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.

**John R. Bach, M.D.,**

*Department of Physical Medicine and Rehabilitation, University of Medicine & Dentistry of New Jersey, Newark*

**References**


**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 modalities 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 PACO2, these are premises with which we fully agree. Thus, while we did not directly measure arterial PACO2 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 PACO2 (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 of arterial PACO2. 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.

**Mark H. Sanders, M.D., F.C.C.P., and Nancy Kern, C.R.T.T., R.PSC.T., Division of Pulmonary/Critical Care Medicine, University of Pittsburgh School of Medicine, Pittsburgh**

**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...
were within 7.5 percent as a coefficient of variation (three to five injections). This is an accepted range and reigns as the "gold standard"; however, we should remember that it may be the best possible measurement at present, but it certainly is not the best absolute measurement. The time lag between injections is not given, and neither is the value for mixed venous saturation during this lag. If the measurements took more than 5 min, the patient's venous saturations may have changed by ten percentage points. This would not suggest a steady state—the condition that would have to be met for the Fick calculation to be accurate; the supposition is that if the metabolic rate remains constant, an increase in cardiac output should be matched by a proportionate fall in venous content. Examination of the data of Villar et al. shows that the mixed venous saturations were not reported. A 10 percent drop in a venous saturation of 48 percent does not suggest a steady-state condition, especially as far as consumption is concerned.

Further, the accuracy of hemoglobin measurement is ±2 percent, as is the accuracy of saturation measurement. There is no information given on the accuracy of the hemoglobin or saturation measurements. All these factors could combine to create a range of error of ±15 percent in calculated oxygen consumption. Taken together with the large standard deviation in oxygen consumption in the septic group, it can be seen that steady states between cardiac output measurements probably did not exist, making it difficult to accept the consumption calculation as equivalent to a measured value.

The time has come to ask for consumption measurements rather than calculations. This would have the benefit of a more accurate assessment of oxygen consumption dependency. With today's technology, the accuracy of indirect calorimetry is approximately ±5 percent over a wide range of FiO2 values. This would uncouple the variables and reduce magnification errors in data. It should also be noted that some of the computerized hemodynamic calculations generated daily in the intensive care unit have calculated consumptions. These should be abandoned. As investigators examine oxygen consumption dependency on transport, proper methods of consumption determination are necessary before interventions are recommended.

Richard R. Peace, M.D., F.C.C.P.,
Chief, Pulmonary Division,
Mercy Hospital,
Pittsburgh

Reprint requests: Dr Peace, Department of Medicine, Mercy Hospital, 1400 Locust Street, Pittsburgh, PA 15219

REFERENCES

Use of the APACHE II System in Surgical Lung Carcinoma Patients

To the Editor:

We read with interest the September 1990 issue of Chest the article by Giangiuliani et al regarding the use of the APACHE II score in the assessment of surgical lung carcinoma patients. Based on our experience in the extensive use of this score in critically ill patients, we would like to make some comments on that interesting article.

The APACHE II score is designed to quantify the severity of illness at intensive care unit (ICU) admission. Like other prognostic indexes, it is accurate, specific, and sensitive in predicting outcome in groups of intensive care patients. The usefulness of APACHE II at hospital admission in outcome prediction has never been validated in large series of patients. Variables used at ICU admission have been shown to classify groups of patients and accurately predict outcome. These parameters probably are completely different from prognostic variables to assess at hospital admission of scheduled surgical patients. Such a study would require an assessment of potentially useful variables by means of discriminant analysis and building a new model with use of multiple logistic regression.

Giangiuliani et al obtained only mediocre results when comparing observed and expected outcomes using the APACHE II score and the TNM staging system for lung carcinoma. A total correct classification rate of 55.6 percent at a cutoff point of 0.7 and an area under a receiver operating characteristic curve of 0.54 is far from good performance, especially considering that tossing a coin would give an area of 0.5. Some degree of discrimination with the method proposed by Giangiuliani et al is not surprising given that the APACHE II score includes age and chronic abnormalities. In fact, high-risk patients were older (not significant in univariate analysis) and showed significantly more chronic abnormalities of the respiratory system than low-risk patients did. Probably the acute physiology scores of both groups were equivalent.

In summary, it is our opinion that APACHE II is not validated for stable patients and should not be used to categorize patients admitted for scheduled surgery. The acceptable performance of their method can be explained by different degrees of respiratory chronic disease and age between the groups. We suggest that a better analysis could have been identification of risk factors by means of logistic regression analysis. Nevertheless, we found the article by Giangiuliani et al very interesting. We think that it adds a new perspective on preoperative evaluation and is a source of future research in the field of prognostic factors in surgical lung carcinoma.

Xavier Castella, M.D., F.C.C.P.,
Hospital de Sabadell,
Barcelona, Spain

REFERENCES

To the Editor:

We read with interest Dr Castella's comments on our article, and we agree with him that discriminant analysis followed by linear logistic regression on the significant predictors would achieve a higher performance in the classification of specific groups of individuals, like lung carcinoma patients. However, three considerations prevent us from adopting such a scheme: First, a large patient base and a multicentric approach are necessary in our opinion to derive meaningful results; such conditions are not available in our institution. Second, we believe that an exhaustive procedure on a particular patient set would be sensitive to variations in the data and would be of limited usefulness in the prediction of outcome in new patients in the same category (unless the numbers