treatment assessment who is best able to evaluate the appropriateness of the delivery technique and who, after consultation with the physician, changes the medication, delivery device, or in some cases recommends the discontinuation of inappropriate or ineffective therapy.

Ronald Baty MA RRT-NPS
Respiratory Care
Inova Fairfax Hospital
Falls Church, Virginia

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

REFERENCES

1. Rubin BK. Bye-bye. blow-by. Respir Care 2007;52(8):981.
2. 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.*
3. Geller DE. Comparing clinical features of the nebulizer, metered-dose inhaler, and dry-powder inhaler. Respir Care 2005; 50(10):1313-1322.
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. 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.
6. 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 (Suppl 1):S46-S58.
7. 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.
8. 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.

The author responds:

I am delighted that Mr Baty has commented on my editorial regarding using the blow-by technique to deliver aerosol medication.[1] As he points out, there have been great changes in the practice of respiratory care in the 25 years since I began my career as an academic pediatric pulmonologist and aerosol scientist. There have been advances in nebulizer technology and improvements in the interface between the child and the nebulizer. I agree with his contention that the RT should choose the appropriate interface supported by evidence-based research, especially because the clinical assessment of bronchodilator response is inaccurate in young children. The published peer-reviewed data clearly demonstrate that blow-by delivery of aerosol is inferior to using a mouthpiece or a face mask sealed on the child's face. Mr Baty claims that there is a literature supporting blow-by aerosol therapy, and he gives several references for this claim. Let's see what these papers cited by Mr Baty really say.

Three of these papers were written by my friend, Dr David Geller. Dave is a pediatric pulmonologist and a superb aerosol scientist. However, 2 of these papers are review articles that contain no data. The review published in RESPIRATORY CARE indicates that studies of blow-by must be validated by clinical trials.[2] In the review with Thorsson,[3] Thorsson and Geller write that, "To avoid crying, some caregivers will move the mask away from the face and give 'blow-by' treatments. However, a poor face mask seal will result in 40–85% declines in inhaled dose with both metered-dose-inhaler/spacer devices and nebulizers."[3] This hardly supports the use of blow-by as an alternative technique. Dr Geller also presented unpublished data in an abstract that compared fine-particle dose from a T-piece nebulizer, using an in vitro model with a close-fitting face mask, blow-by with a mask, and blow-by with an extension tubing.[4] The blow-by tubing was aimed directly at a filter, and the dose captured on that filter was measured. It is not surprising that when blowing drug aerosol at a filter with a gap of less than 1 inch, there was a similar amount of medication deposited as when the filter was placed on a mask. This surely does not represent a realistic clinical scenario.

Similar to this, Nikander and colleagues evaluated a front-loading face mask at a gap of less than 2 inches from a face model with a fixed, open mouth 22 mm in diameter.[5] A filter was placed behind this open mouth, and a breathing simulator provided flow. The authors found that "in the evaluation of the blow-by technique with this bench model, the inhaled mass was clearly affected by the increase in distance between the face and the face mask." Although there was adequate deposition at very close range, when the drug was aimed directly at the open mouth, the drug mass significantly decreased as the mask was brought even a short distance away. Clinically, these studies would be like asking an infant to keep his mouth wide open so that a tube can continuously deliver aerosol into the mouth from a distance of less than 1 inch while the child and the tubing are held absolutely still. Although this sounds silly, such are the limitations of in vitro studies.

An interesting finding of the Nikander study, supported by Dr Restrepo's work,[6,7] is that a front-loading face mask is more likely to entrain aerosol than is a mask where the tubing is at the bottom of the mask. The fish-face mask described by Restrepo has the following modifications:

1. The mask is front-loaded so that the aerosol can stay within the mask rather than being blown out of the top.

2. The mask size is larger and has an extended face cover.

3. The side holes are much smaller than that of a standard mask, which reduces the area of potential aerosol loss to one eighth that of the standard mask.

Restrepo et al showed that, with less than a 1 inch gap, blow-by delivery reduces aerosol available to the patient by 58%, compared with a sealed face mask. This newly designed face mask *only* reduced the amount of medication available by 38% at a distance of 2 cm.[7] However, even under these optimal bench conditions, using the new mask, only a mean of 2.26% of the nominal (nebulized loading) dose was deposited on the filter! They concluded that the best way to deliver aerosol medication to an infant is with a mask held sealed against the face.

Mr Baty also cites an abstract presented a decade ago at the 44th International Respiratory Congress of the American Association for Respiratory Care, where Dickerson and colleagues studied aerosol deposition using T-adapter blow-by aimed toward a manikin head with open mouth.[8] They measured aerosol concentration in the respiratory range and showed that blow-by at a distance of 4 cm delivered significantly less than the sealed mask. They concluded that, "The results support the use of aerosol face masks as a recommended interface for infants." Thus it appears that a careful reading of each of these references condemns the use of blow-by as an alternative technique.

Most interesting was Mr Baty's remark that, "for a variety of reasons a substantial

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.

Bruce K Rubin MEngr MD FAARC
Department of Pediatrics
Wake Forest University
School of Medicine
Winston-Salem, North Carolina

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.