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 themselves 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 disadvantages 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 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.

Mark H. Sanders, M.D., F.C.C.P., and
Nancy Kern, C.R.T.T., R.P.S.C.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 Deichert. 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 patients'
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 45 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 informa-
tion given on the accuracy of the hemoglobin or saturation mea-
surements. 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 groups, 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 tech-
nology, the accuracy of indirect calorimetry is approximately ±5
percent over a wide range of \( \text{FiO}_2 \) 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 consump-
tions. These should be abandoned. As investigators examine oxygen
consumption dependency on transport, proper methods of con-
sumption determination are necessary before interventions are
recommended.

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

Reprint requests: Dr Pace, Department of Medicine, Mercy Hos-

tial, 1400 Locust Street, Pittsburgh, PA 15219

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Use of the APACHE II System in Surgical Lung Carcinoma Patients

To the Editor:

We read with interest in 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,¹ 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.² Like other prognostic
indexes,² 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.³

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 clas-
sification 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 respi-
atory 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

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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 consid-
erations prevent us from adopting such a scheme: First, a large
patient base and a multivariate 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

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