bronchoscope. They did not emphasize the increased risk from general anesthesia (two myocardial infarctions, cardiac arrhythmia), the need for prolonged mechanical ventilation, and the increased cost of using general versus topical anesthesia, as well as monitoring in the intensive care unit for two days. Curiously, the authors did not find an advantage of the rigid bronchoscope over the fibreoptic bronchoscope in the trachea.

We have administered more than 200 LPT treatments to more than 125 patients since 1983 with only one episode of major (>50 ml) endobronchial hemorrhage, which occurred in the posttreatment period. All treatments were administered with a two-channel fibreoptic bronchoscope through at least an 8.5-F endotracheal tube. Most procedures were performed with the use of topical anesthesia except for those cases in which the patient was already intubated and was being mechanically ventilated because of respiratory distress before treatment. Our topically anesthetized patients go home 24 h after LPT. Our technique emphasizes vaporization of tumor mass rather than mechanical debriement. We believe that the reports of our early results indicate that the improvement of airway obstruction is as good in proximal lesions as in peripheral lesions.

Chan et al concluded that the rigid bronchoscope should be used for proximal lesions, the fibreoptic bronchoscope should be used for distal lesions, and both should be used when the airway anatomy is distorted. We believe that there is a good case for the use of the fibreoptic bronchoscope in all of these settings and that the skill and judgment of the operator should be the deciding factors.

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REFERENCES

To the Editor:

Doctors Dedhia and Lapp present some arguments for the preferential use of the fibreoptic bronchoscope (FOB) over the rigid bronchoscope (RB) during Nd:YAG laser phototherapy (LPT).

Although there were two cases of myocardial infarction when using the RB, we were unable to attribute this solely to the use of general anesthesia, as one patient was critically ill, and the other had complete proximal bronchial obstruction. In addition, most of our patients undergoing LPT under general anesthesia were successfully extubated in the recovery suite. Indeed, we have found that use of the RB under general anesthesia during LPT allows more controlled and safer conditions, especially in unstable patients and during significant airway hemorrhage.

Use of LPT under local anesthesia also requires significantly more treatment sessions to obtain a response compared with use of LPT under general anesthesia. The shorter operating time of the latter together with the lesser number of treatment sessions lends to cost effectiveness.

We feel that our data strongly suggest that, whenever possible, the RB should be used for proximal lesions and the FOB for distal lesions. Because both types of bronchoscope are sometimes used together during LPT, we again conclude that Nd:YAG laser bronchoscopists should be proficient in both techniques.

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Bi-PAP, Nasal Ventilation, and Body Ventilators

To the Editor:

We read with interest the article by Sanders and Kern in which the use of bi-level positive airway pressure (Bi-PAP) was studied in the treatment of sleep apnea. They noted difficulty in raising inspiratory positive airway pressure (IPAP) over 15 cm H2O because of mask leaks. Another recent publication reported nasal bridge soft-tissue breakdown resulting from continuous positive airway pressure (CPAP). The widely acknowledged intolerance of CPAP by approximately 30 percent of patients is at least in part due to leak and discomfort from the CPAP mask. Newer CPAP masks (Adam, Puritan-Bennett, Boulder, Colo) are generally more comfortable and better tolerated at IPAP pressures up to 20 cm H2O, at which point they too may leak or pinch the nostrils. It is important to call attention to the fact that custom-molded nasal interfaces have been used for the delivery of nasal intermittent positive pressure ventilation (IPPV) for the past six years. Similar interfaces are now commercially available (SEFAM mask, Lifecare, Lafayette, Colo). These interfaces are effective and convenient for the entire continuum of CPAP, Bi-PAP, and nasal IPPV at considerably greater pressures than their generic counterparts are.

Sanders and Kern also noted that nonapneic oxyhemoglobin desaturation could be prevented by raising IPAP alone and cited several mechanisms to explain this, including the effect of reversing airway narrowing and reversal of CPAP-induced depression of cardiac output. The ability of their patients to adequately ventilate themselves, however, was not established.

We have observed that ventilatory assistance or support is a direct function of the difference between the IPAP and the expiratory positive airway pressure (EAP). Thus, the normalization of SPO2 is at least in part due to correction of PACO2. As the IPAP-EAP difference approaches 18 to 25 cm H2O, the technique becomes the equivalent of nasal IPPV. The effect of the IPAP-EAP difference of nasal IPPV in supporting ventilation during sleep is observed most dramatically in patients with little or no measurable vital capacity.

It is of concern to see articles devoted to the management of chronic hypoventilation in which the use of negative-pressure body ventilators is still being encouraged and the effectiveness and convenience of mouth and nasal IPPV is regarded with skepticism, as evidenced by such statements as "This technique has not been fully evaluated nor has its efficacy been definitely demonstrated." We have managed over 200 patients on 24-h ventilatory support without tracheostomies by using either nasal or mouth IPPV for nocturnal support. Many switched to these techniques from body ventilators because of the sleep apneas and inconvenience associated
with body ventilator use. Although there have been many recent reports exploring the use of nasal IPPV, we find that mouth IPPV is at least as effective and is preferred by many patients. We encourage other investigators to explore these techniques.

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To the Editor:

We are pleased that our article interested Dr Bach. We wish to emphasize, however, that the primary goal of our study was to evaluate the relative contributions of inspiratory and expiratory positive pressure administration toward mitigating the upper airway dysfunction culminating in obstructive sleep-disordered breathing. Our investigation indicated that it is possible to achieve satisfactory amelioration of obstructive sleep-disordered breathing and improvement in oxyhemoglobin saturation during sleep using positive airway pressure that is relatively lower during expiration than during inspiration. By definition, differential pressure administration during the two phases of the ventilatory cycle is not possible with conventional nasal CPAP. The clinical message is that when using nasal CPAP, sleep apnea patients may be receiving greater pressures during expiration than are necessary to alleviate their sleep-disordered breathing.

While not losing sight of the main thrust of our study and our article, we agree with Dr Bach that treating patients with both sleep apnea and sleep-associated hypoventilation entails a twofold problem, including the ability to tolerate the positive pressure modality itself and the degree of acceptance of the interface between the device and the patient. Our experience thus far indicates that most sleep apnea patients who cannot, or prefer not to tolerate nasal CPAP because of the sensation of excessive pressure are able to tolerate treatment when the expiratory pressure is lowered relative to the inspiratory pressure. With regard to patient tolerance of the interface, we have found this to be a very variable and personal decision by patients. There are clearly a number of options available, each with its potential merits and detractions and none having been compared to the others in large, controlled trials. Two of our 13 patients experienced a leak through the nasal mask during CPAP sufficient to necessitate supplemental oxygen administration. On the other hand, in our experience and that of others, there is a comparable distribution of preference for nasal mask and commercially available nasal prong systems in delivering CPAP across a population of sleep apnea patients. We are aware of Dr Bach's extensive use of mouthpieces to deliver positive pressure, but one still needs to be concerned about the possibility of aspiration, mouth dryness, abnormal dentition, and the effects of buccopharyngeal muscle competence on the adequacy of this technique. These issues have limited our application of mouthpieces in several instances.

Dr Bach expressed concerns about the adequacy of ventilation in our patients during the administration of the Bi-PAP. He notes that the degree of ventilatory augmentation is related to the pressure difference between inspiration and expiration, and that normalization of the oxyhemoglobin saturation is at least in part attributable to reduction in Pco2; these are premises with which we fully agree. Thus, while we did not directly measure arterial Pco2 during sleep in our sleep apnea patients, it is likely that ventilation was being maintained at acceptable levels. Along these lines, since initiating our study of sleep apnea patients we have had the opportunity to apply Bi-PAP to hypercapnic patients with neuromuscular disorders. We have found that during sleep, this modality can normalize the Pco2 (as reflected by arterial blood gas analysis in some patients and in others by continuous transcutaneous monitoring validated at points by arterial blood analysis). Nocturnal application of Bi-PAP can result in reduction in arterial Pco2 during wakefulness. These data suggest that this modality can indeed provide adequate ventilatory assistance to sleeping patients.

It is clear that at this time, with respect to the interface between positive-pressure devices and patients, no single technique is likely to be universally acceptable. Accordingly, in our center, within the confines of clinical application, we routinely offer patients a choice of interfaces. The real message is that more work needs to be done in this area, and well-designed clinical trials are needed to indicate the role and limitations of each new modality that is developed. Even the best hardware is clinically useless unless it can be properly and consistently interfaced with the patient.

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REFERENCE

Need for Oxygen Consumption Measurements

To the Editor:

I read with interest the article by Villar et al, in the September 1990 issue of Chest, on oxygen transport and oxygen consumption in critically ill patients. It supplies some interesting data. However, the authors' method of determining oxygen consumption by calculation from the Fick principle leaves much to be desired in today's arena of critical care investigation. The problems were summarized recently by Bartlett and Dechert. They outlined four categories of potential errors: accuracy of primary measurements, mathematical coupling of consumption and delivery calculations, statistical analysis based on few data points per patient, and the definitions of steady state and pathophysiology in the critically ill.

Villar et al had three to five data points per patient in each group. The accuracy and reproducibility of cardiac output measurements

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