However, none of the organisms reproduced in *P. aeruginosa*.

Among the inoculated saline implants, four of the six species planted (n = 53). The animals were sacrificed at 1-, 3-, and 2-week study duration, as did the gram-negative bacterium *K. pneumoniae*. However, none of the organisms reproduced in either the soybean oil or saline exceeded the 100 organism per milliliter inoculation dose.

For the in vivo experiment, sterile flasks containing either saline or soybean oil were inoculated with the tested organisms, and serial cultures were performed at intervals of 1, 2, 3, 7, and 14 days. This study showed that soybean oil did not support the survival or growth of any gram-positive or gram-negative bacteria, but one fungal species (*P. variotii*) did survive for up to 72 hours in the soybean oil. All three fungal species tested survived in saline for the 2-week study duration, as did the gram-negative bacterium *K. pneumoniae*. Microbiologist Dr. Patrick Murray, of the Department of Pathology at the Washington University School of Medicine, recommended using three representative microorganisms to have metabolic pathways that would enable them to metabolize triglycerides. For the in vitro experiment, sterile flasks containing either saline or soybean oil were inoculated with the tested organisms, and serial cultures were performed at intervals of 1, 2, 3, 7, and 14 days. This study showed that soybean oil did not support the survival or growth of any gram-positive or gram-negative bacteria, but one fungal species (*P. variotii*) did survive for up to 72 hours in the soybean oil. All three fungal species tested survived in saline for the 2-week study duration, as did the gram-negative bacterium *K. pneumoniae*. However, none of the organisms reproduced in either the soybean oil or saline exceeded the 100 organism per milliliter inoculation dose.

For the in vivo experiment, saline-filled (n = 57) or soybean oil–filled (n = 54) implants were inoculated with 100 cfu/ml of the same test organisms and implanted into rabbits. As a control, noninoculated saline implants also were implanted (n = 53). The animals were sacrificed at 1-, 3-, and 6-month endpoints, and the intraluminal saline or soybean oil and specimens of the capsular tissue surrounding the implants were cultured. In the intraluminal soybean oil, neither gram-positive nor gram-negative bacteria were culture positive at any time point, but all three species of fungi tested survived for at least 3 months. Only one species, *C. albicans*, had reproduced in soybean oil at the 3-month endpoint. No organisms were cultured from the intraluminal saline of implants that had not been inoculated (controls). However, among the inoculated saline implants, four of the six species of bacteria had survived and reproduced in the intraluminal saline for at least one of the studied time points; these bacteria were *S. epidermidis*, *P. aeruginosa*, *E. cloacae*, and *K. pneumoniae*, the latter of which reproduced during the full 6-month duration of the study. In addition, two fungal species (*A. niger* and *P. variotii*) were culture positive after 6 months in the inoculated saline implants. Comparisons of capsular tissue specimens taken from the rabbits with either soybean oil– or saline-filled implants revealed that only 3 of 54 capsules surrounding soybean oil were culture positive, and all three may have resulted from contamination of the implant’s outer surface with the inoculum or from migration of the inoculum through the filler valve. Among the control saline implants, 8 of 53 capsular specimens were culture positive. For the inoculated saline implants, 12 of 57 capsular specimens were culture positive, with the great majority of these positive cultures being for organisms other than those with which the implants had been inoculated.

We suspect that the nearly complete absence of bacteria in the capsules surrounding soybean oil–filled implants relates to the fact that small amounts of oil “bleed” through the shell and into the intracapsular space. The types of fatty acids contained in triglyceride oils have long been known to have significant antimicrobial properties. Consequently, the

“bleed” from an oil-filled implant may have a potential salutary effect in preventing or reducing capsular contractures if, as many researchers have suggested, the severity of capsular contracture may be related to the presence of bacteria around an implant. Early analysis of data from patients in Europe who have received Trilucent implants indicates that their capsular contracture rates may be lower than for patients with either silicone gel or saline implants. After 2 years of follow-up, women undergoing primary augmentation with soybean oil implants had the following capsular contracture rates: grade I = 62 percent, grade II = 36 percent, grade III = 2 percent, and grade IV = 0 percent.

In summary, the results of our studies clarify the fact that small amounts of triglyceride oil implant fillers do “bleed” through silicone shells. Subsequent research by the implant manufacturer has defined shell and oil characteristics that optimize shell-triglyceride filler interactions. Increased understanding of this interaction should be relevant to other medical devices (such as pacemakers, hydrocephalus shunts, penile prostheses, and dialysis devices), since triglycerides and fatty acids are ubiquitous in biologic environments. While I agree with many of the points raised by Drs. Brucker, Winger, and Sendelbach and would have preferred to perform more robustly designed experiments, budget constraints prevented doing so. Nevertheless, I believe the results of the study are valid and indicate that the small amounts of triglycerides that do “bleed” through silicone elastomer implant shells seem to be absorbed and either metabolized or redistributed to the body’s normal fat storage sites.

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EXPLORATION OF INTRACRANIAL STRUCTURES ENDSOCOPICALLY

Sir:

After reading the article by Tutino entitled Exploration of Intracranial Structures Endoscopically through Minimal Craniotomies (*Plast. Reconstr. Surg. 97: 1027, 1996*), I was quite astonished and flabbergasted.

He claimed the method used was new and innovative (it also was published under the heading *Ideas and Innovations*). He also claimed that “the success of this study is promising for neurosurgery” and that neurosurgeons will profit from this “new” technique. These statements are presumptuous.

First, we strongly contradict that the described procedure is new. The use of endoscopes in neurosurgery is not by far new. In 1910, L’Espinasse already used an endoscope to remove choroid plexus for the treatment of hydrocephalus. Since then, many attempts have been made to use endoscopes intracranially, but the quality of the scopes and light sources restricted their successful use. Since the 1980s, there has been a resurgence of interest in neuroendoscopy, particularly due to the development of better scopes, cameras, monitors, and light sources. Nowadays, endoscopic techniques are well known and frequently used within neurosurgery. Several textbooks, especially the beautifully illustrated book on intracranial endoscopic anatomy by Pernecky et al., and articles on this subject with illustrations of much better quality than in the article have been published.

Second, the author used a flexible 3.4-mm endoscope. The only advantage of a fiberscope over a lenscope is its smaller size. In this respect, there are commercially available fiber-
optical neuroendoscopes that are much smaller with the same optical quality as shown in the author’s illustrations. However, we would strongly recommend the use of a lenscope (there are neuroendoscopes with a rod lens optic with a diameter of 2.4 mm), which will allow a much clearer view during exploration of the intracranial content. Their rigidity is not a disadvantage if the procedure and the trajectory are carefully planned based on CT and MRI data.

Third, we would advise using neurosurgical instead of orthopedic instruments to perform a craniotomy. We presume the author does not use gynecologic instruments for breast reconstruction.

Finally, the proposed incisions to gain access to the anterior, middle, and posterior cranial fossae need to be elucidated. The proposed incision for access to the middle fossa is located at the level of the linea temporalis, which is not appropriate to reach basal structures. Furthermore, we can hardly imagine how one can reach the posterior fossa by making a craniotomy in the parietal region.

The incision for access to the anterior fossa, presented in Figure 1, is placed too far medially, which will endanger the supraorbital nerve. We often use an eyebrow incision for basal frontal or frontotemporal approaches to skull base lesions and aneurysms. The incision is made laterally from the supraorbital foramen in order to preserve the supraorbital nerve. There also is no need to place the incision superior to the eyebrow. An incision within the eyebrow itself gives excellent cosmetic results.

In conclusion, the article is pretentious and flaunty without being new or innovative. We would like to refer the interested reader to the existing excellent textbooks or articles.

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REFERENCES

A REVIEW ON THE HISTORY OF END-TO-SIDE NEURORRHAPHY

Sir: We apologize for being late in writing, but we hope that our letter may be of some value. The article entitled End-to-Side Neurorrhaphy with Removal of the Epicranial Sheath: An Experimental Study in Rats, by Viterbo et al. (Plast. Reconstr. Surg. 94: 1038, 1994) is very detailed and should be of great interest to all plastic surgeons. This and their other study are both very important with respect to contemporary clinical use of end-to-side nerve coaptation.

The authors mentioned that “the first report was by Bal-

lance et al. in 1903, for the treatment of facial palsy having sutured the distal end of the sectioned facial nerve laterally to the accessory spinal nerve.” However, Kennedy proposed the concept of end-to-side neurorrhaphy 2 years earlier than Ballance (Fig. 1). He performed end-to-side nerve coaptation in a patient with facial spasm in 1899 and then published a report in 1901. Ballance et al. and Harris and Low also mentioned very clearly in their articles that “in 1899 Kennedy divided the facial nerve for facial spasm and united it to the spinal accessory (end-to-side junction).” We believe that it is important to clarify this point for current investigators because Viterbo et al. did not mention it.

The purpose of the end-to-side neurorrhaphy technique is to use a donor nerve without sacrificing the supply to its distribution area. Kennedy reported that the facial nerve was divided close to its exit from the stylomastoid foramen, and then the peripheral end of the facial nerve was sutured to the side of the spinal accessory nerve after opening a window (a large gap extending to the other side of the perineurium). This was done in a 46-year-old woman with incessant muscular twitching on the right side of the face for 10 years. Some temporary paralysis in the territory of the donor nerve (the sternomastoid and trapezius muscles) developed after dividing the donor nerve, but it recovered by 49 days postoperatively. Finally, at 23 months postoperatively, the hemifacial spasm was cured. The patient subsequently had remarkably free movement of the face on movement of the shoulder, and there was no deficiency in the territory of the accessory nerve. Although Kennedy sectioned the accessory nerve as