number [of infants] will not tolerate a mask on their face. Some will let you hold it 2 cm from their face but will not let you put it on their face." He cites my editorial for that remark.[1] Not only did I not write that, but it has been my experience over 20 years that the majority of young children will accept a face mask placed on their face by a caregiver, which allows medication to be easily administered from a meter-dose inhaler and holding chamber (our preferred mode of delivery in young children) or from a jet nebulization. It is true that some infants and young children will not tolerate having a mask placed on their face, but almost uniformly these same infants will not tolerate having the mask placed immediately in front of their face, and so it is improbable that these infants will breathe quietly while holding absolutely still with a mask less than an inch from their nose and mouth. In real life, when I have observed blow-by being administered to a child by the parent, the tubing is invariably held 5 cm or more from the child's face, which is in more or less constant motion. An RT would be deluded to believe that any medication is being deposited under these circumstances.

As Mr Baty points out, in this era of evidence-based medicine it is incumbent on all of us to read and to understand the literature in order to provide the best possible care to our patients. This careful literature review shows that there are no clinical data supporting the use of blow-by aerosol administration as an adequate substitute for a comfortably applied mask.

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Dr Rubin has been a consultant for Pfizer, Ventaira, Trudell Medical International, Monaghan Medical, GlaxoSmithKline, and Medihale. He reports no other conflicts of interest in the content of this letter.

REFERENCES

1. Rubin BK. Bye-bye, blow-by. Respir Care 2007;52(8):981.
2. Geller DE. Comparing clinical features of the nebulizer, metered-dose inhaler, and dry-powder inhaler. Respir Care 2005; 50(10):1313-1322.
3. Thorsson L, Geller D. Factors guiding the choice of delivery devices for inhaled corticosteroids in the long-term management of stable asthma and COPD: focus on budesonide. Respir Med 2005;99(7):836-849.
4. Geller DE, Kesser B. Blowby vs. face mask for nebulized drugs in young children (abstract). J Allergy Clin Immunol 2004; 113(2):S32.
5. Nikander K, Berg E, Smaldone GC. Jet nebulizers versus pressurized metered dose inhalers with valved holding chambers: effects of the facemask on aerosol delivery. J Aerosol Med 2007;20 supp 1:S46-S58.
6. Restrepo RD, Dickson SK, Rau JL, Gardenhire DS. An Investigation of nebulized bronchodilator delivery using a pediatric lung model of spontaneous breathing. Respir Care 2006;51(1):56–61.
7. Lin H-L, Restrepo RD, Gardenhire DS, Rau JL. Effect of facemask design on inhaled mass of nebulized albuterol using a pediatric breathing model. Respir Care 2007; 52(8):1021-1026.
8. Dickerson B, Delich N, Sakomoto G. Comparison of 4 nebulizer-patient interfaces in a pediatric lung model (abstract). http://www.rcjournal.com/abstracts/1998/?id-A00000861. *Accessed May 28, 2008.*

Airway Pressure-Release Ventilation

My comments and concerns are directed toward the article in RESPIRATORY CARE titled "Does Airway Pressure Release Ventilation Offer Important New Advantages in Mechanical Ventilator Support?" by Timothy Myers and Neil MacIntyre.[1] My concerns regarding this article are 4-fold.

First, there was no true "champion" of airway pressure release ventilation (APRV) represented in the article or present at the conference. If we are going to point out the shortcomings of a particular ventilation mode, then maybe a proponent of that mode should be part of the conference faculty.

Second, when discussing APRV in the context of end-inflation stretch and ventilator-induced lung injury in the "Con" section, Myers and MacIntyre assumed that spontaneous ventilation at P_{high} will automatically increase transpulmonary pressure to a dangerous level, yet no proof is given. This then is translated from "hypothetical concern" to accepted fact in the article's summary, which states, "However, because spontaneous breaths are encouraged during the inflation period, end-inflation transpulmonary pressure (stretch) will be higher than the applied inflation airway pressure and could be higher than conventional assist-control modes." I am not certain that the article made that connection in an evidence-based manner. It is asserted in the article's abstract that, "if the patient makes a spontaneous breath during T_{high}, the tidal volume generated could be much larger than the clinician-set target tidal volume. . . ."[1] Target tidal volume is not a value that is set when using APRV.[2]

Third, Myers and MacIntyre indicated that there is substantial discomfort and asynchrony with APRV, which is something I have not seen clinically in my hospital practice. In fact, in my practice most patients indicate when asked that they are more comfortable on APRV than on assist-control or pressure-regulated volume control. It has been hypothesized that that is related to the re-establishment of functional residual capacity by APRV, which thus begins spontaneous inspiration from a higher lung volume.[2] In reference to the article that Myers and MacIntyre quoted[3] to support the claim of discomfort and asynchrony, it appears that they used demand-flow APRV in a manner that may have predisposed patients to lung derecruitment, by using a substantially longer T_{low} than I have seen clinically in my practice. Appropriate use of APRV requires that T_{low} be set to terminate expiration at a percent of peak expiratory flow, in order to prevent derecruitment.[2] It is possible that the strategy used in that study allowed lung derecruitment and thus caused discomfort and asynchrony with the patient's spontaneous breaths.

Fourth, Myers and MacIntyre's comments about the study by Putensen et al[4] are of concern to me. A careful read of that article indicates that the study's findings correlated with Putensen's original hypothesis. "We hypothesized that in patients at risk for acute respiratory distress syndrome (ARDS), spontaneous breathing with APRV prevents deterioration of gas exchange or allows it to recover faster than does controlled mechanical ventilation."[4] Putensen did set out to prove the benefits of spontaneous breathing with APRV, as compared to controlled mechanical ventilation. Note that the controlled mechanical ventilation in this case was pressure-control ventilation. To have controlled mechanical ventilation they had to sedate and paralyze the patient. From a mechanical standpoint, APRV without spontaneous breathing was identical to pressure-control ventilation.[4] My impression is that the author was focusing on the benefits of spontaneous breathing, which is accomplished with APRV.

The lack of an APRV "champion" at the conference, along with misstatements regarding APRV and transpulmonary pressure changes, asynchrony and discomfort, and the benefits of spontaneous breathing in APRV are the points of concern to me. We use APRV in my institution in daily clinical practice and are impressed with its ability to provide lung recruitment, deliver the physiologic benefits of spontaneous breathing, and serve as an effective weaning modality. APRV serves our patients with ALI/ARDS as a combination lung-protective and open-lung modality, and we have been quite pleased with the results.

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The author reports no conflict of interest in the content of this letter.

REFERENCES

1. Myers TR, MacIntyre NR. Does airway pressure release ventilation offer important new advantages in mechanical ventilator support? Respir Care 2007;52(4):452-458.
2. Habashi NR. Other approaches to open-lung ventilation: airway pressure release ventilation. Crit Care Med 2005;33(3):S228-S240.
3. Chiang AA, Steinfeld A, Gropper C, MacIntyre N. Demand-flow airway pressure release ventilation as a partial ventilatory support mode: Comparison with synchronized intermittent mandatory ventilation and pressure support ventilation. Crit Care Med 1994;22(9):1431-1437.
4. Putensen C, Zech S, Wrigge H, Zinserling J, Stuber F, von Spiegel, Mutz N. Long-term effects of spontaneous breathing during ventilatory support in patients with acute lung injury. Am J Respir Crit Care Med 2001;164(1):43-49.

The authors respond:

Brent Kenney raises several issues about our paper,[1] which was part of the 38th RESPIRATORY CARE Journal Conference, "Respiratory Care Controversies in the Critical Care Setting." We will address his 4 specific points.

1. The format of this series of manuscripts in RESPIRATORY CARE was of a "pro-con" debate. Kenney is concerned that a strong proponent of airway pressure-release ventilation (APRV) was not invited to take the "pro" side. We would argue, however, that a more objective approach is to invite experts who can synthesize the evidence, not simply extol their beliefs or anecdotal experience. We believe that the data on APRV in our paper were inclusive and that our conclusions about APRV were as evidence-based as possible. Having said that, we would point out that, despite the existence of APRV for over 20 years, the evidence base supporting it is remarkably thin. Indeed a PubMed search for "airway pressure release ventilation or APRV" retrieved only 17 peer-reviewed clinical studies, most of which were observational in nature.

2. We raised 2 concerns about the physiologic effects of APRV that are often overlooked. First, the spontaneous breaths taken at P_{high} add to end-inspiratory transpulmonary pressure and end-inspiratory volume. Though we agree that this extra pressure and volume (stretch) may be small, it is nevertheless still present and should be recognized. Claiming that APRV reduces set airway pressure is often true, but the implication that this translates to lower end-inspiratory stretch may not be true. Second, it is often assumed that the short T_{low} prevents substantial derecruitment. Lung units with short time constants (ie, with poor compliance and low resistance) can easily derecruit in only a few hundred milliseconds. Thus, the potential for derecruitment-rerecruitment lung injury can not be ignored. Our paper did not state that these effects were always harmful; we only wanted to point out that these effects needed to be considered when assessing the potential role of APRV.

3. The data on patient comfort during APRV are difficult to interpret. Studies that have claimed that APRV is "comfortable" generally compared it to assist-control or pure control modes that are more challenging to synchronize with patient effort than are interactive modes such as pressure support. We accept that spontaneous breathing and appropriate functional residual volume may enhance comfort, but there are many more factors involved in optimizing patient-ventilator synchrony.

4. For any new mode to be widely adopted, it must be shown to improve important clinical outcomes, compared to a current "standard of care." To date only 2 reasonable-sized clinical trials have addressed this. The study by Putensen et al[2] clearly showed benefit from APRV, compared to their control strategy. However, their control strategy: (1) required paralysis for 3 days, and (2) produced a dramatic drop in oxygenation from the baseline (pre-randomization). In the current era of ARDS Network management algorithms, that control strategy is clearly not "standard of care." Thus, no conclusions can be drawn other than that APRV is better than a non-standard control strategy.

A better study was that by Varpula et al,[3] who used a more standard synchronized intermittent mandatory ventilation pressure-support control strategy, and between the APRV patients and the control patients there was no difference in sedation needs, ventilator days, or mortality. Until APRV is shown to improve important clinical outcomes, it is difficult to recommend widespread use.

In conclusion, we point out that the question we addressed was whether APRV offered "important new advantages" over current strategies. We believe (and the participants at the Journal Conference agreed) that, though there may be some theoretical reasons why APRV may have some advantages, and a small clinical database suggests that APRV can supply adequate ventilatory support, the notion that APRV offers "important new advantages" remains speculative at present.

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Dr MacIntyre has been a consultant for Viasys Healthcare. The authors report no other conflicts of interest in the content of this letter.

REFERENCES

1. Myers TR, MacIntyre NR. Does airway pressure release ventilation offer important new advantages in mechanical ventilator support? Respir Care 2007;52(4):452-458.
2. Putensen C, Zech S, Wrigge H, Zinserling J, Stuber F, von Spiegel, Mutz N. Long-term effects of spontaneous breathing during ventilatory support in patients with acute lung injury. Am J Respir Crit Care Med 2001;164(1):43-49.
3. Varpula T, Valta P, Niemi R, Takkunen O, Hynynen M, Pettilä VV. Airway pressure release ventilation as a primary ventilatory mode in acute respiratory distress syndrome. Acta Anaesthesiol Scand 2004;48(6):722-731.