Gaudino II study from the aforementioned meta-analysis.

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of their article was described in previously published studies.^{2,3}

Figure 1 is a copy of the illustration used in my article.

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TREATMENT OF SUBGLOTTIC STRICTURE

To the Editor:

It was a pleasure reading the article by Mercy George and colleagues in the *Journal*.¹ I would like to note that the technique for treating benign subglottic stricture on pages 414 and 415

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Reply to the Editor:

We thank Dr Demos for his comment on our article on the management of pediatric subglottic stenosis with glottis involvement.

The management of glotto-subglottic stenosis is challenging, and its surgical management, therefore, has evolved over the years with descriptions of the surgical technique and its modifications, along with the use of stents. The technique we have used in this large pediatric series is different from that described earlier by Demos and colleagues in 1969.^{1,2} Notably, this series is the first pediatric series of its kind. The technique combines a subglottic resection and thyro-

tracheal anastomosis with a posterior cricoid split and costal cartilage graft for glotto-subglottic stenoses. This operation was named “extended partial cricotracheal resection.” The LT-Mold prosthesis is also different in that it calibrates the supraglottic and glottic spaces in the abducted position of the vocal cords, while restoring a triangular shape to the glottis with a sharp anterior laryngeal commissure.³ All currently available stents are round or cigar shaped and do not restore a triangular glottis, especially in the case of vocal cord synechia or grade IV transglottic stenosis. The use of the LT-Mold prosthesis has significantly diminished the incidence of postoperative granulations that require repeat endoscopic/open procedures. This study was therefore a synthesis of our experience of complex pediatric glotto-subglottic stenosis that documents the evolution of the technique with an emphasis on the silicone LT-Mold.

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THE FUTURE OF CARDIAC SURGERY

To the Editor:

What is the future of cardiac surgery? This question undoubtedly brings to mind images of fancy new technology, robots, stents, lasers, and other minimally invasive or futuristic techniques.

FIGURE 1. Temporary plastic obturator in upper trachea and glottis fixed with wire. Reprinted with permission.^{2,3}

However, the real question that our specialty needs to address is this: How should cardiac surgery deal with the fact that technology changes rapidly, that the potential therapeutic options for patients increase faster than prospective trials can evaluate them, and that each year patients who undergo cardiac surgery continue to be sicker, older, and at higher risk for complications?

Currently, significant and often unperceived conflicts of interest exist for everyone involved, and it is difficult for the patient to make a well-informed opinion. As patients get sicker and hear about advancing technology, they are more likely to have unrealistic expectations. New technology is often offered to patients more as part of a marketing tool than as a proven therapy. Often, both the surgeon and the cardiologist will be reimbursed from a newer hybrid technique. Conflict of interest can arise from the need to maintain referral patterns and maintain reimbursement. The patient is unlikely to argue against a smaller incision involving a newer approach, even if the newer approach has not been proven to be as good as the current standard of care.

Dr Alec Patterson, President of the American Association of Thoracic Surgery, highlights in his presidential address a need for a change in our professional behavior from the individual to the team approach to care. This change is likely to be a solution to these potential conflicts. Patients with cardiovascular disease should be offered the benefits of a multidisciplinary team approach. This approach has existed for many years (eg, tumor boards and transplant selection conferences) but is not routinely practiced in cardiovascular surgery. A multidisciplinary approach requires that a regular meeting take place at which all of the consultants are present to review the data, discuss what each can offer, and propose a therapeutic plan. Such an approach lays the groundwork for the patient to benefit from the expertise of the entire team and the foundation from which all new technologies and therapies can

be evaluated as they continuously surface. The multidisciplinary approach is to medicine what the scientific method is to research: the best defense against conflict of interest.¹ Thus this approach will always keep the patient's best interest at the center of our focus.

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IS EARLY ANTITHROMBOTIC THERAPY NECESSARY IN BIOPROSTHETIC VALVES?

To the Editor:

We read with great interest the contribution by ElBardissi and associates¹ addressing a controversial issue: the need for early antithrombotic therapy in patients with tissue valves. The authors sought to determine whether this therapy was necessary in patients in sinus rhythm with a tissue valve in the aortic position. Their sample of 861 patients is large enough to conclude that early anticoagulation after isolated aortic valve replacement with a tissue valve does not reduce the risk of thromboembolism.

The authors are right as to whether anticoagulation is a must or not in this type of patients and refer to the recent American Heart Association/American College of Cardiology and European Society of Cardiology recommendations. The impactful and classic paper by Heras and colleagues,² summarizing an experience from the Mayo Clinic, led to the consideration of anticoagulation as an adjunct of choice based on a retrospective analysis during a long period of time with special focus on patients with no antithrombotic therapy. The results of the study of ElBardissi and associates¹ give additional light to previous publications confirming that

the 90-day risk of thromboembolism was 5% in both groups with or without anticoagulation. The authors identified classic risk factors for thromboembolic complications, such as advanced age, female gender, short stature, smoking, and a 19-mm valve. We fully agree with this and their final conclusion.

The authors refer to thromboembolism and the lack of randomized studies to confirm their conclusion. That was our concern when we designed our prospective and randomized pilot trial.³ In this cooperative pilot study, 193 patients were randomized to acenocumarol and triflusal (an antiplatelet agent) for primary prevention of thromboembolism early after tissue valve implantation; 93.8% were aortic valves. We³ aimed at evaluating the efficacy and safety of antiplatelet therapy versus acenocumarol with the primary end point of a composite of thromboembolism, hemorrhage, and valve-related mortality. The results of our randomized trial were that the rate of thromboembolic events was not significantly different among groups, thereby confirming that antiplatelet therapy was an equally protective strategy. In addition, there was a statistically significant difference favoring the antiplatelet group in terms of hemorrhagic events. The conclusions of this first reported randomized trial comparing antiplatelet versus anticoagulant therapies were that there was a safer profile with significantly lower bleeding in the antiplatelet group. The results by ElBardissi and coworkers¹ are consistent with these data in terms of protection against thromboembolic events. Despite some limitations in our design that have already been addressed, we strongly believe that early anticoagulation might not be the ideal therapy considering the currently accumulated experience that, of course, includes this very appropriate and updated experience by ElBardissi and colleagues.¹

Finally, similar conclusions were recently reached by Brueck and associates.⁴ They also addressed patients with no risk factors for