treated with the high-affinity tissue angiotensin-converting enzyme (ACE) inhibitor quinaprilat had significantly better recovery of regional wall motion, a lower incidence of ventricular arrhythmias, and a smaller infarct size than controls. When the low-affinity tissue ACE inhibitor enalaprilat was administered, the decrease in infarct size was less (24% enalaprilat vs 14% quinaprilat; $P < .0001$). The beneficial effects of quinaprilat's action could be explained by its superior preservation of endothelial function, since quinaprilat-treated hearts had significantly greater coronary arterial relaxation in response to bradykinin (66% quinaprilat vs 32% enalaprilat; $P < .0001$). When the bradykinin antagonist HOE140 was added to the quinaprilat-treated hearts, the protective effect on endothelial function was abolished and these hearts had a significant increase in infarct size (48% quinaprilat + HOE vs 14% quinaprilat; $P < .0001$).

I agree that ACE inhibitors enhance myocardial protection during cardioplegic arrest. However, ACE inhibitors are not all equally protective. ACE inhibitors with high tissue ACE inhibition such as quinaprilat result in better preservation of endothelial function, which decreases ischemic necrosis. This improvement in endothelial function appears to be bradykinin-mediated. I concur with Korn and coworkers that high-affinity tissue ACE inhibitors may play an important role in enhancing cardioplegic protection during coronary revascularization.^{2,3}

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Tracheal replacement

To the Editor:

It is encouraging that efforts continue to produce a "tissue-engineered trachea,"¹ since this is about the only technique of the many attempts at tracheal replacement by a variety of approaches over more than 50 years that seems to offer any real promise.² I question, however, whether the experiments reported succeeded in forming "the structural component of a functional tracheal replacement."

Although visually resembling a trachea, the construction described failed to function (in sheep) over 5 and 7 days in 2 animals (with resultant malacia) or longer than 2 days in 4 animals (with stenosis). The authors' findings suggest that inflammatory response might be responsible for degeneration of the graft. Cell density and hydroxyproline content support this hypothesis. Conversion toward scar probably had begun, even with survival of some cartilage. The authors also allude to future advisability of assuring blood supply, which may indeed be a critical factor in this early deterioration of the implant. One of the keys to failure, even of autotransplanted free tracheal segments of any significant length, has been failure of blood supply, despite muscular or omental pedicles. Very short tracheal grafts, of course, are of no potential clinical use.

A subject that is deferred by the authors to later experimentation, perhaps with engineered epithelium, is the lack of epithelial coverage in these experiments. In time this would lead to graft obstruction by cicatrization. Epithelium in general has failed to recolonize any extended length of tracheal replacement by ingrowth from the normal tracheal ends alone.

I shall look forward with interest to future reports by this and other groups using tissue engineering in an effort to meet the challenge of fashioning a usable tracheal prosthesis for the rare occasion when it is indeed needed.

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Cardiac herniation after intrapericardial pneumonectomy

To the Editor:

The brief communication by Baisi and associates¹ reporting a case of cardiac herniation after intrapericardial pneumonectomy is almost identical with a case report in another journal.² Both reports fail to mention that chest tubes were deployed after the pneumonectomy. (They can clearly be seen in their respective illustrations.)

The previous case^{2,3} instigated a spirited correspondence^{4,5} centering on the role, if any, of chest tubes after pneumonectomy and their relation to cardiac herniation. The current case also fails to mention a chest tube in the left posterior thoracic gutter clearly seen on the computed tomographic scan (see Figure 1). No mention is made of its presence or mode of use. Rather the authors emphasize (without justification or substantiation in my mind) that the induction chemotherapy was to blame. Actually the initial closure of the postpneumonectomy pericardial defect failed because the sutures, placed under tension, cut through—a mechanical problem, not a neoplastic or chemotherapeutic problem.

The empty postpneumonectomy space is not to be equated with that containing variable amounts of lung tissue, which require chest tubes attached to suction. Using such continued suction in a postpneumonectomy space even for a short period of time is to court disaster produced by excessive mediastinal shift and, in some cases, cardiac herniation. Despite the opinion of Walker⁴ of the hazards of chest tubes after pneumonectomy, Cooley⁵ expressed it well when he said that he routinely used tubes after pneumonectomy but that "when not in use the tube should be clamped." [italics mine.]

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Reply to the Editor:

Kirschner's comment focuses on two points: the use of a chest tube after pneumonectomy and the similarity of our case to one recently reported in another journal.¹

Regarding the use of a chest tube after pneumonectomy, it is worthwhile to repeat the commentaries of Nazari and Cooley cited in Kirschner's letter.^{2,3} It is a personal choice of the surgeon, and no evidence exists supporting one option or the other. However, we would like to remind Kirschner that after pneumonectomy, the chest tube is not attached to continued suction and does not "court disaster produced by excessive mediastinal shift," as he says, but is connected to an appropriately balanced pneumonectomy drain set.⁴ This is completely different from the one-way valve used after partial lung tissue resection. It is designed to achieve both positive and negative pressure control avoiding any abnormal mediastinal shift.

We reply to the second point of Kirschner's comment stressing the role played by induction chemotherapy in our case, as emphasized both in the title and in the discussion of our article. In the past 5 years we performed 208 pneumonectomies, 47 with intrapericardial vessel ligation. The pericardial defect was closed directly by stitches in 35 cases and by a pericardial patch in 12 cases in which the defect was too large and a direct suture would have been under tension. The reported case of cardiac herniation was the first in our experience. We are sure that the closure we performed in the first operation was not under any abnormal tension, that the pericardium was fibrotic because of the previ-

ous chemotherapy, and that this played a major role in its disruption.

The message we would like to give is not that cardiac herniation may happen after intrapericardial pneumonectomy—this is widely known—but that it is more likely if the patient has undergone previous induction chemotherapy. In these cases, the use of pericardium to close small defects can also be recommended.

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Intussusception and spontaneous amputation of the esophagus

To the Editor:

I read with interest the case of a patient with acute leukemia and segmental esophageal necrosis reported by Salo and associates.¹ The authors imply that intussusception led to ischemia, and circumferential full-thickness loss of the esophagus was repaired "without interruption." Both assumptions may be challenged. The patient had no risk factor for gastroesophageal intussusception,² in which stomach is the intussusceptum, not esophagus, and perforation, rather than necrosis, a reported complication. Fungal infection of the esophagus in leukemia, however, is known to result in full-thickness necrosis and esophageal expulsion.³ Their final comment relating to *Aspergillus* infection of the esophageal wall points to the likely cause of necrosis.

A complete loss of all esophageal layers, no matter how short, is expected to result in cicatricial stricture that may have

been delayed in this case by cytotoxic therapy. Indeed, dysphagia later developed, necessitating further intervention. The mediastinal soft tissues may resemble the esophageal lumen so that perforation is not obvious. However, bare of epithelium and exposed to esophageal contents, the mediastinum provides an entry to systemic infection, particularly in an immunosuppressed state. Esophageal resection with immediate or delayed reconstruction should be considered even in this patient population to prevent mediastinitis and restore swallowing.

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Reply to the Editor

We thank Gaissert for his very valuable comments.

Spontaneous amputation of the esophagus is a serious and rare event, and its exact pathophysiology is not well understood. There were several reasons that led us to believe that intussusception, followed by ischemic necrosis, may be an etiologic factor. Our patient had a hiatal hernia, as previously stated. Hiatal hernia is a widely known risk factor for intussusception, as well as dysphagia, a condition that developed after severe vomiting.¹ On careful inspection of Figure 1, one can see that the inner circular muscle layer of the esophagus is located superficially as a result of intussusception.

Abildgaard and associates² have described total expulsion of the distal esophagus in a severely immunocompromised patient with *Candida* esophagitis. Multifactorial pathogenesis, including intussusception, is presumable inasmuch as their patient had also had regurgitation, vomit-