Effects of Inverse-Ratio Ventilation on Cardiorespiratory Measurements in Severe Respiratory Failure

To the Editor:

Chan and Abraham's article,1 which appeared in the November 1992 issue of Chest, is a praiseworthy investigation. Severe respiratory failure, either isolated or as a part of multiple organ dysfunction syndrome, is a relatively common entity in intensive care units. Unfortunately, it is associated with a frustratingly high mortality rate and often requires mechanical ventilation with high FiO2 and high airway pressures. Both prolonged exposure to high FiO2 and elevated peak airway pressures have been shown to contribute to the lung injury.4,5 Newer modes of mechanical ventilation, like pressure-controlled inverse ratio ventilation (PC-IRV), are being used more frequently because they are designed to decrease the peak airway pressure. Therefore, it is important that studies like that of Drs. Chan and Abraham be done to better understand the utility of these modes.

In their study, they found that when PC-IRV was compared with pressure-controlled ventilation with a conventional ratio, PC-IRV was associated with a significant improvement in PaO2, a decrease in shunt fraction, a decrease in PaCO2, and a decrease in dead space, but there was no improvement in oxygen delivery as cardiac output fell. They concluded that inversion of conventional ratios produces significant improvement in the overall cardiorespiratory profile. Several points, however, need to be stressed. In an attempt to control for the variability of critically ill patients, the authors limited their trial period of PC-IRV to 1 h. This can be misleading, since the full benefits of inverse ratio may not be fully appreciated after 1 h, but continued improvement can be seen up to 6 h later.

In addition, the authors did demonstrate an improvement in PaO2 with a decrease in the shunt fraction through the use of PC-IRV, which may have important consequences if it allows the physician to taper the FiO2 down to nontoxic levels. This improvement could also allow for a decrease in the extrinsic positive end-expiratory pressure (PEEP). Likewise, the authors demonstrated an improvement in ventilation with a decrease in both PaCO2 and dead space. This improvement would allow the physician to decrease the minute ventilation by decreasing the pressure drive; consequently, peak airway pressure would decrease. Decreasing both extrinsic PEEP and peak airway pressure should reduce the negative effect that PC-IRV had on cardiac output and consequently improve oxygen delivery. In several studies of patients with severe respiratory failure over prolonged times, PC-IRV has been shown to result in significant improvement in oxygenation at lower minute ventilation, lower peak airway pressure, and lower PEEP requirements.6,7

In summary, I think the authors did a commendable job, as it is difficult but necessary to do clinical trials in the intensive care unit. I would caution that although they did not demonstrate an immediate improvement in oxygen delivery after 1 h of PC-IRV as compared with pressure-controlled ventilation with conventional ratio, improvement might have been seen with a more prolonged trial of PC-IRV or with improvement in cardiac output through reduction of extrinsic PEEP and peak airway pressures (maneuvers afforded by the demonstrable improvement of shunt fraction and reduction in dead space by PC-IRV). Further studies still need to be performed.

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References
1 Chan K, Abraham E. Effects of inverse ratio ventilation on

Communications to the Editor

References

To the Editor:

Dr. Venkataramani and his colleagues describe an interesting case of chronic pulmonary infection with polymicrobials including A actinomycetemcomitans. Septic embolization to the left hand in the absence of echocardiographic evidence of endocarditis was also noted. No systemic embolization was found in the case we reported or in the reviewed literature concerning pulmonary infection with A actinomycetemcomitans.

From January 1985 to December 1990, four cases of infective endocarditis caused by A actinomycetemcomitans were seen in our hospital.1 All patients met the criteria for diagnosis of infective endocarditis, which included underlying valvular disease, persistently positive blood culture, fever, echocardiographic evidence of valvular vegetation, or systemic embolism.2 Three of the patients had undergone xenograft replacement, and the fourth had prolapse of the mitral valve. The echocardiogram was performed more than once for each patient. However, only one patient had definite evidence of valvular vegetation proved by transesophageal echocardiogram. Three also had hematuria and anemia due to possible septic embolization of the kidney.

According to Kaplan et al, A actinomycetemcomitans infection rarely accounts for native valve endocarditis (0.27 percent of cases).3 Although negative echocardiographic findings do not exclude the presence of endocarditis, in the case reported by Venkataramani et al no underlying heart disease, persistently positive blood culture, fever, or signs of heart failure were described. There was also no wound, the most common cause of skin infection with A actinomycetemcomitans, found in the hand before infection. Therefore, we agree that transient septic emboli from the focus of polymicrobial pulmonary infection may be the source of septic embolization of the limb in this patient.

Actinobacillus actinomycetemcomitans is part of the normal oral flora. Its role as a human pathogen in multiorgan infection, especially in patients with periodontitis, has been recognized in recent decades. This should be kept in mind when evaluating infection in such patients.

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References


5 Ravizza AG. Inverse ratio and conventional ratio of inspiratory effects [abstract]. Anesthesiology 1983; 59:A523


To the Editor:

We appreciate the comments of Dr. Read. We agree that improvements may be seen over periods of up to 6 h after a change to IRV, but this is quite difficult to document clinically, given the complexity of the physiologic and therapeutic alterations occurring over time in critically ill patients. We investigated three 1-h periods to limit changes in patient status, thereby allowing determination of physiologic changes that can be attributed solely to ventilatory modality.

We found improvement in PaO₂ with a decrease in shunt fraction through the use of PC-IRV. The improvement in dead space and oxygenation should allow a reduction in extrinsic PEEP, FIO₂, and minute ventilation, which may offset the deleterious reduction in cardiac output that IRV will produce. Both Tharratt et al and Lain and colleagues retrospectively found that oxygenation could be maintained with PC-IRV at lower minute ventilatory rates, lower peak pressures, and lower extrinsic PEEP levels when compared with volume control ventilation. However, neither study measured intrinsic PEEP levels ("auto-PEEP"), which may have been equal to or greater than the initial extrinsic PEEP settings and may be as detrimental to cardiac output as extrinsic PEEP. Additionally, oxygen delivery and oxygen consumption values were not determined in either study, and hemodynamic parameters were not measured by Tharratt et al. Lain et al did mention that there was no significant difference in cardiac output; however, only 11 of the 19 patients had pulmonary artery catheters in place, and other hemodynamic indicators were not calculated.

Dr. Read’s suggestions that the additional ventilatory manipulations used with PC-IRV can offset the deleterious cardiac effects of PC-IRV deserve further investigation. However, since pressure-controlled ventilation with normal ratios of inspiratory to expiratory time improves arterial oxygenation and oxygen delivery without causing a reduction in cardiac output, it may be the preferred mode of ventilation in patients with severe respiratory failure before resorting to PC-IRV.

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REFERENCES


Rules of Evidence and Clinical Recommendations on the Use of Antithrombotic Agents

Errata

To the Editor:

We would like to call your readers’ attention to two errors in our article, which appeared in the supplement to the October 1992 issue of Chest.

In the left-hand column on page 3065, in the second paragraph under the heading “Levels of Evidence,” the first sentence should read as follows: “Low false-positive (α) error indicates a positive trial that demonstrated a statistically significant benefit from experimental treatment.” The third sentence in that paragraph should read as follows: “Low false-negative (β) error (high power)” indicates a negative trial that demonstrated no effect of therapy, yet was large enough to exclude the possibility of a clinically important benefit (ie, had a very narrow 95% confidence interval, the upper end of which was less than the minimum clinically important benefit, thereby excluding any improvement due to the test treatment).”

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REFERENCE


Is Chest CT Performed Too Often?

We have had very similar experience with computerized tomography to that described by Dr. DiMarco in the April 1993 issue of Chest (103:985-86). We appreciate his succinct editorial comments, but there is one area of encouragement. Those coin lesions being approached as potential clinical and roentgenographic T1N0M0 neoplasms of the chest have already been evaluated. The T1N0M0 lesion does not require chest CT due to the “unnecessary evaluation of false-positive studies and the delay in diagnosis associated with false-negative studies.” Dr. DiMarco outlined the cost and radiation risk of the chest CT, but the cost and radiation risk in evaluating false-positive results is alarmingly additive.

We agree that chest CT often produces "no new clinical information" and "no impact on patient care" when the chest x-ray and previous films are analyzed in conjunction with the patient's history. We are often consulted about a CT result when no effort to create the patient's roentgenographic history has been attempted. We wonder if it is really legitimate for the x-ray report to read, "no old films for compar-