

to the effects of varying utility values and posttransplantation maintenance costs as well, thereby changing the conclusions formulated by Anyanwu and associates.¹

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Reply to the Editor:

We appreciate the comments of Ouwens and colleagues regarding our article on economic evaluation of lung transplantation. These comments highlight a major limitation of any economic study, which is the reliance on assumptions. Economic studies of therapies that have long-term benefits necessarily make predictions of future events. Because the future is always unknown, assumptions have to be imposed to allow modeling of data into the future. Methods used for modeling data vary from study to study. For this reason most economic studies include sensitivity analyses that examine the impact of alternative assumptions on the study conclusions.

Ouwens and colleagues propose two alternative approaches to examining the benefits of lung transplantation—assessing lifetime benefits and imposing a fictitious

“transplant date” for the waiting list survival. Although it is true that the benefits of transplantation will continue beyond the 15-year time frame of our study, we elected to restrict benefits to those occurring in the first 15 years. We chose to restrict our analysis to 15 years because the robustness of the conclusions diminish as predictions go further over the horizon. The Weibull method used to predict long-term survival is based on the observed short- to medium-term survival of a cohort of patients. In our study we predicted long-term survival on the basis of observed 4-year survival. We believe that this method overestimates survival after transplantation, however, because it does not allow for late deaths resulting from long-term complications of immunosuppression and chronic graft rejection or failure. For example, extrapolation of our data to 20 years suggests that 25% of patients will still be alive 20 years after lung transplantation, a figure that we consider overly optimistic. Indeed, if we were to continue our survival curve until there were no survivors, as suggested by Ouwens and colleagues, we would end up with several patients surviving to 100 years of age after having received a lung transplant.

Although such scenarios are statistically correct, we do not think that they are clinically plausible. Furthermore, any benefits after 15 years would be modest, because discounting means that future benefits carry less weight than do immediate benefits. For example, if we had examined benefits through 20 rather than 15 years, the benefit of single-lung transplantation would only improve from 2.1 to 2.2 quality-adjusted life years.

Assumptions also have to be imposed on defining the start of the waiting list episode, because there is no objective point to define as the start of medical treatment on the waiting list. Unlike transplantation, there is no medical intervention or event that can be ascribed to a particular day. There is at present no clear definition of the time of onset of end-stage lung disease, nor is there an objective method of deciding the point at which a patient should be listed for transplantation. Patients are listed at different stages of the disease process, depending on all sorts of patient-related, logistic, and local factors. We argue that the date a patient is registered on the waiting list is itself a robust measure neither of onset of end-stage lung disease nor of the need for lung transplantation. Methods of ascribing day 0 for waiting list survival (as a proxy for survival without transplantation)

are therefore arbitrary and are bound to vary from study to study. As demonstrated by Ouwens and colleagues, different methods of ascribing day 0 for waiting list cohorts simply result in a small shift of the whole survival curve; they do not change its profile.

Although the approaches suggested by Ouwens and colleagues are valid alternatives to the methods that we used, they would not affect our study conclusions. We believe that the explanation for the modest benefit we found with lung transplantation lies not in our mathematical assumptions or techniques but in the relatively high early mortality after lung transplantation, which is observed on a global scale.¹ Our findings are indeed not unique; analyses of large North American databases have also questioned the notion that lung transplantation results in large survival improvements.^{2,3}

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To close or not to close?

To the Editor:

In their article “Intrapericardial Left Pneumonec-tomy after Induction Chemotherapy: The Risk of Cardiac Herniation,” Baisi and associates¹ reported the use of a polytetrafluoroethylene patch for closure of the pericardium after herniation of the heart and recommended routine use of a patch for closure of pericardium without tension. I agree wholeheartedly that pericardial closure, if done at all, must always be without tension. However, pericardium does not always have to be closed. When lung resection for cancer requires excision of part of

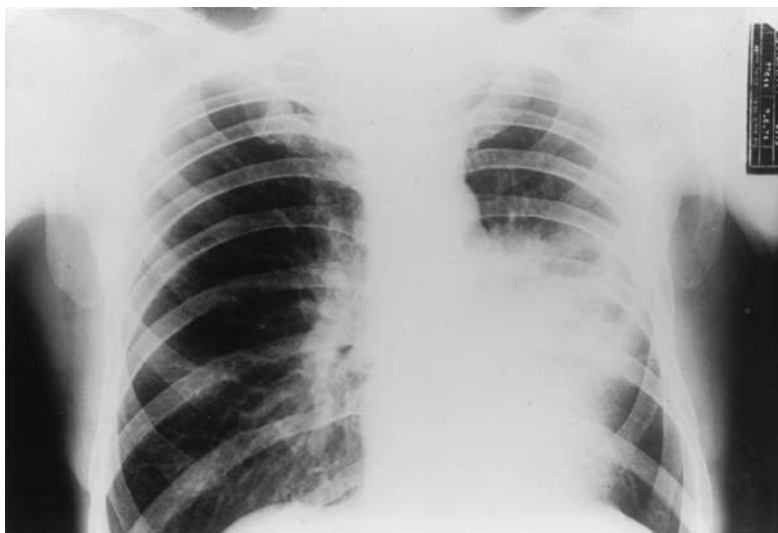


Figure 1. Radiograph of 48-year-old man with squamous cell carcinoma of left lung, penetrating pericardium.



Figure 2. Patient in Figure 1 after pneumonectomy and pericardiectomy. Although area of penetration was small, most pericardium was resected, avoiding herniation.

the pericardium, I prefer not to close the pericardium at all but rather to do a near-total pericardiectomy. Figure 1 shows a radiograph of a 48-year-old man with squamous cell carcinoma of the left lung, penetrating the pericardium. Figure 2 shows the same patient after pneumonectomy and pericardiectomy. Although the area of penetration was small, most of the pericardium was resected, thus avoiding herniation. During the past 22 years (1980-2001), my associates and I have performed 14 such

operations at the Wolfson Medical Center, with no undue effects.

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Reply to the Editor:

Thank you for the opportunity to comment on Weissberg's letter concerning our article. The technique of leaving the pericardium open, enlarging the defect if it is small, is a described option after intrapericardial left pneumonectomy.¹ In fact, on the left side, differently than on the right side, the life-threatening complication is not the herniation of the heart but rather its strangulation through a small pericardial hole. Unfortunately, we have no personal experience to comment on this technique, because we always close any pericardial defect on both the right and left sides, as suggested by other authors.² However, we are a little concerned that "near-total pericardiectomy," as suggested by Weissberg, may cause any injury to the phrenic nerve, the function of which is also important after a pneumonectomy.¹

"To be or not to be," said Hamlet. "That is the question," he continued. "To Close or Not to Close," Weissberg entitled his letter. "This is not a question," we would continue—if you are on the left side and enlarge the defect with regard to the phrenic nerve.

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Which cell dose supports motor neurons' survival?

To the Editor:

We read with interest the article by Perdrizet and associates¹ titled "Preoperative stress conditioning prevents paralysis after