Correspondence

Indications for Computed Tomography after Minor Head Injury

To the Editor: The carefully done and clearly documented study by Haydel and colleagues (July 13 issue)[1] represents important progress in refining criteria for the use of computed tomography (CT) in patients with minor head injury that were originally proposed by me and others.[2-4] However, I believe a word of caution is in order. Their claim that the seven criteria they used had a sensitivity of 100 percent must be viewed with some skepticism. I have encountered exceptions to their rule that these findings always accompany abnormal CT scans in patients with minor head injury. Even though such exceptions must be quite rare, the failure to identify even a single intracranial hematoma would have an enormous impact on outcomes and costs. I would also be reluctant to claim that the use of this approach could substantially reduce health care expenditures until many more patients, and the actual costs of their care, have been studied.

SHERMAN C. STEIN, M.D.
University of Pennsylvania
Philadelphia, PA 19104

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To the Editor: In the study by Haydel and colleagues, the predictive variables in phase 1 were well standardized, but there was no assessment of the interobserver agreement, and some potentially valuable findings were apparently not evaluated: the mechanism of injury and the presence or absence of chronic alcohol abuse, signs of basal skull fracture, and signs of open skull fracture.[1-5] Although the outcome measure of a finding of any acute traumatic intracranial lesion on CT was well defined, it was certainly not a clinically important outcome in terms of patient care. The 909 patients included in the validation cohort in phase 2 made up a relatively large sample, but there were far too few clinically important outcomes in this group for sensitivity to be measured with an acceptably narrow 95 percent confidence interval. Fewer than six patients required surgery, so the 95 percent confidence interval for sensitivity was 54 to 100 percent. Finally, the specificity of the set of criteria used by Haydel et al. is so low that 77 percent of patients who present with a score of 15 on the Glasgow Coma Scale would require CT. This would actually lead to an increase in use of CT at most Canadian and European facilities.

IAN G. STIELL, M.D.
University of Ottawa
Ottawa, ON K1H 8M5, Canada

ANDREAS LAUPACIS, M.D.
Institute for Clinical Evaluative Studies
Toronto, ON M4N 3M5, Canada

GEORGE A. WELLS, PH.D.
University of Ottawa
Ottawa, ON K1H 8M5, Canada

FOR THE CANADIAN CT HEAD AND CERVICAL-SPINE STUDY GROUP

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clinical decision rules in emergency medicine. Ann Emerg Med 1999;33:437-47.
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To the Editor: Although the aim of the study by Haydel et al. was to reduce the number of CT scans obtained in patients with minor head injuries, it may have the opposite effect. If we can safely forgo CT scanning in patients who do not have any of the findings listed in the study, must we obtain CT scans in patients who do have one of these findings? For example, does every patient with "physical evidence of trauma above the clavicles" require CT of the head? This study does not identify the patients for whom CT is indicated. Rather, it identifies a group of patients for whom scanning is not indicated. Determining when scanning is indicated requires consideration of the likelihood of an abnormal finding, the effect of an abnormal finding on the patient's outcome, and the costs and risk of scanning, including the costs of false positive results. A nonzero yield of an abnormal finding is not sufficient to justify the use of an expensive study.

MICHAEL A. KOHN, M.D., M.P.P.
THOMAS B. NEWMAN, M.D., M.P.H.
University of California, San Francisco
San Francisco, CA 94143

Dr. Haydel replies:

To the Editor: Our goal was to derive and validate a set of bedside findings to identify patients with minor head injury who could safely forgo CT of the head. In the United States, patients with any loss of consciousness as a result of trauma routinely undergo CT. We evaluated only patients who had a loss of consciousness as a result of trauma, a normal score on the Glasgow Coma Scale, and normal findings on a brief neurologic examination; therefore, our findings should not be extrapolated to those without a loss of consciousness. Previous studies have shown that 6 to 9 percent of patients with minor head injury have evidence of intracranial injury on CT and that certain bedside findings are 100 percent sensitive in identifying those requiring neurosurgical intervention but are less sensitive in identifying all patients with an abnormal CT scan.[1-3] Patients with even small subdural hematomas or isolated cerebral contusions are typically admitted for observation; therefore, the outcome measure in our study was evidence of intracranial injury on CT, not neurosurgical intervention.

We selected the items evaluated after a review of the literature, especially studies of patients who had a normal score on the Glasgow Coma Scale. A history of chronic alcohol abuse was not a significant variable in any of the studies; the mechanism of injury was significant in one.[4] Signs of basilar or open skull fracture were included as evidence of trauma above the clavicles. As stated in our article, to determine the reproducibility of the clinical data, 50 patients were examined for the presence or absence of any of the seven findings by a second physician (extent of agreement between observers, 92 percent; $\kappa = 0.78$).

In the validation phase, we found that the presence of any of seven findings identified by recursive partitioning (headache, emesis, an age of more than 60 years, drug or alcohol intoxication, seizure, short-term memory deficits, or physical evidence of trauma above the clavicles) was 100 percent sensitive in identifying patients with intracranial injury on CT scanning. Applying the seven-item guideline to our group of patients would have reduced the need for CT by 22 percent without failing to identify any patients with an abnormal CT scan.

In countries where CT is not readily available and patients with minor head injury typically do not undergo CT scanning, the application of these guidelines may increase the use of CT, but it is unlikely that patients with evidence of intracranial injury that is detectable on CT scanning would fail to be identified. As we stated in our conclusions, "the lower limit of the confidence interval [95 percent] indicates the possibility of missing an intracranial lesion that would be detected by CT scanning." We look forward to the validation and further refinement of these findings at other centers.

MICELLE J. HAYDEL, M.D.
Louisiana State University Health Science Center at New Orleans
New Orleans, LA 70112

1. Miller EC, Derlet RW, Kinser D. Minor head trauma: is computed tomography always necessary? Ann Emerg Med 1996;27:290-4.
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Staging of Non–Small-Cell Lung Cancer with Positron-Emission Tomography

To the Editor: In their study of the value of positron-emission tomography (PET) in the preoperative staging of non–small-cell lung cancer, Pieterman et al. (July 27 issue)[1] found that PET was more sensitive and specific than computed tomography (CT) in detecting mediastinal metastases and that PET identified distant metastases that had not been found by standard methods in 11 of 102 patients. Of 29 "hot spots" potentially representing distant metastases, 9 were considered to be false positive results; the evaluation of such false positive findings may result in complications, increased costs, and delayed treatment.

Experience with the use of imaging tests to identify distant metastasis in patients with lung cancer has demonstrated that accuracy depends on the pretest probability of metastatic disease. A relatively simple and inexpensive clinical evaluation consisting of a history taking, physical examination, and laboratory examinations has a high negative predictive value (92 to 97 percent) with respect to the identification of metastatic disease on CT or radionuclide scanning.[2] In the light of these findings, the American Thoracic Society (ATS) and the European Respiratory Society have recommended that an extensive search for metastatic disease not be carried out in patients with negative findings on clinical evaluation[3]: the absence of weight loss, bone pain, and neurologic symptoms; the absence of neurologic findings, lymphadenopathy, hoarseness, the superior vena cava syndrome, hepatomegaly, a soft-tissue mass, and bone ten-

derness; and the finding of a normal hematocrit, liver function, and calcium levels. Whether PET scanning will be useful in detecting distant metastases in patients with negative findings on clinical evaluation is an important issue that was not addressed in the study by Pieterman et al., nor were the clinical criteria the investigators used to identify potential instances of metastatic disease defined.

YORK E. MILLER, M.D.
Denver Veterans Affairs Medical Center
Denver, CO 80220-3803

1. Pieterman RM, van Putten JWG, Meuzelaar JJ, et al. Preoperative staging of non–small-cell lung cancer with positron-emission tomography. N Engl J Med 2000;343:254-61.
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To the Editor: We disagree with the system of mediastinal lymph-node mapping used by Pieterman et al. in their assessment of the value of PET in the staging of non–small-cell lung cancer. Their classification of lymph-node levels is misleading, because it alters the universally accepted approach described by Naruke et al.[1] and accepted by the ATS.[2] As defined by Pieterman et al., level 1 is pretracheal and is equivalent to level 3 in the ATS system. Moreover, the numbering in general is confusing because paratracheal lymph-node areas recognized by the ATS system are combined in new areas. In addition, the subaortic and paraaortic areas included in level 4 by Pieterman et al. correspond to a level of 5 in the ATS system, whereas in the ATS system, level 4 is divided into level 4R, which includes the right-sided lower paratracheal nodes between the cephalic border of the azygos vein and the intersection of the caudal margin of the brachiocephalic artery with the right side of the trachea, and level 4L, which includes the left-sided lower paratracheal nodes between the top of the aortic arch and the level of the carina, medial to the ligamentum arteriosum.

Furthermore, Pieterman et al. divide the lower mediastinum differently; they consider the paraesophageal area and the pulmonary ligament (equivalent to ATS levels 8 and 9, respectively) together and instead differentiate between the left side (their level 6) and the right side (their level 7) of this region. Instead of including the left side of the paraesophageal area and pulmonary ligament, level 6 of the ATS system includes the nodes anterior to the ascending aorta or the innominate artery, and level 7 includes subcarinal nodes arising caudal to the carina of the trachea.

The extrapulmonary locations of the intrathoracic lymph nodes in humans were described by Rouvière[3] in 1938, and the intrapulmonary lymphatic anatomy and its interconnecting network were summarized in 1952 by Borrie,[4] who elucidated the patterns of dissemination of lung cancer in the intrapulmonary lymphatics of resected specimens. Since then, the system of lymph-node mapping proposed by Naruke et al.[1] and accepted by the ATS[2] has guided the techniques of lymph-node sampling and dissection. If Pieterman et al. want to propose a new system of lymph-node mapping, they should take care to eliminate the inconsistencies I have pointed out, so that their system will conform to the generally accepted classification. Otherwise, their system will only lead to unnecessary confusion on the part of thoracic surgeons, pathologists, and oncologists.

RAFAEL ROSELL, M.D.
Hospital Germans Trias i Pujol
08916 Badalona, Spain

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To the Editor: Pieterman et al. demonstrated the excellent sensitivity of PET with ^{18}F-fluorodeoxyglucose for the detection of metastases of non–small-cell lung cancer. In their editorial, Berlangieri and Scott[1] ascribe the problems of sensitivity in detecting small pulmonary lesions to respiratory motion and the limits of PET resolution. In addition, we want to stress that the type of tumor may have a role. The results of PET with ^{18}F-fluorodeoxyglucose may be falsely negative in patients with bronchioloalveolar carcinomas and carcinoid tumors.[2,3] Consequently, a negative result on PET with ^{18}F-fluorodeoxyglucose may also provide information about the nature of lung cancer, especially lung tumors that induce ectopic secretion of corticotropin. In patients with this condition, a negative result strongly suggests a carcinoid tumor rather than a small-cell lung carcinoma as the cause of ectopic secretion of corticotropin.

JULIAN E. DONCKIER, M.D., PH.D.
VÉRONIQUE ROELANTS, M.D.
JEAN-MICHEL POCHET, M.D.
Université Catholique de Louvain at Mont-Godinne
B-5530 Yvoir, Belgium

1. Berlangieri SU, Scott AM. Metabolic staging of lung cancer. N Engl J Med 2000;343:290-2.
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Dr. Groen replies:

To the Editor: Dr. Miller asks which clinical criteria we used to identify potential instances of metastatic disease. At the time of preoperative staging and during follow-up after thoracotomy, patients were clinically evaluated for symptoms and signs suggestive of metastatic disease in the way Dr. Miller suggested. I agree that the pretest probability of metastatic disease determines the accuracy of PET. Indeed, not all hot spots identified on PET were diagnosed as malignant disease. In the colon, hot spots and, more often, linear areas of increased uptake of ^{18}F-fluorodeoxyglucose on PET were diagnosed as Crohn's disease or ulcerative colitis in patients with only marginal symptoms.

Although false positive results of PET require further evaluation, in our study, PET identified distant metastases that had not been found with traditional methods in 11 percent

of our patients. In these patients, thoracotomy could have been avoided. Randomized studies of PET are being performed in the Netherlands to determine whether PET is cost effective with regard to the number of diagnostic tests and to what extent the use of PET as an early diagnostic test for lung cancer obviates the need for invasive procedures.

Dr. Rosell disagreed with the system of classification of mediastinal lymph nodes that we used. By our use of this system we did not mean to imply that the ATS system should be changed; rather, we wanted to compare the results of various imaging techniques with those of histopathological examinations of dissected mediastinal lymph nodes that were labeled according to the classification of Mountain and Dresler.[1] Because of the limited anatomical resolution of PET, the use of a broader lymph-node category was necessary for an adequate comparison of PET, CT, and surgical lymph-node mapping. The numbering of the lymph-node categories has no relation to that of the ATS system.

I agree with Donckier et al. that not all lung tumors have increased uptake of ^{18}F-fluorodeoxyglucose. Recently, we found no pulmonary hot spots at all in four patients with bronchioloalveolar carcinoma and one patient with bronchial adenocarcinoma.

HARRY J.M. GROEN, M.D., PH.D.
University Hospital Groningen
9700 RB Groningen, the Netherlands

1. Mountain CF, Dresler CM. Regional lymph node classification for lung cancer staging. Chest 1997;111:1718-23.

Medical Mystery: The Answer

To the Editor: The medical mystery in the October 5 issue[1] involved a 19-year-old man, shown here in Figure 1, who had numerous dome-shaped elevations on the surface of the iris, or Lisch nodules. Lisch nodules are melanocytic hamartomas that are either yellow or brown. They are visible on inspection, pathognomonic of neurofibromatosis type 1, and do not cause symptoms. The incidence of Lisch nodules among patients with neurofibromatosis type 1 increases with age: at the age of 5 years only 22 percent have Lisch nodules, whereas at the age of 20 years 100 percent have them.[2] Therefore, older patients who do not have Lisch nodules are also unlikely to have neurofibromatosis type 1.

GERHARD KURLEMANN, M.D.
OTFRIED DEBUS, M.D.
University of Münster
D-48129 Münster, Germany

1. Kurlemann G, Debus O. A medical mystery. N Engl J Med 2000;343:1019.
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Editor's note: We received 551 responses to this medical mystery. About 26 percent of the respondents said that the photographs of the eyes showed Lisch nodules and that the man had neurofibromatosis. The most common response, suggested by about 43 percent of respondents, was that the photographs showed Kayser–Fleischer rings and that the diagnosis was Wilson's disease or some other disorder of the liver. An additional 1 percent suggested that the correct answer was a sunflower cataract, indicating the presence of Wilson's disease. The other responses offered about 50 different explanations, including hypercholesterolemia; Koeppe nodules, indicating sarcoidosis; reflection of a lamp; rubeosis iridis, indicating diabetes mellitus; Waardenburg's syndrome; and Williams's syndrome.

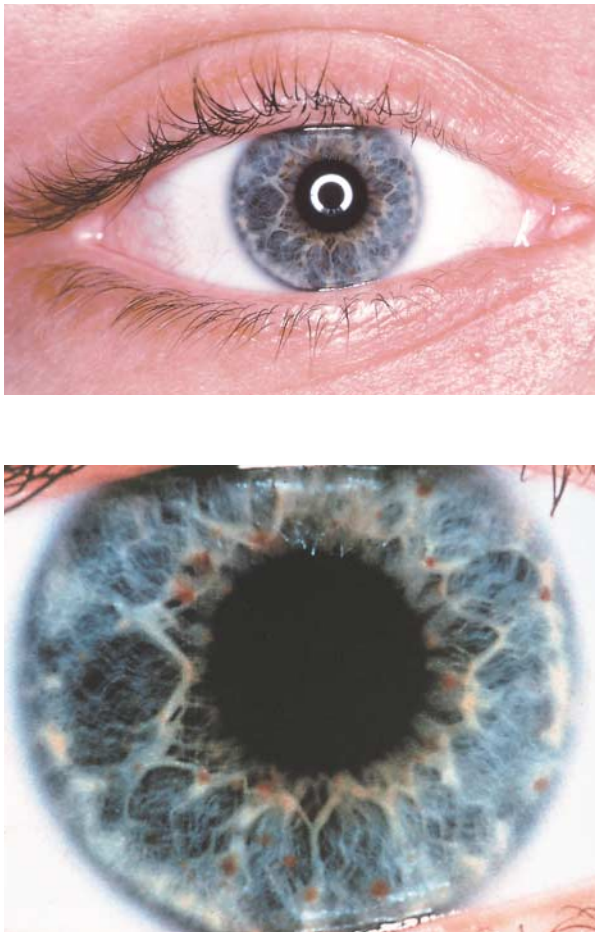

Figure 1. Photographs of the Iris of a 19-Year-Old Man with Neurofibromatosis Type 1, Showing Multiple Lisch Nodules.

The Case for More U.S. Medical Students

To the Editor: In his thought-provoking Sounding Board article (July 20 issue)[1] Mullan suggests that the number of U.S. medical schools and medical students be increased. By any combination of standard measures of health in developed countries, the United States ranks at or near the bottom of the list.[2] Throwing more homegrown physicians and expensive medical schools into this malfunctioning system will have a miniscule effect on our overall health. We already far exceed any other society in terms of the cost of health care and the number of physicians per capita. Un-

fortunately, despite the advent of managed care, the market for new physicians in the United States remains almost insatiable.

The United States obviously benefits in many ways from the immigration of foreign-trained physicians, most of whom are highly motivated and outstanding. Those who stay in the United States or eventually return here add to our enviable reputation as an international medical melting pot. Many pursue careers in academic research that would be unavailable to them in their countries of origin. Those who return home often become medical leaders. I have little guilt about this so-called brain drain; frequently, the exchange benefits all parties.

FERRIS M. HALL, M.D.
Beth Israel Deaconess Medical Center
Boston, MA 02215

1. Mullan F. The case for more U.S. medical students. N Engl J Med 2000; 343:213-7.
2. Starfield B. Is US health really the best in the world? JAMA 2000;284: 483-5.

To the Editor: I was astonished by Mullan's call to increase the number of medical students. The fact that the number of entry-level residency positions substantially exceeds the number of graduates of U.S. medical schools is not evidence of a mismatch between training capacity and the health needs of the population. Rather, it reflects two other phenomena: the reliance of teaching hospitals (at substantial expense to the public) on residents as cheap labor to staff inpatient services and the ability of the U.S. health care system to absorb additional practicing physicians. Despite the fact that many bright young people cannot get into medical school, Mullan's proposal for expanding their career opportunities carries a hefty price tag. Given issues such as the growing number of uninsured persons, the inadequate health care infrastructure in our inner cities and rural areas, and the high cost and limited benefit of expanding medical education, this item should be low on the list of national health priorities.

ANNE L. SCHWARTZ, PH.D.
Grantmakers in Health
Washington, DC 20036

To the Editor: There are a number of problems with Mullan's proposal. First, increasing the number of U.S. medical students would substantially increase costs and further decrease the pool of teachers and patients available for clinical training. Who would pay these extra costs, especially in these times when there is serious concern about the overall cost of health care and graduate medical education? In contrast, the cost of training graduates of foreign medical schools is borne by other countries.

Second, it is very difficult to get graduates of U.S. medical schools to enter residency programs or practice in underserved areas. Loan-forgiveness programs and other maneuvers clearly have not attracted substantial numbers of these graduates to apply to training programs or to practice in the inner city. In fact, recently, graduates of U.S. medical schools have been unable either to enter or to get paid by these programs. Therefore, graduates of foreign medical schools remain virtually the sole source of residents as well as practicing physicians in inner-city hospitals, facilities that are vital for the provision of health care to the poor.

Third, since there are many more applicants who are graduates of foreign medical schools than there are positions, programs that accept these graduates can recruit excellent, often superior, physicians, rather than the graduates of U.S. medical schools who are least able to compete. This point is reflected by the fact that scores on National In-Training examinations and rates of passage of board examinations in inner-city residency programs rival or even surpass those of residency programs in which graduates of U.S. medical schools predominate. In the face of such competition, it is not clear that expanding the pool of graduates of U.S. medical schools without restricting visas will lead to the placement of all such graduates in residency programs; some will be displaced by more talented graduates of foreign medical schools.

Fourth, each year, in many inner-city programs, a number of graduating residents who were educated at foreign medical schools go into practice either at their hospital or in the immediate underserved area. Others go to underserved areas outside the city. It is unclear whether graduates of U.S. medical schools will ever fill these slots.

GERALD POSNER, M.D.
ERIC A. JAFFE, M.D.
Interfaith Medical Center
Brooklyn, NY 11238

To the Editor: A very important factor that Mullan does not consider is that physicians have no monopoly on the provision of medical care in the United States. Physicians' assistants and nurse practitioners are in direct competition with doctors for patients. As their numbers continue to grow and health care facilities continue to employ them, it will lessen the demand for new physicians, especially those in primary care.

ASHOK VAGHJIMAL, M.D.
3201 Hargrove Rd. E.
Tuscaloosa, AL 35405

Dr. Mullan replies:

To the Editor: One can certainly agree with the concern of Hall about the cost of health care in the United States and the implied role of physicians in it. However, training more U.S. medical students would not increase the number of physicians in practice, since it is the number of residents rather than the number of medical students that determines the number of physicians who enter practice. Several European nations, in fact, surpass the United States in terms of the number of physicians per capita, and our physician-to-population ratio is stabilizing in the range of 270 per 100,000.[1]

Schwartz points out, quite accurately, that Medicare funds for graduate medical education underwrite the costs of residency training. The question that might fairly be asked is why a quarter of the trainees who benefit from these expenditures (approximately $6 billion in 1999) are graduates of foreign medical schools when thousands of young

people in the United States with excellent qualifications are being denied opportunities to study medicine and benefit from this support.

Schwartz raises an important related matter by citing the "hefty price tag" associated with the cost of medical school education. Accounting practices related to the cost of medical education are so variable that little can be said with specificity about the real cost of educating a medical student.[2] However, the national experiment that has been performed over the past 20 years, in which the osteopathic medical school community has succeeded in opening some 12 new medical schools and filling their classes on a tuition-driven basis, suggests that the costs of expanding opportunity in medicine are not prohibitive.

Posner and Jaffe are incorrect in stating that loan-forgiveness programs and other maneuvers have not attracted substantial numbers of graduates of U.S. medical schools to practice in the inner city. In fact, both the scholarship program and the loan-repayment program of the National Health Service Corps are substantially oversubscribed and underfunded. The issue is not the absence of U.S.-trained physicians who are willing to work in poor communities but the lack of political will to fully fund incentive programs for service in these communities. The growing willingness of the medical community and of U.S. policymakers to rely on medical education systems from abroad to train people to serve the poor of the United States is simply an abdication of responsibility.

Training more U.S. medical students would increase opportunities for young people in this country — especially members of minority groups — and would foster self-sufficiency in our system, so that we could continue to rely on the public budgets and medical schools of other countries to train a quarter of our physician workforce.

FITZHUGH MULLAN, M.D.
Health Affairs
Bethesda, MD 20814

1. Organization for Economic Co-operation and Development (OECD). OECD health data 2000: a comparative analysis of 29 countries. II. Health care resources: health employment (CD-ROM).
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The Rise and Fall of the Futility Movement

To the Editor: For evidence of a rise and fall of the futility movement, Helft et al. (July 27 issue)[1] use a parochial if not dubious source of empirical data — the number of citations found in a Medline search of scholarly articles. I suggest they look at health care organizations around the country that are actively developing futility policies. Although I am not aware of any systematic survey, I am in frequent contact with institutions engaged in this activity. A recent statewide conference in California critiqued the policies of 26 hospitals. In the majority of these, medical futility was specifically defined, and the definitions were remarkably similar.[2] As far as I can tell, the only fall in the futility movement has been from the scholarly towers to the street level, where these decisions are being made every day.

Helft et al. use the term "futile care" rather than the more precise "futile treatment." Language matters when one is trying to reassure families of one's commitment to ongoing and compassionate attention even if such attention does not involve aggressive interventions.[3] Many institutions specify in their policies that, although a particular treatment may be futile, care is never futile.

Helft et al. place their hopes in a process-based approach that arrives at a compromise through conflict resolution. However, the use of such an approach in the absence of underlying principles and definitions may lead to ethically problematic decision making, not by reference to the most appropriate medical standards, but more capriciously, through the demands of the most powerful, uncompromising, and threatening parties.[2]

LAWRENCE J. SCHNEIDERMAN, M.D.
University of California, San Diego
La Jolla, CA 92093-0633

1. Helft PR, Siegler M, Lantos J. The rise and fall of the futility movement. N Engl J Med 2000;343:293-6.
2. Schneiderman LJ, Capron AM. How can hospital futility policies contribute to establishing standards of practice? Camb Q Healthc Ethics 2000; 9:524-31.
3. Schneiderman LJ, Faber-Langendoen K, Jecker NS. Beyond futility to an ethic of care. Am J Med 1994;96:110-4.

To the Editor: Contrary to assertions that there has been a decline in the futility movement and that futile treatment cannot be defined, many hospitals and other health care organizations have developed policies and procedures covering this area, and more will probably do so.[1] Such policies can include a careful and inclusive review of any putative case of nonbeneficial treatment, which would include talking to patients and their families about the treatment, as Helft et al. suggest, and the use of second opinions, consultation with specialists, and review by an institutional ethics committee. In other words, the process is about as inclusive as is practicable.

During the lengthy process of developing such broad-based guidelines in northern California, there was a strong consensus that such policies were needed but that fears about liability precluded their implementation.[2] Yet, cases involving determinations of futility have more often been marked by careful conflict resolution than by legal action.

STEVE HEILIG, M.P.H.
San Francisco Medical Society
San Francisco, CA 94109

1. Schneiderman LJ, Capron AM. How can hospital futility policies contribute to establishing standards of practice? Camb Q Healthc Ethics 2000; 9:524-31.
2. Bay Area Network of Ethics Committees (BANEC) Nonbeneficial Treatment Working Group. Nonbeneficial or futile medical treatment: conflict resolution guidelines for the San Francisco Bay Area. West J Med 1999;170:287-90.

To the Editor: We agree with Helft et al. that "futile care in hospitals is still very much an issue." However, it is no longer true that "doctors today are no more empowered to declare a treatment futile unilaterally than they were 15 years ago."

The Texas Advance Directives Act of 1999[1] established an extrajudicial mechanism of due process that allows physicians to stop futile treatments without fear of civil or criminal liability if the process is followed. If a physician in

Texas concludes that continuing life-sustaining treatment for a terminally or irreversibly ill patient is futile, but the patient's family demands that such treatment be continued, a five-step process can be invoked.

The family is given 48 hours' notice of and an opportunity to participate in an ethics-review process with the facility's ethics committee. More often than not, this review process resolves any disagreement. However, if at the end of the review process, the committee agrees with the treatment team that continuing life-sustaining treatment is futile and the family still insists on continuing aggressive treatment, then additional steps may be taken. Life-sustaining treatments are continued for 10 days while attempts are made to transfer the patient to a facility willing to provide the treatment that has been found to be futile. If no alternative provider is found, then treatment other than comfort care may be stopped without civil or criminal liability unless the family chooses to seek an extension from the state courts to continue the search for an alternative provider. The courts are instructed by the law to grant an extension of the 10-day period only if there is reasonable evidence that an alternative provider can be found during this extension.

Our initial experience as ethics consultants to large tertiary care hospitals has been quite favorable. The process places both temporal and conceptual boundaries on the concept of futility. When both the treatment team and the ethics committee come to the conclusion that further treatment is futile, it is extraordinarily unlikely that another facility will accept the patient. This point helps to persuade the family of the appropriateness of switching to comfort care alone. Placing a process familiar to many ethics committees within the context of the law has changed the tenor of the conversation between providers and patients' families for the better.

ROBERT L. FINE, M.D.
Baylor Health Care System
Dallas, TX 75204

THOMAS W. MAYO, J.D.
Southern Methodist University School of Law
Dallas, TX 75205

1. Texas Advance Directives Act of 1999. Texas Health and Safety Code ch. 166 (Vernon Supp. 1999). (See http://www.capitol.state.tx.us/statutes/he/he016600.html#he001.166.001.)

To the Editor: It is certainly true, as stated by Helft et al., that the medical profession has not agreed on a clear set of standards to define futile care. However, even if this were done it is doubtful that courts and the public would feel comfortable allowing physicians to deny care — no matter how futile — unilaterally.

Since the end of the period reviewed by Helft et al., far broader changes in the public's perception of physicians' authority have taken place. With medical information much more readily available through the Internet and other media sources, physicians' authority and, similarly, the respect with which we are viewed by the public may have less and less to do with our clinical judgment and epidemiologic skills and more to do with our ability to participate in medical decision making as one voice among many. Although some may mourn the day when physicians were held in such esteem that they could consider unilaterally denying care, such regret misses the point of larger social changes that go to the very heart of the way in which physicians' authority is constructed. The better question is not, "What do doctors think is appropriate," but simply, "How can doctors share their wisdom?"

ERIC R. WOLD, M.D.
Weill Cornell College of Medicine
New York, NY 10021

To the Editor: Medical futility has a very long tradition, tracing back to Sumerian and Egyptian healers, who were capable of deciding and empowered to decide whether to treat or to avoid treating. Moreover, in classical times it was generally considered prudent and moral for physicians to avoid treating those who were hopelessly ill; to do otherwise might have been criticized as fraudulent. The writers of the Hippocratic Corpus persistently advised physicians to refuse to undertake cases "in which the disease has already won the mastery, knowing that everything is not possible to medicine."[1] The same attitude was supported by philosophers such as Plato, who had a particular interest in defining the limits of medicine. Plato referred to the fate of Aesculapius, who, having successfully dared to exceed these limits, was killed with a thunderbolt by Zeus.[2] Physicians could declare a treatment futile unilaterally, regardless of their patients' objections. Quite interestingly, this attitude continued even after Christianity became predominant. Thus, when the physicians of the 12th-century Eastern Roman Emperor Alexius I Comnenus concluded that his disease was incurable, it was completely acceptable to let him die alone.[3]

EMMANOUIL GALANAKIS, M.D., PH.D.
University of Crete
71409 Heraklion, Greece

1. Hippocrates. Hippocratic writings: edited with an introduction by G.E.R. Lloyd. Lloyd GER, ed. Chadwick J, Mann WN, trans. London: Penguin Books, 1983.
2. Plato. The republic. New ed. Lindsay AD, trans. London: Dent, 1976.
3. Lascaratos J, Poulakou-Rebelakou E, Marketos S. Abandonment of terminally ill patients in the Byzantine era: an ancient tradition? J Med Ethics 1999;25:254-8.

The authors reply:

To the Editor: In a sense, the debate about futility comes down to a question of whether process is preferable to principles. Schneiderman prefers principles, since in his view, process is simply a way of veiling domination by the powerful. Schneiderman also appears to believe that principle and definition provide tools for managing "the demands of the most powerful, uncompromising, and threatening parties," by which we assume he means the "capricious" desires of patients and their families. Such anarchy need not follow from a process that allows patients to participate. Rubin, in her comprehensive review of the futility debate, offers principles for fair process.[1] According to Rubin, it must involve "genuine conversation using complete sentences, moral persuasion, and transparent disclosure." With such safeguards, a process for resolving futility dilemmas may

offer a fairer solution than the application of principles not shared by all participants in the dialogue.

To our mind, the elaborate policies described by Fine and Mayo as part of the Texas Advance Directives Act of 1999 fall somewhere between unilateral decision making by physicians and the sort of process we imagine to be fairer and preferable. The family has nearly two weeks of process, impartial outside review by both ethics committees and courts, and the chance to find an alternative provider. Our hospital has a similar, though less cumbersome, policy. It is never invoked.

Physicians today often offer their opinion that further treatment is futile. They may even move to discontinue treatment on that basis before patients or their families would wish to. They may write policies to justify these actions. Those policies may be incorporated into state laws. However, the policies will almost always include ultimate recourse to outside judicial review. To date, courts have been extremely reluctant to override a family's request for continued treatment. Such cases of irresolvable disagreement are both rare and symbolic.

PAUL R. HELFT, M.D.
MARK SIEGLER, M.D.
JOHN LANTOS, M.D.
University of Chicago
Chicago, IL 60637-1470

1. Rubin S. When doctors say no: the battleground of medical futility. Bloomington: Indiana University Press, 1988.

The Red Eye

To the Editor: In his review of the red eye (Aug. 3 issue),[1] Leibowitz makes one point that I would like to challenge. He states that the discharge that characterizes viral conjunctivitis is watery and that the discharge that characterizes bacterial conjunctivitis is purulent or mucopurulent (what is the difference?) and mats the lid on awakening. It seems to me that Leibowitz perpetuates a well-entrenched myth that has little, if any, scientific basis.

It is hard to imagine why a viral inflammatory process should differ in character from a bacterial inflammatory process. Certainly, in cases of pharyngitis or cases of bronchitis or pneumonia, it is not possible to examine either a pharynx or gross bronchial secretions and make the distinction between viral and bacterial causes. Why should the conjunctiva have this special property? It seems to me that the primary care practitioner cannot — and should not — rely on the gross appearance of conjunctival secretions to decide whether to prescribe antibacterial treatment, either topical or systemic.

MICHAEL K. REES, M.D.
1415 Beacon St.
Brookline, MA 02446

1. Leibowitz HM. The red eye. N Engl J Med 2000;343:345-51.

To the Editor: An infrequent but potentially lethal cause of subconjunctival hemorrhage is near-asphyxia. Too often I have seen this diagnosis missed by primary care physicians and have later performed the autopsy, after there was repeated injury.

GEORGE R. NICHOLS II, M.D.
2307 Greene Way
Louisville, KY 40220

To the Editor: The excellent review by Leibowitz on the red eye might have included another treatment for inclusion conjunctivitis (or trachoma). Azithromycin, taken in a single dose, has the advantage of high compliance as well as rates of clinical cure similar to those of tetracycline and erythromycin.

SHEILA WEST, PH.D.
Johns Hopkins University
Baltimore, MD 21287-9019

Dr. Leibowitz replies:

To the Editor: Strangulation, as a cause of sudden, severe venous congestion of the head, can certainly cause subconjunctival hemorrhage, but I did not cover any of the many forms of asphyxia in my review, since it is not generally encountered by ophthalmologists. I am surprised by the diagnostic importance that Nichols attributes to this sign.

West's suggestion is an important one. Because of space limitations, I did not discuss azithromycin for the treatment of ocular chlamydial infection in my article. Though a relatively new therapeutic approach, a single oral dose of azithromycin does appear to be effective in children with trachoma in areas where this disease is hyperendemic. However, I know of no studies that have demonstrated the effectiveness of this regimen for the treatment of chlamydial inclusion conjunctivitis in adolescents and adults. Since a single dose of azithromycin eradicates *Chlamydia trachomatis* from the urogenital tract, one might anticipate that it would be effective against the ocular infection. If it is effective, its use would indeed be advantageous and would reduce poor compliance.

With regard to Rees's challenge, I stand by my statement that "a purulent discharge generally suggests a bacterial infection, but otherwise, the nature of the discharge is not clinically useful in determining the cause." In my article I also describe the discharge that generally accompanies acute bacterial conjunctivitis as purulent or mucopurulent (the difference is in the amount of mucus mixed with the collection of leukocytes and cellular debris) and the discharge that generally accompanies acute viral conjunctivitis as watery. These are accurate and useful characterizations. The primary care practitioner can choose to ignore the appearance of conjunctival secretions as myth, but would do so at potential peril to the patient.

HOWARD M. LEIBOWITZ, M.D.
Boston University School of Medicine
Boston, MA 02118-2394

The Breasts of "Night": Michelangelo as Oncologist

To the Editor: The unusual appearance of the left breast of Michelangelo's "Night," a marble statue of a female fig-

ure, has often been mentioned in the literature on Michelangelo's Medici Chapel (Church of San Lorenzo, Florence, Italy). One of us, an oncologist, found three abnormalities associated with locally advanced cancer in the left breast. There is an obvious, large bulge to the breast contour medial to the nipple; a swollen nipple–areola complex; and an area of skin retraction just lateral to the nipple (Fig. 1). These features indicate a tumor just medial to the nipple, involving either the nipple itself or the lymphatics just deep to the nipple and causing tethering and retraction of the skin on the opposite side. These findings do not appear in the right breast of "Night" or in "Dawn," another female figure in the Medici Chapel, or in the many other depictions of women in works by Michelangelo.

Modern scholars agree that the unusual appearance of the breast of "Night" is intentional and not due to an error or its slightly unfinished state. Art historians and even plastic surgeons have argued that it reflects the artist's supposed lack of interest in or familiarity with the nude female figure.[1-4] We suggest that Michelangelo carefully inspected a woman with advanced breast cancer and accurately reproduced the physical signs in stone. Even if he did not see the disease in a model, he could have studied the corpse of a woman; moreover, autopsies were legal at that time. Given that Michelangelo depicted a lump in only one breast, he presumably recognized this as an anomaly. Many doctors in his day could probably diagnose this condition in a woman. Historians of breast cancer agree that the disease and its treatment were discussed, often at length, and described as cancer by the most famous medical authorities of antiquity — Hippocrates, Celsus, and Galen — and by several prominent medieval authors, including Avicenna and Rolando da Parma.[5-7]

For these reasons, there is a strong possibility that Michelangelo intentionally showed a woman with disease and that he may have known that the illness was cancer. If Michelangelo indeed depicted "Night" as having a consuming disease, this would complement the imagery in the Medici Chapel, help us understand his study of the female body, and add to our knowledge of the depiction and allegorical associations of illness in the Renaissance.

James J. Stark, M.D.
Cancer Treatment Centers of America
Portsmouth, VA 23704

Jonathan Katz Nelson, Ph.D.
New York University
50139 Florence, Italy

1. Hibbard H. Michelangelo. 2nd ed. New York: Harper & Row, 1985:191.
2. Hayes H Jr. A question on a Michelangelo sculpture. Plast Reconstr Surg 1990;87:192.
3. Mangus D. Michelangelo and the female breast. Plast Reconstr Surg 1991;88:374-5.
4. Wise DM. Michelangelo and the female breast. Plast Reconstr Surg 1991;88:375.
5. Obinu GM. Il cancro dal punto di vista storico. Genoa, Italy: Don Bosco, 1961.
6. De Moulin D. A short history of breast cancer. Boston: Martinus Nijhoff, 1983.
7. Pluchinotta AM. Iconographia senologica: l'immagine del seno nella storia, nella cultura e nell'arte. Padua, Italy: La Garangola, 1985.

Figure 1. Michelangelo's "Night."
Photograph courtesy of the Kunsthistorisches Institut Florenz, Florence, Italy.