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 is 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 FEV, of less than 1 liter did not differ from that of patients with an FEV, of greater than 2 liters, and draw the conclusion that the knowledge of the FEV, 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 FEV, 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.

Alain 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 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 Simelro 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