conclude that bronchoscopy is unnecessary in young patients, many more young patients should have been included in study by Weaver et al.

Philip Harper, M.D.,
Johns Hopkins University,
Baltimore

REFERENCE
1 Lusted L. Introduction to medical decision making. Springfield, Charles C Thomas, 1968

To the Editor:
We appreciate these thoughtful comments on our paper. The indications for bronchoscopy suggested by our analysis should be considered a preliminary answer to the common clinical problem of hemoptysis and should be evaluated in the light of experience at other institutions. The Drs. Cole seem to be advocating more extensive use of bronchoscopy than we favor. Their position is certainly defensible and has many sympathizers; however, our article was an attempt to advance the analysis of this important issue. We are familiar with all the points raised in favor of the Coles' position, but we do not think that they warrant changing our conclusions.

Dr. Harper apparently assumes that likelihood ratios provide the best way to compare various possible indications for bronchoscopy. We believe that other methods may be more useful sometimes. Although likelihood ratios have been employed by several investigators seeking to improve medical decision-making, they are not completely satisfactory. First, the indications for a procedure chosen through use of these ratios may be contrary to the implicit preferences of clinicians. For example, Dr. Harper favors anemia over advanced age as a criterion for performing bronchoscopy; yet, in our experience, age is mentioned more frequently when physicians are defending their decision to perform this procedure.

Second, direct calculation of the likelihood ratios from the data in our paper may be illegitimate because our investigation was not designed to estimate the relevant variables. Forty of the 110 patients with hemoptysis did not undergo bronchoscopy. This occurred sometimes because the attending physician attributed the bleeding to some recognized benign condition. If some of these patients had had bronchoscopy, the likelihood ratios would have been different, and it is conceivable that the differences could have altered the relative rankings of age, hematocrit, etc. The two-step decision procedure emphasizing false negatives that was used in our analysis might be less affected by this problem.

Norman Sollday, M.D., F.C.C.P.
Northwestern University Medical School, Chicago

To the Editor:
One of the definitions of perspective is "the interrelation in which a subject or its parts are mentally viewed." It is apparent that the Drs. Cole, working as they do in a department of thoracic surgery, were not viewing the group of patients to whom I was referring when I stated in my editorial2 that patients with hemoptysis, who had strong clinical evidence of non-neoplastic lung disease, did not need bronchoscopy.

As the Coles state, bronchoscopy will generally be advisable, to exclude the development of bronchogenic carcinoma, in patients with hemoptysis who have previously had tuberculosis, other granulomatous disease or some other chronic problem such as pulmonary cystic disease. One disease does not protect against another.

But surely the Drs. Cole would not have us bronchoscope every acutely ill patient with hemoptysis admitted to a medical service. Such patients often have bleeding due to pneumonia, pulmonary infarction, purulent lung abscess or cavitary granulomatous disease. Nor do I think they would have us bronchoscope every patient who expectorates a little blood in the morning after a nocturnal epistaxis from a demonstrated bleeding site. These are the patients, comprising a group of 40 individuals with hemoptysis, who were not bronchoscooped by Weaver et al. These are also the patients referred to in the early but still pertinent report by Moersch4 and to whom I was referring in my editorial.

I suggest that the disagreement between the Drs. Cole and me is more apparent than real—and is all just a matter of perspective!

Gordon L. Snider, M.D., F.C.C.P.
Boston University School of Medicine
Veterans Administration Medical Center
Boston

REFERENCES
2 Snider GL. When not to use the bronroscope for hemoptysis. Chest 1979; 76:1-2
3 Weaver Lj, Sollday N, Cugell DW. Selection of patients with hemoptysis for fiberoptic bronchoscopy. Chest 1979; 76:7-10
4 Moersch HJ. Clinical significance of hemoptysis. JAMA 1952; 148:1461-1465

In Defense of Preoperative Pulmonary Function Tests
To the Editor:

It is the time of therapeutic nihilism in the name of cost effectiveness bowing to the temple of H.E.W. Let's see how little we can do for the patient in the name of stopping inflation. The purpose of this letter is not to refute the argument that medical care is too expensive (it is not), but rather the operative conclusions drawn by Cain et al in the article entitled "Preoperative Pulmonary Function and Complications after Cardiovascular Surgery" (Chest, 1979; 76: 130-135).

Whether or not "alarming economic ramifications" occur by disproving the value of obtaining preoperative pulmonary function tests is of no significance. If they are of value, they should be done; if they are of no value, they should be discontinued. Preoperative pulmonary function tests are not ordered for either the profit of this hospital nor for the profit of this medical director. I certainly concur with Cain et al. . "that maintenance of the proper scientific and intellectual integrity requires validation or reputation..." that pulmonary function tests are of no value preoperatively. I would, therefore, suggest that Dr. Cain's retrospective study lacks the "proper scientific and intellectual integrity."

The arbitrarily chosen postoperative stay in the intensive care unit as an index of postoperative complications in no way reflects the very complications that would be expected to be prevented by pulmonary function testing and treatment preoperatively. The postoperative stay in the intensive care unit reflects numerous other complications as well, in addition to the surgeon's attitude towards the use of the ICU.

No description is given of the preoperative and postoperative respiratory therapy other than stating they are "standard
modalities such as bronchodilators, drugs, IPPB and chest physiotherapy. I would like to suggest that type, dosage and frequency of bronchodilator drugs, method of administration, as well as frequency of IPPB, and chest physiotherapy is of significance. An IPPB unit put in a patient's room at the beginning of the day with instructions to the patient to use it four times is not the same as a trained respiratory therapist standing at the bedside adjusting the therapy and encouraging the patient to cough.

Additionally, Cain and associates state that the postoperative course of patients with an FEV₁ of less than 1 liter did not differ from that of patients with an FEV₁ of greater than 2 liters, and draw the conclusion that the knowledge of the FEV₁ preoperatively makes no difference in postoperative complications. However, they do not examine the difference in preoperative or postoperative therapy for these two groups of patients. It may well be that when the physician noted an extremely low FEV₁ that the patient received more aggressive therapy both preoperatively and postoperatively. The lack of difference in complications would then only prove the value of preoperative spirometry. Additionally, the study does not at all review the severity of postoperative respiratory complications. It is obvious that there is a difference between discoid atelectasis as opposed to lobar atelectasis as opposed to pneumonia, empyema and finally respiratory failure.

Finally, one must wonder about the efficacy of respiratory therapy at the authors' hospital, with the occurrence of atelectasis in 100 percent of patients with TAA resection, 58 percent in patients with coronary bypass surgery, 82 percent in valve aneurysm surgery and most significantly 48 percent of patients with peripheral vascular surgery.

It has been well documented in the literature as well by personal experience that preoperative evaluation, preoperative therapy if indicated, and preoperative instruction in postoperative hyperinflation maneuvers have markedly decreased both the incidence and severity of postoperative respiratory complications. The suggestion that preoperative evaluation is performed for economic gain and that it is of no value is both offensive to myself, as well as harmful to the patient.

Aloin J. Schachter, M.D.
Director, Department of Pulmonary Medicine,
St. Michael Hospital, Milwaukee

To the Editor:

We agree that the arbitrarily chosen postoperative stay in the ICU of more or less than five days is not an ideal index of complications; but as stated, we analyzed the data according to other lengths of stay from three to eight days and the conclusions were the same. In addition, length of stay combined with mortality (< 5 days - 1 percent vs > 5 days - 16 percent, P < .001) does reflect the incidence of complications as suggested by our data.

With regard to the pre- and postoperative respiratory therapy, we can only state that this institution is not a newcomer either in the field of inhalation therapy or major cardiovascular surgery. We have a very well-trained and aggressive inhalation therapy department and all of the modalities that Dr. Schachter suggests might be useful were used on many patients. However, depending on the individual physician's philosophy regarding these modalities, some patients did not receive them. We have data concerning the effects of such preoperative and postoperative therapies on the postoperative course, and we can assure Dr. Schachter that at various degrees of preoperative pulmonary dysfunction there was absolutely no difference with regard to the outcome of patients who did versus those who did not receive such pre- or postoperative therapy. In the interest of not sounding too nihilistic, we decided not to include these data in the paper until a prospective validation could be done.

Obviously, we agree with Dr. Schachter that discoid atelectasis is quite different from lobar atelectasis. However, our paper specifically states that our incidence of atelectasis was taken from the radiologist's interpretation of the daily chest radiographs that are taken on all patients in the ICU. This accounts for the very high incidences of atelectasis noted and obviously does not reflect their clinical significance. Nevertheless, we think this is still a valid index of whether patients with severe versus mild abnormalities of pulmonary function are more or less predisposed to the development of atelectasis. Under these circumstances, we hope Dr. Schachter realizes that in certain types of surgery, such as for thoracic aortic aneurysm and cardiac valve replacement, postoperative atelectasis is virtually unavoidable.

Finally, we would like to remind Dr. Schachter that our paper addresses itself specifically to pulmonary function and complications after cardiovascular surgery. This has not been well documented in the literature and we cannot accept the "personal experience" of Dr. Schachter. Cardiovascular surgery patients are different in that many undergo completely elective procedures, and prior to being referred for such surgery, these patients have been deemed to be in adequately good health by their primary physicians so as to be safe candidates for the procedure. This is very different than surgery done for other reasons such as abdominal catastrophes and lung resections. In addition, much of the older literature relating to postoperative complications was obtained prior to the days of skilled postoperative intensive care. Thus, we are suggesting that only in this group of patients and under these circumstances preoperative pulmonary function testing may be of minimal value. Furthermore, we feel that extensive pulmonary function testing with measurements such as distribution of ventilation, plethysmography, diffusion, exercise, etc. are totally unwarranted as a routine unless very specific questions are asked. These tests require expensive equipment and do add to the cost of health care without validation of their usefulness. Until then, we agree that simple spirometry and blood gas determinations ought to be continued in all patients with any suggestion of lung dysfunction. More extensive testing should be reserved only for selected patients in whom these data would likely influence the decision to operate or for clinical research purposes.

Paul M. Stevens, M.D., F.C.C.P.;
Professor of Medicine,
Baylor College of Medicine,
Houston and Harold Cain, M.D., Austin

Bronchography following Fiberoptic Bronchoscopy

To the Editor:

The technique of selective bronchography following fiberoptic bronchoscopy, as described by Simelaro and coauthors (Chest 1979; 76:240-241) is nothing new. It has already been reported by Dr. John J. Fennessy of the University of Chicago in Radiology 1970 95:689-691.

Mahmood Moinuddin, M.D., F.C.C.P.
Parma, Ohio