so we had to rely on the online report of the rural medical authorities, who noted that no significant postoperative complications were seen and that the wounds had healed uneventfully. However, we have failed to keep in touch with the patient despite all efforts and lost her to follow-up after several months.

In conclusion, we have suggested that the main pathology was markedly thickened and distorted keratinized nail tissue, known as onychogryposis. It is a rarely seen and reported nail pathology referring to claw nails.1–3 It may arise from trauma or peripheral vascular disease. More importantly, it can be seen after neglected nail cuts, which we assumed was the underlying etiology in our case. Her aggressive behavior patterns and invariably negative responses seemed to have prevented any appropriate body care, which led to this end result.

Onychogryposis is most commonly seen on the great toes of elderly patients, and fungal infection is often present. Patients may also have some keratinization disorders. Hard and rounded nails are consistently deviated laterally, and keratotic debris fills the web spaces.

Only a few treatment methods have been suggested for onychogryposis, including cutting the nails totally and destroying the nail matrix. Mechanical drilling and burring have also been suggested.1 In our particular case, at least asymptomatic relief was provided with the amputations under the above-mentioned circumstances.

In this brief communication, we present a severe type of hand pathology with a possible diagnosis of onychogryposis that has been rarely reported in the literature. To our knowledge, no other similarly severe case has ever been noted or described.

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REFERENCES

REPAIR OF FINGERTIP AMPUTATIONS WITH LOCAL DIGITAL FLAPS AND PERIONYCHIAL COMPOSITE GRAFTS FROM THE AMPUTATED PART

Sir:
I recently received a gracious communication from Professor Rossello in Savona, Italy. He sent me reprints of two publications. The first was coauthored by his predecessor, Professor Renzo Mantero.1 It seems that Mantero and Bertolotti published a method very similar to the one Dr. Meade and I reported in Plastic and Reconstructive Surgery2 for fingertip amputations in which the volar pulp was reconstructed with a cross-finger flap and the perionychial tissues were restored with a composite graft from the amputated part. The second reprint Prof. Rossello sent me concerned a 20-year follow-up experience using that so-called “Mantero technique” for fingertip amputation reconstruction.3 While we did not use a cross-finger flap exclusively, the principles of the reconstruction as we described are essentially the same as those published by Mantero back in 1973, namely, flap reconstruction to restore the bulk of the volar terminal pulp pad of the finger and then using those perionychial parts from the amputated piece, which are very difficult to otherwise reconstruct as a composite onlay graft. Interestingly, after our publication, yet another similar study was published espousing the same surgical techniques.4

It is encouraging to know that this technique has been reported independently by at least three different centers, thus attesting to its utility and success. However, on the flip side, none of the later publications recognized the earlier contributions. Prof. Mantero should be acknowledged, and I hope that Prof. Rossello will be given the opportunity to respond to my communication on Prof. Mantero’s behalf.

Electronic journals will undoubtedly make publications more

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universally accessible and so such unintentional omissions will be avoided. Brief translations, with key words, if they accompany a foreign language text, would also help. While confirmatory reports validate previous publications, one should obviously give appropriate credit and avoid duplication whenever possible.

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REFERENCES

REPLY

Sir:

I want to thank Dr. Netscher for his acknowledgment of the long-standing experience of my former chief, Renzo Mantero, and of myself on the technique described by Dr. Netscher and Dr. Meade in “Reconstruction of Fingertip Amputations with Full-Thickness Perionychial Grafts from the Retained Part and Local Flaps,” published in Plastic and Reconstructive Surgery (104: 1705, 1999). Dr. Netscher’s behavior is a rare and beautiful example of professional and scientific morality which has to be absolutely underscored. I am really happy that this technique, which we have used since 1972 (reported in Rivista Italiana di Chirurgia della Mano 11: 78, 1973/1974), has been appreciated and, in some ways, “re-discovered” in the United States. I personally performed a long-term evaluation of the results of the procedure (published in Rivista Italiana di Chirurgia della Mano 28: 109, 1991) that demonstrated the effectiveness and reliability of the technique, particularly in fingertip reimplantation. The technique is always in use in our hand surgery center in Savona and still gives high rates of success. Please note that our center is one of the eight hand surgery units in Italy recognized for upper limb major traumas and reimplantation. We perform about 350 emergency interventions per year, of which about 30 are reimplantations (forearm, hand, fingers, and fingertips).

I think you can add these comments to Dr. Netscher’s letter in any future issue. Unfortunately, our “Rivista,” which is one of the oldest in the world dedicated to hand surgery, is not yet listed in Index Medicus, but we are trying to have it added, hopefully in 2005. Thank you again for your interest. Please do not hesitate to contact me if necessary.

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VASCULAR LEIOMYOMA OF THE HAND

Sir:

Vascular leiomyomas, first described by Virchow in 1854, are benign, solitary, smooth muscle tumors that can arise anywhere in the body.1,2 They originate from the tunica media layer of vein walls3 and are uncommon in the hand. Vascular leiomyoma has been described as a painful nodule found more frequently on the extensor and volar surfaces.

Fig. 1. Intraoperative appearance.

Fig. 2. Histopathological appearance (hematoxylin and eosin stain; original magnification ×400).
of the hand. The rarity of the condition may, in fact, result from incorrect diagnoses. The differential diagnosis of vascular leiomyoma of the hand is difficult. Diagnosis is usually made after excision and a histopathologic study of the tumor. Pain and tenderness are the only clinical characteristics suggesting this diagnosis. If resection is complete, the chance of recurrence is minimal and the prognosis is excellent.

We report a 29-year-old man who presented with a painful, slow-growing mass on his left palm. On physical examination, the mass was firm and there was serious limitation of the range of movement of the second and third fingers. The tumor was excised under axillary block anesthesia. It was a well-encapsulated mass settled between the second and third metacarpal bones and the lumbrical muscles (Fig. 1). The mass was excised and a histopathological examination revealed vascular leiomyoma arising from the vascular smooth muscle. There were no signs of malignancy (Fig. 2).

We think vascular leiomyomas are among the differential diagnoses that should be considered in hand tumors.

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REFERENCES

A TENDON APPROXIMATOR

Sir:

Tendon repair is a common procedure in hand surgery. It is often made difficult when the proximal end of the severed tendon is retracted. As a result of this, it is sometimes difficult to keep the severed ends of the tendons in close proximity during repair. We outline a technique whereby a commonly used surgical instrument can be easily modified to allow dynamic approximation of the ends of tendons, to provide both alignment and tension-free repair of tendons in the hand and wrist. Various surgical instruments are used as self-retaining retractors to provide exposure during surgical procedures. One of them is the Alm’s retractor. This device has a screw and lever mechanism whereby the edges of the wound are retracted. The mechanism utilized by this device may be exploited for dynamic tendon approximation during repair.

The tendon approximator is devised using an Alm’s retractor, two 1-ml syringes, two hypodermic needles, and sterile tape (Fig. 1). With the paws of the self-retaining Alm’s retractor facing upward, a 1-ml syringe is attached to each limb of the retractor with sterile adhesive tape. Once the retracted ends of the tendons have been delivered, hypodermic needles are inserted into the tendons in a horizontal plane and at a right angle to the long axis of the tendon at a suitable distance from the ends of the tendon, to facilitate repair. The needles are then attached to the tips of the syringes (that have been taped to the limbs of the retractor).
The ends are then approximated by adjusting the tension using the screw device of the retractor. Tension-free repair of the tendon can then be carried out (Figs. 2 and 3).

The art of tendon repair is facilitated by approximation (to relieve tension during repair) and precise orientation of the ends. Hypodermic needles are usually used for this purpose, but this approach is associated with difficulties because the needles tend to slip and have to be repositioned several times during the repair. This not only hampers the accuracy of the repair but also places the surgeons at risk of accidentally needlestick injury.

Recently, Majumder et al. described a technique of temporarily suturing the ends of the tendon to the tendon sheath to facilitate a tension-free repair. The ends of the tendon are therefore in a static state during repair. Our technique is a dynamic one, allowing the tension between the ends of the tendon to be adjusted during the repair process if necessary.

There have been a few reports of tendon approximators in the literature. Some of these instruments are either too bulky to be used in areas where space is limited (e.g., flexor zones 1 and 2), expensive, or more difficult to use.

The approximator described here involves a simple modification of readily available instruments that can easily be assembled in the operating theater. It is a relatively small, safe, and easy-to-use instrument that allows tension to be adjusted during tendon repair.

We are grateful to A. J. Howcroft, G. Ross, and G. Vuppulaptti for their support during this work.

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FIG. 1. Palmar defect after severe burns. The defect is covered with loosely attached epithelium.

FIG. 3. The ends of the tendon approximated to facilitate a tension-free repair.

MANAGEMENT OF DEEP PALMAR BURNS WITH REVERSE PREFABRICATED RADIAL FASCIAL FLAPS

Sir:

Management of deep palmar burns presents a lot of problems. All such burns, as with burns anywhere on the body, need resurfacing by skin graft. However, in a patient whose palmar fascia is also damaged, resurfacing has to be done with a flap.

Various flaps are available, from local flaps to distant flaps. All such flaps have two disadvantages: (1) they are very bulky, which can compromise functioning of the hand, and (2) the skin in all such flaps is not as firmly attached to the subcutaneous tissue as to the palmar fascia, the result of which is improper grasp. We have successfully used the prefabricated radial fascial flap in deep palmar burns in two patients (Fig. 1).

The operation is performed in two stages. In the first stage, the fascia of the forearm is exposed as previously described. The graft is placed over the fascia. After 2 weeks, the grafted fascia (now a prefabricated radial fascial flap) is raised and placed over the radial artery (Fig. 2). The granulated area and any area of unhealthy epithelium are excised and the flap is reversed to resurface the defect.

The advantages of our procedure are as follows: there is no donor-site effect; the flap well settled (Fig. 3); and there is no bobbling of the flap when objects are grasped, because the graft is adhered to the fascia. Though our study is very small, the reverse prefabricated radial fascial flap is a good alternative flap for resurfacing any palmar aspect defects.

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SUCCESSFUL SPLIT-THICKNESS SKIN GRAFTING IN A CONTAMINATED WOUND WITH AN ENTEROCUTANEOUS FISTULA

Sir:

Split-thickness skin grafting is an indispensable tool frequently used by the plastic surgeon to cover skin defects. The graft can be easily obtained with little donor-site morbidity. One limiting factor is the suitability of the recipient tissue bed for grafting. A bed that is continually contaminated by the contents of an enterocutaneous fistula can hardly be considered ideal. Enterocutaneous fistulas associated with large abdominal wall defects are associated with high rates of morbidity and even mortality. Some innovative strategies to deal with enterocutaneous fistulas with large abdominal wall defects include extraperitoneal repair and split-thickness skin grafting. We report a case of successful split-thickness skin grafting in a patient with a contaminated abdominal wound with an enterocutaneous fistula.

Our patient was a 35-year-old male motorcyclist who had been involved in a traffic accident. He sustained a degloving injury of his left abdominal wall involving the subcutaneous layer and the external oblique muscles. The defect amounted to 5 percent of his total body surface area. The defect was debrided and dressed. His other injuries were jejunal perforations and a tear in the mesocolon at the splenic flexure, which needed segmental resection with primary anastomosis.

On the ninth postoperative day, feculent discharge was noted from the lateral part of the abdominal wall wound. A diagnosis of colonic fistula was made, but barium studies to

FIG. 1. Wound inspection on the sixth postoperative day. Take of grafted skin was 100 percent. Forceps point to the fistulous opening.

FIG. 2. Wound dressings after inspection. The central portion is dressed separately to allow for a daily dressing change.

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delineate the fistulous tract were unsuccessful. The patient was then referred to us for wound management. The wound was exudative and contaminated, requiring twice-daily dressing changes. *Acinetobacter baumannii* and *Pseudomonas aeruginosa* were cultured from the wound. The wound was dressed for 2 weeks until it became less exudative and the areas surrounding the fistula were covered with healthy granulation tissue. Appropriate antibiotics were started. A decision was made to cover the wound around the fistula with a split-thickness skin graft to help reduce protein and fluid loss and hence minimize the deficits to the patient’s nutritional status. Nursing needs for wound care would also be reduced.

We cleaned the exposed wound thoroughly with Hibiscrub (chlorhexidine gluconate; Astra Zeneca, London, England) and chlorhexidine solution. We then temporarily occluded the fistula opening using a small sponge, adhering using 2 ml of Tisseel (Baxter fibrin sealant). This was done to prevent fecal soiling of the prepared wound. The rolled-in peripheral wound edges were trimmed and tagged to the wound bed. A split-thickness skin graft was harvested from the right anterior thigh and meshed at a ratio of 1:1.5. This was inset onto the wound. The flap was trimmed and tagged to the wound bed. A chlorhexidine-soaked gauze used to cover the fistula opening to absorb feculant discharge and prevent soilage of the graft. A chlorhexidine-soaked gauze used to cover the fistula site was left exposed. This was changed daily in the ward while the rest of the dressings were left intact. The skin graft was inspected on the sixth postoperative day (Fig. 1). Graft take was 100 percent. The graft was dressed with paraffin gauze and a suitably shaped sponge was applied and anchored with staples. The sponge occluding the fistula opening was removed only after the dressing over the graft was completed. Flavin wool was laid around the fistulous opening to absorb feculant discharge and prevent soilage of the graft. A chlorhexidine-soaked gauze used to cover the fistula site was left exposed. This was changed daily in the ward while the rest of the dressings were left intact. The skin graft was inspected on the sixth postoperative day (Fig. 1). Graft take was 100 percent. The graft was dressed with paraffin gauze for another week before it was exposed. Only the small fistulous opening required a daily dressing change (Fig. 2).

This case highlighted the fact that even a contaminated wound can be grafted successfully if the recipient bed is prepared well, notwithstanding the continual soiling from an enterocutaneous fistula that was within the wound. Once the graft is stable, a colostomy bag can be applied to collect the fistulous content. This reduces the odor and maceration of the surrounding area. Successful early skin grafting hence decreased the need for wound dressing and nursing care requirements, with decreased health care costs to the patient.

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**ESTIMATION OF VOLUMETRIC INDICES OF SKIN MICROVASCULAR BLOOD FLOW WITH LASER DOPPLER FLOWMETRY**

*Sir:* Laser Doppler flowmetry is widely used to measure microvascular blood flow in plastic surgery. Laser Doppler flowmetry signal is sensitive to changes in local linear velocity of blood flow and the number of red blood cells. However, changes in the signal may not correlate well with those in volume flow, particularly during vasoconstriction. Therefore, calculation of vascular resistance (mean arterial pressure/laser Doppler flowmetry signal) or cutaneous vascular conductance (laser Doppler flowmetry signal/mean arterial pressure) using the laser Doppler flowmetry signal is not always appropriate. Moreover, the value of the signal represents both nutritive flow and blood shunting.

I propose eight formulas to estimate volumetric indices of skin microvascular blood flow. The formulas include average maximum amplitudes (in arbitrary units, AU) of periodic oscillations at different frequencies in human skin microcirculation obtained by spectral analysis based on wavelet transformation of the laser Doppler flowmetry signal. The cardiac-dependent frequency interval of 0.8 to 2.5 Hz represents a correlation of flow to first-order pressure waves; respiratory-dependent oscillations (0.2 to 0.6 Hz) are correlated with pressure of venous outflow; the interval of 0.06 to 0.2 Hz corresponds to intrinsic myogenic activity (vasomotions) in the vessel wall of precapillary sphincters and metarterioles; frequencies of 0.009 to 0.02 Hz have an endothelial origin and are partly mediated by nitric oxide.

A coefficient (C) is necessary to transfer laser Doppler flowmetry signal/mean arterial pressure to an index of volume flow:

\[
\text{Index of volume flow} = \frac{\text{laser Doppler flowmetry signal}}{\text{mean arterial pressure}} \times C. \quad (1)
\]

The basic law of hemodynamics (volume flow = pressure/vascular resistance) may be used to calculate the value of C.

1. The index of microvascular blood pressure can be calculated as follows:

\[
\text{Index of microvascular blood pressure} = \frac{Ac}{Ar}, \quad (2)
\]

where Ac and Ar are the amplitudes of cardiac-dependent and respiratory-dependent oscillations, respectively.

2. Resistance to blood inflow depends on vascular tone at the level of the arterioles; in the majority of cases, its quantity is inversely proportional to the dominant amplitude of neu-
Congenital Hypertrophy of the Abductor Digiti Minimi Muscle of the Foot

Sir:

Soft-tissue masses of the foot are rare and present a difficult diagnostic problem. These unusual lesions can be congenital or acquired. The diagnosis is made by imaging and histological examination. When the mass is clinically solid, there is concern about the possibility of a malignant soft-tissue
tumor. The indication for surgical intervention is based on functional impairment and usually involves ruling out malignancy or ameliorating pain and discomfort. One case of a rare, isolated, congenital hypertrophic abductor digiti minimi of the foot is presented with reference to its clinical picture and treatment.

A 20-year-old woman presented with a large soft-tissue mass involving the lateral aspect of her left foot (Fig. 1). The mass had been present since birth and there was no history of other congenital anomalies. Clinical examination of the left foot revealed a soft, nontender mass occupying almost the entire lateral arch. The patient had no functional impairment, but of major concern were the remarkable contour deformity and her inability to wear normal shoes. An ultrasound scan of the mass accurately defined an abnormality caused by hypertrophy of normal muscle tissue and not by neoplasm. Surgical excision of the entire mass was decided on, and under general anesthesia and tourniquet control, an enlarged abductor digiti minimi muscle was identified that contained grossly normal-appearing muscle fibers. The muscle was excised completely and the specimen was sent for pathologic evaluation. Three weeks postoperatively, the patient was able to walk independently and to wear matching shoes (Fig. 2). The histological diagnosis was left foot muscle with hypertrophy and areas of fibrosis. At her 1-year follow-up visit, no scar contracture was observed and the patient had experienced no functional problems. Moreover, she was satisfied with her ability to wear normal shoes.

Localized muscle hypertrophy is extremely uncommon. Congenital disorders produce asymmetry and the hypertrophy is attributed to disordered embryogenesis. The clinical examination, together with imaging and histological examination, can decide the diagnosis of muscle hypertrophy, pseudohypertrophy, hemihypertrophy, or neoplasm. Ultrasonography, computed tomography, and magnetic resonance imaging can reliably demonstrate the nature of the underlying mass. In simple muscle hypertrophy, there is a diffuse increase in muscle bulk but the normal architecture is maintained. In pseudohypertrophy, there is a diffuse increase in echos within the muscle that correlates with the degree of pathological changes and particularly the proliferation of fat or connective tissue, as seen in Duchenne’s muscular dystrophy. In partial gigantism, the hypertrophy involves both bone and soft tissue with associated lipomatosis. In this case, the management of the problem is very difficult and simple excision of soft tissue does not produce satisfactory results because of the abnormal growth in the skeleton.

The literature contains scattered reports of congenital muscular hypertrophy involving the foot. A review of 2720 foot lesions in 2443 patients revealed no congenital muscular tumors or hypertrophies, thus confirming their rarity. A few cases of congenital foot muscle hypertrophy have been reported, including that of the extensor digitorum brevis, the abductor hallucis, and the quadratus plantae muscles. Only one case of hypertrophic abductor digiti minimi muscle has been reported. Surgical treatment ranges from no resection to partial or total excision of the muscle, with or without the overlying excess skin.

Although congenital hypertrophic muscles of the foot are relatively rare and present a difficult diagnostic problem,
clinical examination combined with imaging and histological examination can lead to the diagnosis. Surgical excision of the muscle is the appropriate treatment that can provide excellent aesthetic and functional results.

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