the exception of strenuous exercise. Many are out to dinner and shopping the same day as their surgery, attend sporting events, and return to work the next day or the following day. Patients are permitted to lift up to 25 pounds after surgery and may engage in sexual relations the day of surgery. All patients since June of 2004 could put their arms above their heads before they were discharged from my office-based surgical facility (3-year reoperation rate, <2 percent; hematoma, <1 percent). I have also identified only one capsular contracture since 2006.

There is nothing "dubious" about obtaining a recovery in 24 hours after breast augmentation surgery. Like it or not, it is for real, reliable, and reproducible. The critics of this procedure call the proponents of the 24-hour recovery "paternalistic" and "physician-centric." Nothing could be further from the truth. Patient education and surgeon commitment are key requisites to delivering this level of care. My patients are well educated and understand that achieving another level requires their commitment to scientifically proved process, not just their subjective desires. Patients seek my opinion because I want to learn, have learned, and can deliver a different level of recovery and outcome. When I explain to them what is possible, and the level of scientifically published data behind it, no patient would choose a 25-day recovery when they can have a 24-hour recovery.[2]

This system was created to be both reliable and reproducible for any surgeon who is willing to learn it and commit to implementing its principles. I am a testament to that. Twenty-four–hour recovery works. To call it dubious is far from the truth. It is better for us and our patients.

When processes are clearly defined and published enabling a patient to be out to dinner and shopping instead of at home in pain, nauseated from narcotics, bound in dressings, with limited mobility, how can a surgeon not at least try to offer a patient the out-to-dinner/shopping option? [1–7]

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24-Hour Recovery in Breast Augmentation: Refreshing Honesty and Dubious Notions

Sir:

A recent discussion of an article in the *Journal* prompts this Letter.[1] After pointing out several pertinent weaknesses in the article under discussion,[2] the discussant states the following: "However, there are interesting findings. First, the 'back-to-normal' period of 25 days is a refreshingly honest assessment compared with the dubious notion that this can reliably occur with 24 hours." At the conclusion of this sentence, the discussant references the discussion[3] of articles I authored more than a decade ago in 2002 in this *Journal*.[4,5]

Those articles documented 24-hour recovery and the processes used to deliver it successfully in 96 percent of 627 patients, and provided detailed information of every process used to deliver those outcomes. The first of those publications[4] included video clips of a patient operated on at the 2000 Baker Gordon Symposium before some 600 surgeons. Video clips in the *Journal* show the patient blow-drying her hair 3 hours after surgery, eating raw oysters 4 hours after surgery, and shopping and dancing in Coconut Grove in less than 6 hours following surgery. The next year, a similar size audience saw a follow-up videotape documenting totally normal activities, excluding athletics the next day, until she boarded a plane for home. That patient experience was delivered 13 years ago, observed directly by hundreds of surgeons in 2 successive years, and then peer-reviewed and published in this *Journal*. When the articles were submitted, editors were invited to observe surgical procedures, participate in postoperative phone calls, and personally review all of the raw data from the study.

Since 2000, more than 80 surgeons from all parts of the world have personally visited and observed in our operating facility. Many have listened to postoperative telephone calls to patients and directly observed patients 1 day after surgery. Every colleague has always been welcome in my operating room, and I have always offered every level of information sharing with any colleague who calls or contacts me. Surgeons who have made substantial efforts routinely deliver similar results and recovery every day, and I have been overwhelmed by the level of appreciation from colleagues, regardless of the ultimate level of recovery they may deliver,

depending on their individual circumstances. Today, 99 percent of our patients achieve full normal activities within 24 hours, and 92 percent at last assessment 2 years ago were shopping, out to dinner, or caring for their children at home the evening of surgery. "Dubious notion?" "Reliability"? The transferability of these processes has been clearly demonstrated and published by other surgeons, one of whom demonstrated a 97 percent return to normal activity within 24 hours in 300 consecutive patients.[5] Other surgeons, including Dr. Bill Adams and Dr. Steve Teitelbaum, have documented similar patient recovery in patients operated on at live surgery venues, including the Atlanta Breast Symposium.

I respect the prerogative of every colleague to disagree and debate any concept that affects patients. I also respect the rights of every colleague to choose to deliver or not deliver any technique or level of care they choose. I particularly respect those who make extraordinary efforts with limited resources or under particularly challenging practice situations. Every surgeon can certainly choose to say, "I tried, but it doesn't work for me," or "My practice is doing just fine and I am satisfying my market base and earning an income I am happy with, so I simply have no incentive to work at changing what is working," or "I have independent resources that can satisfy my lifestyle with or without surgery, so I choose to continue what I am doing," or even "I simply don't like the author of this information, and have no interest whatever in even reading it." Each of those statements is intellectually honest and ethical. Dismissively labeling more than a decade of reality and peer-reviewed science as a "dubious notion" and contrasting it in the same sentence to another article using the words "refreshingly honest" is something entirely different, especially when written by a discussant who is a section editor of this *Journal*.

I believe in accountability and that individuals, regardless of their title or status, are responsible for the choices and statements they make. Choosing to infer, even tangentially with "artful" wording, that 24-hour recovery is a "dubious notion" defies reality and reflects a dismissive willingness to promote mediocrity for patient recovery when another level has been clearly documented for more than a decade in the most respected forums in our specialty. The discussant has every right to ignore or not make any effort to learn or implement anything he chooses. But I challenge the veracity and scientific credibility of labeling 24-hour recovery and the proved processes that deliver it routinely and predictably, as a "dubious notion."

More importantly, I categorically reject any implication that challenges the honesty, on any level, of anything I have published or stated in my professional life. "Refreshing honesty" contrasted in the same sentence that labels reality and proved science as a "dubious notion" may be interpreted by individuals differently. Every article or discussion written by the discussant has presumably been subjected to the same stringent peer-review processes as my publications in this, the most respected *Journal* in our specialty. I am surprised—whether this statement was inadvertent, a slip of the tongue, a senior moment, or something more self-serving and disrespectful—that it escaped the scrutiny of reviewers (if a section editor's comments are reviewed).

"Refreshing honesty" is reality and truth, not what someone might like it to be to rationalize similar beliefs and choices to justify the status quo and choose to not improve the recovery experience for patients. "Dubious notion" is the belief that such statements can be made without challenge, regardless of who makes them. Dismissiveness neither justifies nor validates mediocrity in patient recovery.

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Repairing the High-Riding Nipple with Reciprocal Transposition Flaps

Sir:

We read with great interest the article entitled "Repairing the High-Riding Nipple with Reciprocal Transposition Flaps"[1] and commend the authors on their well-designed flaps. As we all know, symmetry is one of the core principles in achieving good cosmetic outcomes for the breast. Without the symmetry of the nipple-areola complex, the surgical results will never be satisfying, even if the breast lumps are symmetrical. The high-riding nipple-areola complex is a clinical entity

that is not rare in patients after mastectomy or mastopexy or in patients with congenital breast diseases.

For Western patients, reciprocal transposition flaps and reciprocal skin grafts[2] have been shown to be effective, although inevitably they will all leave several scars on the breast. However, for Asian patients, who are more prone to developing hypertrophic scars and skin graft hyperpigmentation, the results may not be as aesthetically pleasing. We therefore recommend that surgeons should try to avoid any unnecessary skin grafts or incisions on the breasts of Asian patients.[3] During our operations, the nipple-areola complex is harvested as a full-thickness skin graft, with the subsequent donor site closed into a transverse line. Then, the patient is sat upright and the location of the new nipple is marked. To increase the projection of the new nipple, only the epidermal layer of the new nipple-areola area is removed. The graft is then sutured using standard tie-over and bolster techniques. The tie-over dressing is left in place for 3 weeks (Fig. 1).

The end result of this procedure leaves only a transverse scar at the donor site and a circular scar around the nipple-areola complex. Because Asian patients are more susceptible to developing hypertrophic scars and graft-site hyperpigmentation, we strongly believe this technique achieves more favorable results.

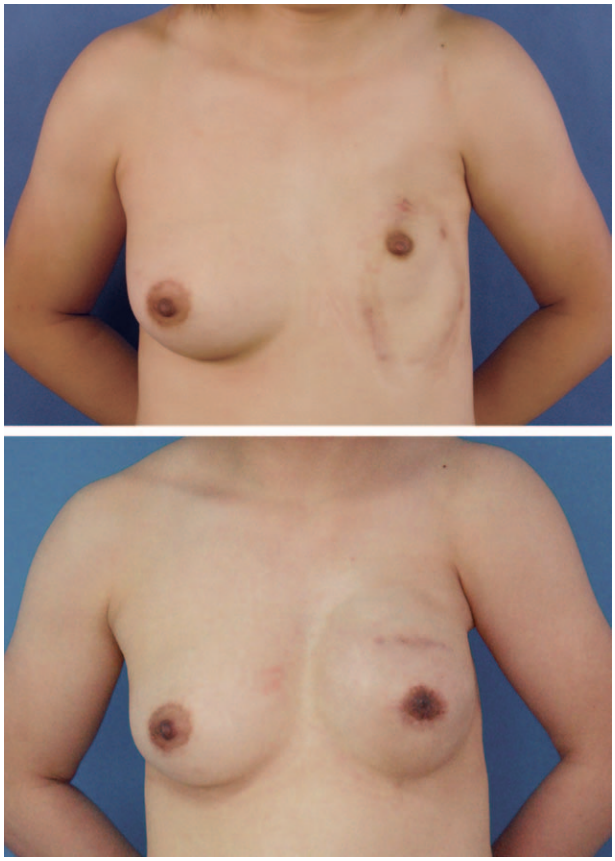

Fig. 1. (*Above*) A 28-year-old female Poland syndrome patient. The left nipple-areola complex was hypoplastic and displaced superiorly. (*Below*) Six months after breast reconstruction and high-riding nipple correction.

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Incidence of Concomitant Airway Anomalies When Using the University of California, Los Angeles, Protocol for Neonatal Mandibular Distraction

Sir:

We read with great interest Dr. Andrews and colleagues' article entitled "Incidence of Concomitant Airway Anomalies When Using the University of California, Los Angeles, Protocol for Neonatal Mandibular Distraction."[1] Their report represents the largest study to date on the use of mandibular distraction for the treatment of infants with Pierre Robin sequence and severe airway obstruction. Based on their experience, which includes an impressive success rate of 97 percent, an algorithm is proposed for the management of these patients.

Our experience with mandibular distraction in this same patient population, also published in the *Journal*,[2] supports some of the recommendations from the University of California, Los Angeles. In 50 consecutive patients, we demonstrate that gastroesophageal reflux disease and the need for Nissen fundoplication was statistically associated with failure of distraction. However, the study by Andrews et al. makes clear recommendations not to perform mandibular distraction on patients with laryngomalacia, a suggestion not arising from evidence-based

medicine.[1,3] Many years ago, we initiated mandibular distraction on patients with Pierre Robin sequence and select airway anomalies. Our published experience demonstrates no statistical association of airway abnormalities with failure of distraction in appropriately selected patients. In our study population, 13 of 50 patients had associated airway abnormalities and 11 of 50 had laryngomalacia.

There have been many algorithms published in the *Journal* based on successful treatment plans instituted by high-volume centers. Evidence of positive results are provided, but often without proof that deviation from the proposed algorithm is indeed detrimental. This assumption of a negative effect can prevent the application of useful techniques to the benefit of our patients. Mandibular distraction is effective and can alleviate airway obstruction secondary to micrognathia. We believe the indications can be extended to select patients with airway abnormalities, including laryngomalacia. Our study provides statistical support for this proposal. We hope that the craniofacial team and the University of California, Los Angeles and other institutions may consider application of distraction in this patient population as, in our experience, it can be a safe and effective intervention.

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Reply: Incidence of Concomitant Airway Anomalies When Using the University of California, Los Angeles, Protocol for Neonatal Mandibular Distraction

Sir:

We would like to thank Dr. Flores et al. for their comments concerning the use of mandibular distraction in neonates with laryngomalacia. The authors reference their own article that has been accepted but not published, which demonstrates the use of mandibular distraction in 11 of 50 children with laryngomalacia. In our study, we developed a protocol that we believe to be safe and efficacious for the treatment of neonates with Pierre Robin syndrome, as no clear treatment criteria have been accepted in the literature. In our study, concomitant airway anomalies were more prevalent than we expected (28 percent). Laryngomalacia accounted for more than half of these anomalies (53 percent). The diagnosis of laryngomalacia was made at the time of diagnostic laryngoscopy as described by Olney et al. at the University of Iowa.[1] Type 1 laryngomalacia (most common) demonstrates prolapse of the mucosa overlying the arytenoid cartilage, type 2 has shortened aryepiglottic folds, and type 3 (least common) has posterior displacement of the epiglottis. Of these three, only type 3 would seem to benefit from mandibular distraction, as the repositioning of the tongue base most likely moves the epiglottis into a more anterior position, alleviating airway obstruction. It is also well known that most children with laryngomalacia (80 percent) will outgrow this condition within the first several months of life without intervention. Therefore, we acknowledge that the authors successfully used this surgical technique safely in their article. However, we caution others that a conservative approach to an airway obstructed by two or more lesions is most prudent. Mandibular distraction is only one of many tools that may be used to treat upper airway obstruction safely.

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Harvesting the Omentum for Poststernotomy Mediastinitis

Sir:

The eloquently written and beautifully illustrated article by Vyas and colleagues[1] presents some strong arguments for limiting the abdominal access to harvest the omentum in the management of poststernotomy mediastinitis. We concur with this principle. It is with a similar intent that some advocate harvesting the omentum laparoscopically.[2,3] Indeed, it is true that more equipment is required for the latter—not more personnel (one surgeon, one assistant, and one scrub nurse). The other points advanced in the penultimate paragraph in their article[1] against laparoscopic harvest of an omental flap also warrant closer scrutiny.

First, yes, laparoscopy does require additional stab wounds (all <10 mm); however, bear in mind that in at least 15 percent of their cases[1] additional abdominal incisions were necessary to release the adhered omentum. Second, the percentage of ventral hernias was more than doubled by converting the transdiaphragmatic approach to a laparotomy. The incisional hernia rate is distinctively higher when compared with the 1.5 to 1.8 percent rate reported following laparoscopy.[4] Third, the mean blood loss (Table 3) following laparotomy was significantly higher compared with not only the transdiaphragmatic approach but also with laparoscopy. The latter offers a clear and complete view during dissection, avoids blunt adhesiolysis, and reduces bleeding to an absolute minimum. Finally, during laparoscopic harvest of an omental flap, the loss of any amount of pneumoperitoneum only occurs once the flap has been safely dissected, prepared, and at the end of the abdominal surgical procedure. Obviously, creating a wrist/lower arm–sized transdiaphragmatic opening at the beginning of the procedure limits the possibility of laparoscopic harvest of an omental flap. This opens the door to a pertinent question. In the study by Vyas et al.,[1] a steady increase in the number of laparotomies since 2002 can be observed (Fig. 3). Was this because of the failure of intention-to-treat by technique (transdiaphragmatic harvesting) or were the laparotomies planned preoperatively? If planned, it would be of interest to know which selection criteria the authors developed for either of the procedures.

It may be fair to seek an alternative to the transdiaphragmatic approach to the omentum in patients who have undergone previous, perhaps extensive, abdominal surgery. This does not have to be a laparotomy. Laparoscopic harvest of an omental flap has been shown to be feasible and *not* a contraindication in these cases. Most of the patients in the four largest case series of laparoscopic harvest of an omental flap for reconstruction in poststernotomy mediastinitis had a history of previous, major abdominal surgery.[2,3] In each, the outcome following laparoscopic harvest of an omental flap was good.

A recently published meta-analysis for a systematic review comparing patient-based outcome with muscle flaps or an omental flap indicated a slight survival advantage for reconstruction with an omental flap (overall relative risk, 1.29; 95 percent CI, 0.58 to 2.88).[5] In addition, the results of our systematic review[5] suggest that muscle flaps are associated with more, and more frequent, complications compared with omental flaps.

Vyas et al.[1] do not compare their results with a similar cohort of patients in whom muscle flaps were harvested. However, their good results lend further support to lowering the threshold in preferentially choosing the omentum in the treatment of poststernotomy mediastinitis when a flap is indicated.

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Reply: Transdiaphragmatic Omental Harvest: A Simple, Efficient Method for Sternal Wound Coverage

Sir:

We thank Dr. van Wingerden and colleagues for their thoughtful discussion about the benefits of laparoscopic omental harvest for sternal wound coverage. Fortunately, these cases are rare and the incidence of sternal wounds has decreased dramatically in our hospital.[1] For clarification, using our technique, we perform the omental dissection off the transverse colon under direct vision. Also, hernias tend to occur several months after the operation in patients that have put on substantial weight. We would caution readers about drawing too many conclusions when comparing the results of studies conducted at different institutions, as the baseline patient characteristics, surgical techniques, and postoperative care may differ.

The omentum provides excellent coverage of the mediastinum and is quite effective for treating complex thoracic infections. The specific harvest approach is less important and can be adjusted based on institution- and patient-specific factors.

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High Blood Pressure (Hypertension) May Influence the Results of Clinical Trials for Scar and Keloid Treatments

Sir:

In the field of scar management, including its prevention and treatment, the number of clinical trials using new drugs and devices has increased considerably, as evidenced by recent publications in *Plastic and Reconstructive Surgery*.[1,2] In this Letter, I would like to draw attention to the link between high blood pressure (hypertension) and scar quality and appearance, which could have a considerable impact on future clinical trials. In light of this, I would like to propose that screening for hypertension should be an integral part of clinical trials for keloid and scar treatments.

In our hospital, where cases of keloid and hypertrophic scar have been treated over an extended period, we have observed that severe keloids are often associated with high blood pressure (hypertension). Indeed, an analysis of 100 keloid patients in 2011[3] revealed that patients with multiple (more than three) or large keloids (>10 cm^2) had higher blood pressure than patients with mild keloids (fewer than two or <10 cm^2). For example, in one case, keloid symptoms were alleviated when antihypertensive drugs were administered.[4] Although an analysis of the link between hypertension and keloids is ongoing, the results so far suggest that hypertension is an important risk factor for the aggravation of keloids and scars.

The generation of heavy scars, including keloid and hypertrophic scars, is associated with multiple factors. These include local factors (e.g., tension), genetic factors (e.g., single nucleotide polymorphisms), and systemic factors (e.g., hypertension). Hypertension is one of the risk factors for heavy scars. I would like to draw attention to the following points:

1. The main cause of keloids and heavy scars is unlikely to be hypertension, because one-fourth of adults suffer from hypertension, but most do not develop keloids.
2. However, we observed that keloid or hypertrophic scar formation was higher after invasive surgery in patients with hypertension than in similar patients with normal blood pressure. Actually, many elderly patients developed keloids for the first time after undergoing such surgery (Fig. 1). This suggests that such patients are different from patients who acquired keloids earlier in life.
3. By itself, treatment for high blood pressure cannot cure heavy scars, but symptoms of scars may be improved by such treatment. Moreover, treatment for high blood pressure may prevent the formation of heavy scars.
4. People of African American or African origin have significantly higher rates of hypertension than Caucasian people[5]; this may be related to the fact that the former have higher rates of keloids.
5. Heavy scars and hypertension may have a common pathologic condition, but this should be verified in the future.

I would recommend that the blood pressure of patients with heavy scars be checked, and that hypertensive patients be excluded from clinical trials of new drugs and devices for the prevention and treatment of

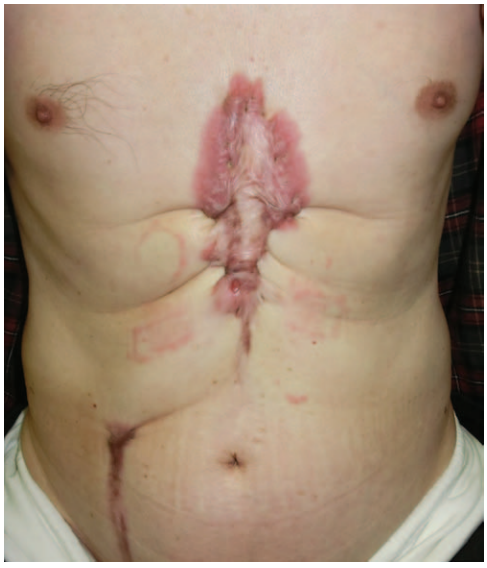

Fig. 1. A keloid case associated with hypertension. The patient was a 66-year-old man with hypertension. He had no keloids before the age of 60 but developed keloids after surgery. Patients who do not have a strong genetic disposition for keloid formation, but who experience hypertension, may be more susceptible to severe scars.

scars. Recently, a clinical trial for transforming growth factor-β3[1,2] was stopped in phase III. Such results may be changed if the blood pressure of the participants is tested before the clinical trial is commenced.

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Local Anesthetics in Liposuction: Considerations for New Practice Advisory Guidelines to Improve Patient Safety

Sir:

Dr. Pace et al. revive an old prejudice against bupivacaine.[1] Their article echoes Klein's objections that were published in the *Journal* in 1998.[2] The authors contend that "there is evidence to support the avoidance of bupivacaine in infiltrate solution recipes because of the potential for fatal complications of toxicity." However, they do not reference any examples of adverse effects when bupivacaine is administered in this manner. The authors offer no data of their own or personal experience with this anesthetic agent.

Bupivacaine has been used for decades in plastic surgery. To date, there has been not a single reported case of toxicity from its use in wetting solutions. My investigation, published last year and referenced by the authors, represents the only study to actually measure levels of this anesthetic agent in plastic surgery patients. The maximum plasma level was 0.81 μg/ml, well below the toxic threshold of approximately 3 μg/ml, with no evidence of toxicity.[3]

The authors acknowledge that "the risk of toxicity from any local anesthetic is lower when uptake into the systemic circulation is gradual."[1] Plasma assays reveal a very slow absorption of bupivacaine, much slower than lidocaine.[3] Bupivacaine is not even detected in the circulation until 4 hours after its infusion into the abdominal fat layer. Its level rises very gradually over 20 hours, and it may still be detectable in the plasma at 48 hours (Fig. 1). This profoundly delayed release into the circulation is not just an effect of epinephrine, which would be expected to delay lidocaine and bupivacaine absorption equally. Bupivacaine partitions more readily into fat than lidocaine. Small amounts of bupivacaine enter the circulation from this fat tissue reservoir—a sort of physiologic pain pump—incrementally over a period of more than 2 days. Bupivacaine's greater affinity for fat is an important advantage over other, less lipophilic local anesthetics such as lidocaine, in terms of both efficacy and safety (i.e., more in the tissues, less in the blood). Studies of intradermal injections that suggest only a small prolongation of anesthesia for bupivacaine in the presence of epinephrine[4] cannot be extrapolated to infusions into the fat layer.

My practice is to limit the use of bupivacaine in wetting solutions to infusion of the abdomen before abdominoplasty, a procedure that causes more

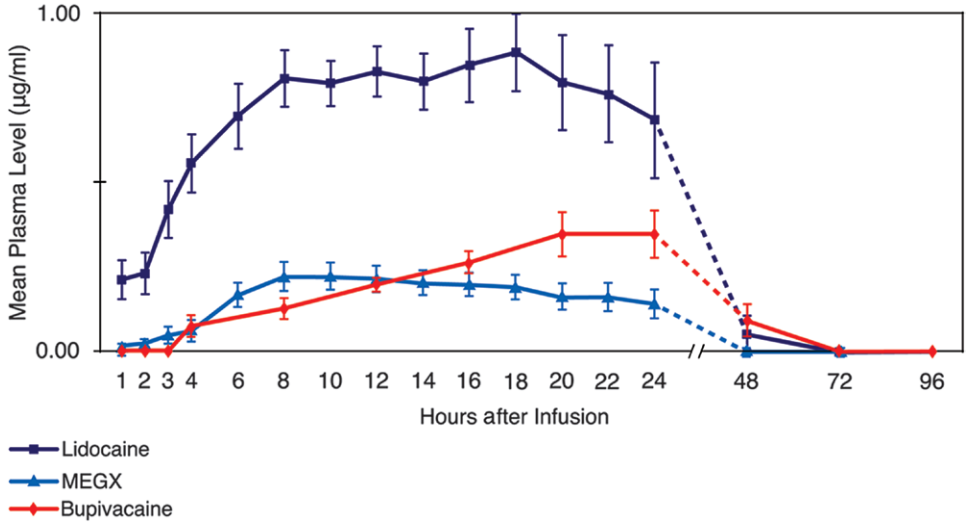

Fig.1. Lidocaine and monoethylglycinexylidide (*MEGX*) levels in 12 consecutive liposuction ($n = 3$) and lipoabdominoplasty ($n = 9$) patients whose levels were measured after infusion, hourly for the first 4 hours; every 2 hours from 4 to 24 hours; and at 48, 72, and 96 hours after infusion. Bupivacaine levels were also measured in the nine abdominoplasty patients who received bupivacaine. Data are presented as means ± SEM. Note the change in the time scale of the graph after 24 hours. (Illustration reprinted from Swanson E. Prospective study of lidocaine, bupivacaine and epinephrine levels and blood loss in patients undergoing liposuction and abdominoplasty. *Plast Reconstr Surg.* 2012;130:702–722.)

postoperative pain than liposuction alone.[3] Failey et al. report that lipoabdominoplasty patients who received bupivacaine wetting solutions had significantly shorter hospitalizations than a control group of patients who received lidocaine or saline infusions.[5] This serendipitous finding is not surprising in view of bupivacaine's well-known potency, prolonged duration of action, and, perhaps most importantly, affinity for fat tissue. In my series of 322 consecutive liposuction and abdominoplasty outpatients,[3] there were no hospital admissions for uncontrolled pain or nausea. Recovery room times averaged 51 minutes. Effective and durable anesthesia at the tissue level hastens recovery and minimizes the need for systemic medications and their adverse side effects. Effective peripheral anesthesia also avoids the need for rib blocks, subfascial injections, and pain pumps, all of which introduce complications of their own.

The authors speculate about possible toxicity from cerebral ion trapping if anesthetized patients develop respiratory acidosis.[1] In my study, carbon dioxide levels in the blood remained normal, and there were no cases of neurologic toxicity.[3] The real clinical issue is actually respiratory alkalosis (causing hypokalemia), which is why spontaneous breathing is preferred over mechanical ventilation.[3] Of course, precipitation of bupivacaine is easily avoided by not adding sodium bicarbonate to infusions.

The authors reproduce Klein's formula (without sodium bicarbonate) and describe a linear relationship between patient weight and the maximum infiltrate volume.[1] In practice, there is no need to infuse more than 5 liters. A 0.05% lidocaine solution is already the standard. More dilute concentrations, in the range of 0.01% to 0.05%, are unnecessary and may compromise efficacy.

The authors caution that mixing lidocaine and bupivacaine, a common practice among plastic surgeons for decades,[3,5] is not supported by the U.S. Food and Drug Administration because of insufficient clinical data. In fact, such data are now available.[3] These guidelines do not always reflect the most recent literature; this federal agency still does not recognize the safety of lidocaine doses greater than 7 mg/kg, either.

Evidence-based medicine calls for the use of the best available evidence in making clinical recommendations, as opposed to relying on old prejudices or unsupported opinions. Our efforts should be directed toward real causes of patient morbidity and mortality[6] rather than hypothetical ones. Complications from oversedation typically occur within the first 24 hours after surgery.[6] By extending tissue anesthesia, bupivacaine may reduce the need for systemic medications during this critical time period and avoid patient overmedication for pain control. Its unique characteristics make bupivacaine a valuable asset in plastic surgery to optimize analgesia and improve patient safety.

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DISCLOSURE

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Reply: Local Anesthetics in Liposuction: Considerations for New Practice Advisory Guidelines to Improve Patient Safety

Sir:

For those practitioners who routinely incorporate regional anesthesia into the management of surgical patients, the benefits of using local anesthetics to control postoperative pain, as described by Dr. Swanson, are most certainly appreciated. Perineural blockade with potent amides, such as 0.5% bupivacaine, provide patients with prolonged opiate-sparing pain control, among other benefits. At our institution, the choice of local anesthetic for surgical cases is not decided on by practitioner "prejudice"; rather, the decision is made by considering both patient and surgical factors in conjunction with the different pharmacokinetic and pharmacodynamic properties of each individual agent. Appropriate choices for safe and effective perioperative patient management are made on the basis of sound scientific evidence and good clinical judgment. The choice of local anesthetic agents for liposuction is no exception.

The central point for discussion is this: to date, no clinical study had been published demonstrating that bupivacaine benefits patients after liposuction. Nor do we know whether large, dilute doses of bupivacaine can provide a greater duration of more potent analgesia than dilute lidocaine. In fact, clinical and pharmacologic data from the dermatology and anesthesia literature published in the latter half of the twentieth century suggest that it does not.[1] Without evidence demonstrating clear benefits of using an agent that has been linked to fatal cardiac arrest in otherwise healthy adult patients,[2] the use of high-volumes of bupivacaine in liposuction is simply an "unsupported opinion." The "old prejudice" at hand here is the assumption that a more lipophilic local anesthetic must be the superior one.

Analysis of bupivacaine levels in plasma after liposuction procedures is premature and not relevant to clinical practice until this agent has been shown to provide benefit to patients. The reference cited by Dr. Swanson (by Failey et al.) did *not* show (and likewise did not conclude) that lipoabdominoplasty patients who received bupivacaine wetting solutions had shorter hospitalizations than patients who received lidocaine. In a subset of 20 patients, 10 who received a bupivacaine-based wetting solution (including one patient who received additional 1% lidocaine) were compared to control patients who received a local anesthetic–free wetting solution.[3] Although pain scores were not reported, it should come as no surprise that patients who received some form local anesthetic would enjoy shorter recovery room stays compared with patients who received no regional anesthetic at all. Until bupivacaine is compared with the current standard of care (lidocaine wetting solutions), its advantages remain in question.

I wholeheartedly agree with Dr. Swanson's sentiment that recommendations in medicine should be made based on evidence. Investigation into the efficacy of bupivacaine as compared with lidocaine for liposuction procedures should include a comprehensive analysis of relevant clinical outcomes, including the degree and duration of analgesia. Patient satisfaction, which may reflect the degree of opiate-sparing effects of a regional block, is critical when evaluating different pain control modalities. Other clinically important measurements include a comparison of 30-day hospital readmission rates, time to hospital discharge, incidence of cardiopulmonary complications, and surgical-site complications such as infection and tissue necrosis. If dilute bupivacaine can demonstrate measurable clinical benefits over lidocaine when infiltrated for liposuction, those benefits may outweigh the risks of toxicity for patients. Evidence demonstrating safe plasma levels of bupivacaine after infusions in excess of 3 mg/kg, such as meticulously reported by Dr. Swanson,[4] will provide a level of confidence to clinicians who use this agent in their practice. Until evidence provides us with that certainty, however, we have an obligation to protect our liposuction patients from any unnecessary risk.

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Preoperative Symptoms of Body Dysmorphic Disorder Determine Postoperative Satisfaction and Quality of Life in Aesthetic Rhinoplasty

Sir:

We read with great interest the publication entitled "Preoperative Symptoms of Body Dysmorphic Disorder Determine Postoperative Satisfaction and Quality of Life in Aesthetic Rhinoplasty" by Picavet et al. (*Plast Reconstr Surg.* 2013;131:861–868). We agree with the authors that being able to diagnose dysmorphic disorder preoperatively is important to prevent the failure of cosmetic procedures.

The plastic surgeon has to resolve not only an anatomical problem but also a discomfort of personality of the patients. Body dysmorphic disorder is a psychiatric disease characterized by worry with a minimal or nonexistent appearance defect and causes significant distress and interferes with the social life of the patient. The perceived physical anomaly may involve the shape and size of the whole body or may be centered around single units.[1] Patients with body dysmorphic disorder are known to request multiple aesthetic procedures that never leave them satisfied. Whenever we are faced with a patient, we must decide whether the patient may have a therapeutic indication for cosmetic surgery. First, we have to evaluate the patient's motivation for the procedure.[2] For example, a question that we could ask our patient is, "Why do you want to undergo to this cosmetic procedure?" We could receive the following two answers: (1) "to please my partner" (this is not an adequate response to submit a patient to a surgical procedure); or, for example, (2) "my nose makes me feel uncomfortable to be with others and with myself" (this is an appropriate response). The physiognomy of mental suffering has changed and continues to evolve over time. In fact, until the 1980s, the most frequent psychiatric disorders were agoraphobia and claustrophobia, whereas now the most common ones are diseases that affect the patient's perception of himself or herself and his or her own body,

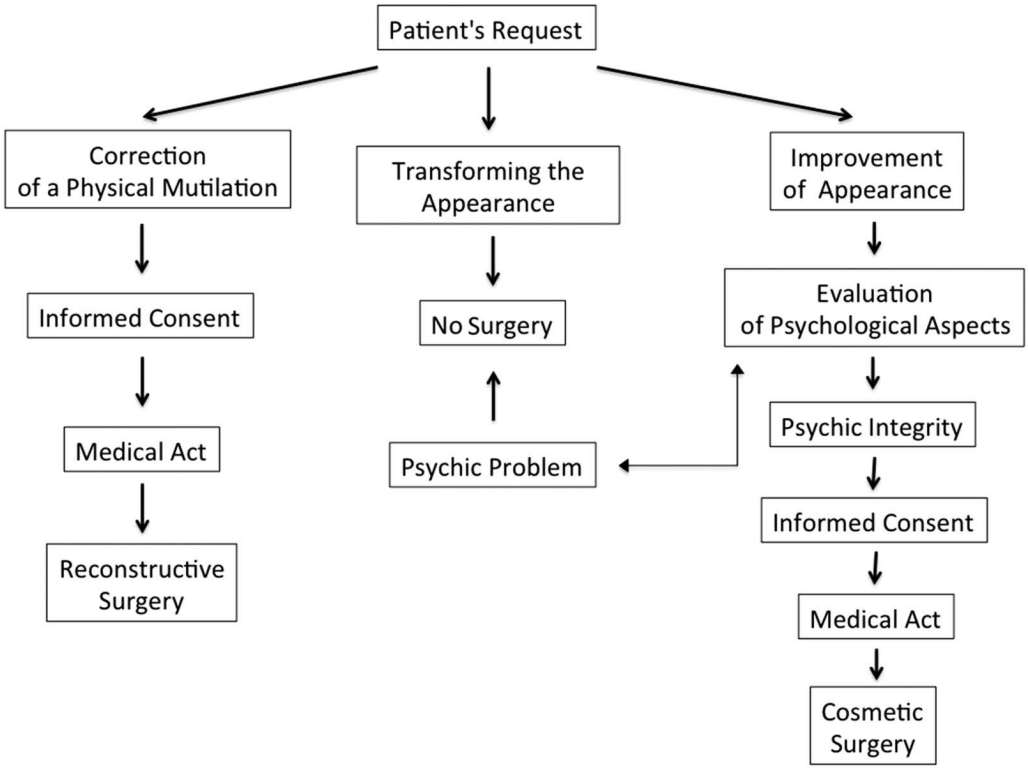

Fig. 1. Therapeutic algorithm for cosmetic surgery.

such as dysmorphic disorder, anorexia, and bulimia.[3] In our experience, we know that rhinoplasty is the most requested procedure by patients with body dysmorphic disorder and therefore this type of surgery requires a greater sensitivity to investigate the patient's history; we noticed that a large number of secondary surgical revisions that we have to perform were on patients affected by psychological disorders. A confirmation of our experience was given in a study by Picavet et al.[4] in which the prevalence of moderate to severe body dysmorphic disorder symptoms in a cosmetic rhinoplasty population was high. Body dysmorphic disorder symptoms significantly reduce the quality of life and cause significant appearance-related disruption of everyday living. Obviously, a patient with this type of disorder can never be satisfied after surgery. How could we answer the patient's request? In our experience, we believe it is necessary to integrate the look and the listening, because the look can make a survey on the presence or absence of an objective problem, whereas the listening caters to the subjective side of a person. In this way, we can understand the person's relationship with their body. We use the algorithm presented in Figure 1 to identify which patients are good candidates for cosmetic surgery, and we are working on another algorithm that can outline an objective indication for cosmetic surgery.

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DISCLOSURE

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

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Deferoxamine Restores Callus Size, Mineralization, and Mechanical Strength in Fracture Healing after Radiotherapy

Sir:

We read with great interest the article written by Donneys et al. entitled "Deferoxamine Restores Callus Size, Mineralization, and Mechanical Strength in Fracture Healing after Radiotherapy."[1] This article was published in May of 2013 and concerns the efficacy of deferoxamine in fracture healing. The authors nicely showed that deferoxamine can restore bony union after radiotherapy and honestly noted some possible limitations of the study. However, we would like to complete the discussion by mentioning another important limitation.

Deferoxamine, a chelating agent used to reduce iron deposition toxicity, has a documented capacity to increase vascularity by means of the hypoxia-inducible factor 1α pathway. Accumulation of intracellular hypoxia-inducible factor 1α leads to an increase of vascular endothelial growth factor. However, recent studies have shown significant side effects of the use of deferoxamine. Deferoxamine can induce dysplastic-like skeletal abnormalities, growth retardation, and knee arthropathy.[1–3] In other words, the importance of bony healing must be weighed against the awful side effects of deferoxamine; in my opinion, it is the main limitation of this study.

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

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Reply: Deferoxamine Restores Callus Size, Mineralization, and Mechanical Strength in Fracture Healing after Radiotherapy

Sir:

We welcome the interesting commentary and questions raised by Dr. Hamid Namazi concerning our article regarding the use of deferoxamine in the prevention of radiation-induced pathologic fractures in the murine mandible. The referenced articles by Chan et al. do indeed show that the high doses of deferoxamine delivered systemically for thalassemia patients receiving transfusions and chelation therapy can cause undesirable side effects such as skeletal dysplasias.[1] In response to his concerns, we would like to address clinically relevant disparities in the duration of treatment, age of the patient population, and dosing between our intended clinical use and the development of skeletal dysplasias in thalassemia patients.

The articles by Chan et al. refer to a pediatric patient population with an average age of 12.1 years that are receiving high doses of systemic deferoxamine over an average period of 8.2 years. According to this dose information, the average pediatric patient received a total of 2,201,325 mg of deferoxamine over the course of their treatment before the development of skeletal dysplasias.[1] It should be noted that in this context, the more established (and rare) severe side effects of deferoxamine are visual and auditory symptoms of neurotoxicity and pulmonary syndrome.[2–4]

In stark contradistinction, our dosage of deferoxamine is a mere fraction of the dose delivered clinically for thalassemia. Furthermore, it is also injected locally into the fracture callus, thereby significantly reducing the possibility of untoward and widespread side effects, such as those reported in thalassemia patients. The clinical application we envision for deferoxamine in the head and neck cancer population would entail elderly patients with a mean age of 62 years, receiving five micromolar injections over a short period. Specifically, in our experiments, we delivered five injections of 200 μM of deferoxamine over a course of 9 days, or 0.263 mg of deferoxamine total.[5,6] Therefore, our dose is smaller than the systemic dose administered to thalassemia patients by a factor of 10^7, or 10 million.

In summary, we disagree with the reviewer's assertions that the consideration of skeletal dysplasia as a side effect of our intended use is a limitation to our study. The reviewer's concern regarding dysplasia is based on data obtained from pediatric patients receiving a high dose of deferoxamine over an extended period. This represents a drastically different patient population, dose amount and duration, and route of administration from what we are proposing in our work. There is no evidence to support skeletal dysplasias resulting from our intended use. Furthermore, deferoxamine has been in use clinically for over 50 years, and its side-effect profile is established and well known.[7] These considerations are essential in the fair evaluation of our work.

We maintain that a more important consideration with this specific application is the tumorigenic safety of deferoxamine in the head and neck cancer patient population. Our findings in this animal model are encouraging and raise important questions about the potential investigation of the timing and dosing of this therapy in a clinical setting. As discussed in the article, the current literature is encouraging for the potential use of deferoxamine in these regards. However, even at these small doses, we still strongly encourage further investigation. We thank you for your commentary and hope to have clarified these concerns.

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