while the cavity was illuminated with a 652-nm laser light and a fluence of 10 J/cm², except for the shielded zone. The large quantity of fluid drained postoperatively gave an indication of the effect of phototherapy on the pleural cavity walls. Slowing of esophageal motor activity was observed in all three cases.

Postoperative electrotherapy was performed in the first two patients on the thoracotomy scar and the pleural cul de sac. No recurrence was observed after 2½ years in one patient (as determined by follow-up CT scan) and after 26 months in the other patient (as determined by follow-up CT scan and negative thoracoscopy findings at 12 months). The third patient developed an infection of the chest wall, which was complicated by an infection of the pneumonectomy cavity. Radiation therapy was impossible to perform. Thoracotomy was performed 9 months later and revealed a late esophageal fistula involving the upper one third of the esophagus (in an unshielded zone). The fistula was closed with a muscle flap. No recurrence was observed in the pleural cavity. Unfortunately, the patient eventually died.

In our limited experience, high-dose preoperative phototherapy therefore appears capable of destroying tumor residues but seems to require major precautions, such as shielding of the incision and the mediastinal organs. Consequently, its place among other techniques seems limited if future series confirm the low local recurrence rate reported by the New York team with the adjunction of high-dose hemithoracic radiation therapy and if the toxicity associated with radiation therapy remains low.

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To the Editor:

We thank Bonnette et al for the valuable comments on our study.1 In their reaction, they addressed the study of Sugarbaker et al,2 using a trimodality approach of extrapleural pneumonectomy combined with chemotherapy and radiotherapy and some aspects of the use of photodynamic therapy (PDT) after resection. In this study, the perioperative mortality is only 3.8% and the median survival is 19 months. Although the survival was not calculated on an intention-to-treat basis, results were better than what is generally achieved with the combination surgery and PDT.1,2,3,4 At least three factors may have been responsible for the difference in treatment outcome. Firstly, the combination surgery, chemotherapy, and radiotherapy may have a better antitumor activity, leading to better tumor control with acceptable toxicity. Secondly, the use of histologic assessment to direct radiotherapy to locations of irradical tumor resection seems an elegant way to treat those locations at risk more effectively. Finally the use of MRI may have improved prediction of resectability, which is considered difficult by many investigators.

In the treatment protocol, with surgery and PDT used by Dr. Bonnette, which is comparable to ours, the esophagus, bronchial and vascular stumps, and pericardium were (partly) shielded from the laser light. This may have the advantage to avoid potential lethal complications (esophaged perforation, bronchial fistula, and myocardial infarction), which occurred in our study. However, organs shielded from light do not receive the additional PDT treatment, and may therefore be at risk for local tumor recurrence. In our opinion, the study of Bonnette et al is of particular importance because it can provide information on the risk of local recurrences.

Improvement of many issues of PDT in combination with surgery, such as patient selection and illumination of the diaphragmatic gutter, still seems possible.1,5 These improvements may better determine the exact place and indication of PDT-mediated therapy in malignant pleural mesothelioma.

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End-of-Life Care: Data Supportive?

To the Editor:

I read with interest the article on ethics in end-of-life care by Kelly et al in CHEST (March 2002).1 Their objective was to...
determine whether the strength of do-not-resuscitate (DNR) recommendations varies with medical specialty and experience. Their conclusion, that the strength of DNR order recommendations varies with different internal medicine specialties and with different levels of experience, is not supported by their data.

None of the seven groups in the study varied significantly in the number of DNR orders recommended, so that all groups demonstrated a similar approach to this end-of-life issue. Only three findings were statistically significant, each in only one group of physicians, and these involved only strength of opinion, not the number of DNR orders recommended. One of the three significant findings, that the more senior house staff recommended DNR more strongly than did the younger interns and junior residents, may reflect increased confidence with increased length of training.

The authors’ use of statistics may have misled them. Statistics, which is concerned with correlations, can be applied to any problem but is not sufficient to show causation, which is the cardinal function of scientific research. The improper use of statistics often results in what has been termed “statistical malpractice.” Findings that are not statistically significant should not be reported as a trend.

Cultural influences are important when dealing with end-of-life issues. The new field of bioethics has created an ethics that places the needs of society and third parties above those of the individual patient. Because of this, the attitudes and behaviors of physicians toward the terminally ill that were reported in the 1970s (references 10 and 11 in the article by Kelly et al) may not be comparable to those from recent years.

Finally, how this study will “help to target educational interventions and . . . ensure effective collaboration with colleagues and communication with patients,” as the authors claim in their conclusions, is not explained.

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To the Editor:

Dr. Arnett’s interest in our publication is appreciated. We again acknowledge that the number of do-not-resuscitate recommendations was not different between subspecialist groups. However, the purpose of the study, as stated in the introduction, was to determine whether the strength of do-not-resuscitate order recommendations, not the absolute number of decisions, varied with medical subspecialty and years of training. Our results did indeed find statistically significant differences in the degree of these convictions. This is important and is of interest to internists in general and chest physicians in particular. We believe that the strength of physicians’ convictions affects their guidance to patients who are making end-of-life decisions.

Dr. Arnett charges statistical malpractice by the confusion of correlation with causation. In our study, we never claim to show any causation. We only report observations from our limited database. Our statistical significance does add greater clarity to these findings by suggesting they are not a result of chance. Additional findings that approached but did not meet significance are so disclosed with p values and statistical methods.

The specific differences that we found among medical subspecialties are consistent with the results of other reports in the medical literature. Dr. Arnett suggests that such references may be out of date (ie, references 10 and 11) but fails to note our citation of this same subspecialty bias in physician actions (reference 9) and in end-of-life publications over the subsequent 20 years (reference 18).

We propose that understanding and respecting subspecialty differences in end-of-life opinions may advance us toward more effective collaboration with colleagues and more effective communication with our patients. We hope that we have shed some light on these differences and invite readers to generate their own hypotheses about causation.

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How To Blow Your Defense

To the Editor:

I was recently consulted by a medical malpractice attorney who asked for assistance in the defense of his client, a board-certified pulmonologist who had been managing a patient with very difficult steroid-dependent asthma. The patient also smoked cigarettes. The man, age 41, had acquired aseptic necrosis in both femoral heads requiring total hip replacements.

The patient had suffered from asthma since the age of 6 months. He had had several hospitalizations for asthma and frequent trips to the emergency department for life-threatening attacks. He had had been appropriately managed by this pulmonologist, with the use of inhaled short-acting as well as long-acting agonists, inhaled corticosteroids, leukotriene modifiers and, at times, bursts of systemic steroids to deal with exacerbations of asthma and/or associated acute and chronic sinusitis.

The major issue in the plaintiff’s strategy was the fact that this pulmonologist had never done spirometry at any time during the management of this patient. Accordingly, the plaintiff argued that the patient never had asthma, which was one of the contentions of the medical expert hired by the plaintiff, also a board-certified pulmonologist. Fortunately, however, numerous measurements of peak flow during exacerbations, which demonstrated increases from low values up to the “personal best” level of approximately 450 to 500 L/min while in remission following corticosteroid treatment had been recorded. But why a simple spirogram was not done by the pulmonologist, as well as other pulmonary function tests, is beyond me. It certainly would have helped in this physician’s defense. Later, an allergist did perform spirometry, which showed severe airway obstruction and air trapping with a normal diffusion test result.

This is the fourth or fifth time I have been asked to defend a