Who Should Manage the Airway?

In a recent special issue of Respiratory Care, Daniel Talmor presented a very nice review of airway management during a mass-casualty event.1 He rightly pointed out that only experienced clinicians should perform intubation in these circumstances, and that training clinicians for the sole purpose of providing intubation during a mass-casualty event is unwise. He also listed anesthesiologists, certified registered nurse anesthetists, intensivists, and emergency medicine physicians as clinicians who the literature shows are able to “successfully manage the airway.” However, the literature also shows that respiratory therapists (RTs) can be trained to perform emergency endotracheal intubation efficiently and safely.

In a small study of 50 consecutive intubations, McLaughlin and Scott2 found that the RTs involved successfully intubated all patients. The mean number of attempts was 1.48, and most patients were successfully intubated in less than 1 min. In a larger study, with over 800 intubations, at Duke Medical Center,3 Thalman and colleagues found a 95% intubation success rate among RTs. Ninety-two percent of the intubations were accomplished with fewer than 3 attempts. Moreover, well-trained RTs at Butterworth Hospital in Grand Rapids, Michigan, had a 90% intubation success rate when physicians failed.4 At my community hospital, RTs in my department have provided bi-level positive airway pressure (BiPAP) for bi-level positive airway pressure (BiPAP) intubation in a short period of time. In addition, the American Association for Respiratory Care Clinical Practice Guideline for Management of Airway Emergencies2 recognizes registered RTs as clinicians capable of being trained to be primary providers of endotracheal intubation. The key, of course, is training. With good initial training and periodic recertification, including book study, RTs can perform emergency intubation with good proficiency.5 Community hospitals are not immune from mass-casualty events and may not be staffed with anesthesiologists and intensivists at all times of the day and night. In that setting, RTs may prove particularly valuable when disaster strikes and multitudes of patients require intubation in a short period of time.

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

REFERENCES


The author responds:

Jeffrey Haynes correctly points out my omission of respiratory therapists (RTs) from the potential pool of providers able to manage the airway in a disaster.1 This is particularly embarrassing, as Respiratory Care is, of course, the official journal of the American Association for Respiratory Care. There is, as he points out, substantial literature that supports the ability of RTs to safely manage the airway. This practice pattern is prevalent in many parts of the country, and in particular in smaller hospitals and other areas that lack 24-hour physician coverage. Also, the American Association for Respiratory Care encourages and supports this practice with its Clinical Practice Guideline for Management of Airway Emergencies.2

It should be pointed out that, though widespread, RT airway management is inconsistently practiced. Many RTs, and in particular those who practice in larger, urban centers, do not have the opportunity to practice these skills after their initial training. An emergency mass-casualty event is not the time for these providers to be refreshing their skills. In other words, only those who have intubation as a part of their daily practice should perform intubation in an emergency.

Also, RTs will be a scarce resource in an emergency. Their unique expertise will be required for managing patients in respiratory failure long after the acute event of intubation. In a scenario where there are other clinicians with intubation expertise available for intubation, I would suggest that RTs’ efforts would be better spent on the more complex issues of managing the ventilated patient.

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

REFERENCES


Noninvasive Ventilation During a Mass-Casualty Event

The January 2008 issue of Respiratory Care published an article by Branson et al,1 which included a recommendation to forgo noninvasive ventilation (NIV) during an event of mass-casualty respiratory failure (a “surge” event). Moreover, they propose that bi-level positive airway pressure (BiPAP)
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The authors report no conflict of interest in the content of this letter.

REFERENCES

The authors respond:

We appreciate John McCracken’s assessment of our paper.1 First, we want to clarify that we never intended to promulgate our recommendations as gospel. Currently, there is insufficient direct evidence to conclusively determine optimal strategies for oxygenation and ventilation during a mass-respiratory-failure surge event. We believe, though, that the various available positive-pressure ventilation (PPV) strategies will not all be equally effective for such events, and strong indirect evidence even suggests that some PPV strategies may be nearly useless (eg, automatic resuscitator to ventilate a patient with severe respiratory failure over numerous days). McCracken argues that “we should plan to use noninvasive ventilation (NIV) as much as possible during an epidemic.” We respectfully disagree. For an event when the number of patients with severe respiratory failure will far exceed the usual capability of appropriate staff and PPV equipment, we strongly recommend against planning for widespread use of NIV when considering (1) optimal use of existing PPV devices, and (2) stockpiling additional PPV equipment.

The United States Department of Homeland Security National Planning Guidelines are intended to coordinate and prioritize emergency preparedness efforts at all response levels. Those guidelines contain 15 National Planning Scenarios, and at least two thirds of those may include catastrophic numbers of patients in acute respiratory failure.2 A successful PPV strategy for such a catastrophe must be grounded in accurate predictions of patients’ needs and healthcare systems’ and communities’ capabilities for these events. There is wide variability in the predicted distribution of types and severity of respiratory failure and the characteristics of the affected populations (eg underlying chronic obstructive pulmonary disease [COPD] or previously healthy). This is where the direct evidence base is thinnest.

When an event occurs, the newly available data may suggest a better PPV strategy than ours for that event. Unfortunately, waiting for the disaster to occur to develop the evidence-based strategy “just-in-time” will probably prove to be “just too late,” and many patients may not have access to a life-sustaining intervention. Our surge-event PPV recommendations were developed to apply across the broad range of mass-respiratory-failure scenarios. The extensive investment for equipment procurement and maintenance, logistics planning, and end-user training requires surge PPV concepts.
to be sufficient across the range of plausible scenarios, rather than having different solutions for different hazards. Surge PPV equipment must therefore be sufficient for "airway protection/neuromuscular ventilatory failure (botulism and trauma), air-flow obstruction (inhalation exposure in patients with underlying airways disease or nerve-agent exposure), and, most importantly, hypoxic respiratory failure due to pneumonia, large hemorrhagic effusions (eg, anthrax), or acute lung injury (ALI) and acute respiratory distress syndrome (ARDS) (eg, plague, influenza, non-water-soluble chemical inhalation, trauma, neutropenic sepsis during acute radiation syndrome, and possibly anthrax).

In the setting of insufficient direct evidence, optimal surge PPV planning has been compiled from numerous related fields and extrapolated to disaster scenarios to develop defensible strategies. Because data must be assembled from divergent fields (eg, critical care, disaster medicine, virology, public health), national and international panels with broad ranges of expertise have been convened to develop PPV surge guidance. Their recommendations have been derived from published literature (when available), opinions of experts in disaster management, and experience of caregivers who have participated in care of patients with severe febrile respiratory illness. The groups consistently caution against deliberate liberal use or stockpiling of NIV PPV equipment. These recommendations have been vetted through medical societies and published in peer-reviewed journals. The recommendations in our paper were crafted from the iterative efforts of those groups.

The most recent reviews of NIV support its everyday use for acute respiratory failure in hemodynamically stable patients without ongoing cardiac ischemia due to COPD exacerbation, cardiogenic pulmonary edema, or ALI/ARDS in immunocompromised patients. NIV probably also has a role in postoperative respiratory failure who can remain unintubated or ALI/ARDS in immunocompromised patients. NIV may have extensive experience with NIV in France. But that explanation is not compelling, but they may not be generalizable to all patients with respiratory failure, since 64 of 172 eligible patients (hypoxemia on 50% oxygen via air-entrainment facemask) were excluded. Some were unable to cooperate with NIV due to agitation ($n = 45$), had severely depressed consciousness ($n = 5$), hemodynamic instability ($n = 4$), or required immediate intubation ($n = 10$). Also, the intubation and mortality rates were not better with NIV than with the standard treatment in the approximately 15% of patients with ARDS. In contrast to the Ferrer et al study, which found benefit from NIV in selected patients with pneumonia, Honrubia et al found no apparent benefit from NIV in patients with pneumonia. As in the other studies, a substantial proportion of patients with respiratory failure were unable to be randomized.

The above-described studies may be optimistically interpreted as indicating that NIV has a role in selected patients with hypoxic respiratory failure, but NIV’s utility in all patients with respiratory failure due to pneumonia and ARDS remains suspect. Of course, the use of NIV in hemodynamically unstable patients is even more uncertain. For a 20-day period in 2002, in 70 ICUs in France, 1,076 of 1,943 (55%) patients required PPV. 74.9% were intubated prior to or at ICU admission. 55.8% of those who received NIV (12.9% of all who required PPV) were able to forestall intubation. Patients with de novo respiratory failure who had a respiratory rate > 35 breaths/ min and $P_{O_2}/F_{O_2} < 200$ mm Hg were more likely to fail NIV. Again, NIV may have benefited a small subset of patients in this study of everyday practice, but, overall, NIV was not an appropriate option for the overwhelming majority of patients. This is probably in part because patients with conditions less likely to respond to NIV (eg, de novo respiratory failure [42%]) were more common than patients with acute-on-chronic respiratory failure (16%).

Another explanation could be that the limited overall benefit of NIV was due to missed opportunities caused by underutilization of NIV in France. But that explanation is not supported by recent reports from ICUs that have extensive experience with NIV. In those ICUs the overall percentage of patients with ARDS and acute hypoxic respiratory failure who can remain unintubated is relatively small. Antonelli et al had a 50% success rate with NIV, after excluding patients with hypotension, excess secretions,
more than one organ failure, bleeding, and neurologic disturbances. Close inspection of the data reveals that, of the 479 patients who met the ARDS criteria, 322 (69%) were already intubated, due to altered mental status, inability to manage secretions, hemodynamic or electrocardiographic instability, severe trauma, and/or more than 2 organ failures. The remaining 147 patients (30.6%) were then studied, and in fact only half of those selected patients with ARDS (15%) were successfully ventilated with NIV. The experience that a high proportion of patients with hypoxemic respiratory failure cannot be successfully managed with NIV was also reported from a large academic center in the United States. Those authors and other recognized ventilator experts have therefore cautioned against liberal application of NIV to patients with ALI and ARDS. Since we anticipate that most victims will have pneumonia, ALI, or ARDS during most mass-respiratory-failure events, we caution against stockpiling PPV equipment specifically designed for NIV and not intended for invasive ventilation. In addition, the majority of devices designed for NIV are not capable of volume ventilation. Volume ventilation is the only breath type that has been demonstrated to have a mortality benefit in ARDS. Caution must even be applied when considering NIV devices that have a volume-control ventilation mode, because these devices are likely to perform less well than devices designed for invasive mechanical ventilation, when used for invasively ventilating patients with severe respiratory failure. Hence, we advise against stockpiling NIV ventilators, but we encourage re-purposing NIV ventilators already on hand for invasive ventilation, if no other option exists. That is, though NIV ventilators should not be stockpiled, NIV ventilators already present and that are capable of ventilating through an endotracheal or cuffed tracheostomy tube should be re-purposed during a disaster. We do not believe that we or most other clinicians are able to immediately discriminate between all patients who will tolerate NIV and those who will fail NIV and require emergency intubation. Already a large proportion will bypass NIV because most clinicians will not attempt to use NIV on hemodynamically unstable patients and those with multi-organ-dysfunction syndrome. Several studies have shown that, in patients with hypoxemic respiratory failure, a low \(P_{\text{aO}_2}/\text{FIO}_2\), after 1 or 2-hours, and a high Simplified Acute Physiology Score II score portend the need for intubation. The Simplified Acute Physiology Score requires several laboratory studies, and ideally is scored with the worst variables over 24 hours. Time and laboratory capacity may not be abundant resources during a mass-respiratory-failure event. Also, experienced staff are likely to be in short supply, and patients who fail NIV may not be identified at the 1 hour or 2 hour mark. We disagree with McCracken that early watching of patients is no longer resource-intensive. The data indicate that the first 2 hours of NIV is very important for getting the patient to tolerate NIV and to identify NIV failure. Even after the first day, intense observation is necessary. Scettino et al reported that 38% of NIV failures in hypoxemic respiratory failure occurred after 24 hours, and Antonelli et al reported that 30% of failures occurred after 48 hours. A high proportion of these patients required intubation, and their mortality rate was high. Even if NIV equipment and adequate numbers of appropriately trained staff are available, we would still caution against planning to use NIV to ventilate patients with respiratory failure, including those who have conditions that are more likely to respond to NIV (COPD exacerbation due to viral respiratory infection). McCracken appropriately highlighted the uncertainty regarding NIV and secondary transmission of respiratory infection. Some facilities successfully used NIV during the severe acute respiratory syndrome (SARS) epidemic, albeit with many modifications (expiratory filters in rooms with negative pressure, > 8 air exchanges per hour, and powered air-purifying respirators), others restricted use, and some reported a possible mode of transmission. If the pandemic strain of influenza is able to bind to receptors on the proximal respiratory epithelium, then perhaps transmission can occur via NIV. Also, if we apply McCracken’s methodology to all personal protective equipment, we could argue that the lack of secondary transmission of SARS in the United States, despite inadequate use of personal protective equipment, indicates that even basic personal protective equipment is not needed. Paramedics are not obligated to rescue a person in an unsafe building (eg, fully involved with fire or structurally unsound), even if the victim would clearly benefit from assistance. Similarly, clinicians should wear personal protective equipment when providing respiratory care for patients with contagious diseases for which effective prophylaxis is not available and that can cause severe disease. The data are limited and inconclusive on the risk of SARS transmission via NIV, and we do not know if NIV will be implicated in transmission of other pathogens. We therefore stand by our recommendation not to plan for widespread use of NIV, even with existing equipment, when a contagious pathogen is suspected. We find no data in the literature to change our stance regarding NIV. The basis of our recommendations includes provision of best care for the patient and safety for the caregivers.

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REFERENCES

LETTERS TO THE EDITOR


Blow-By Revisited

Respiratory care has changed substantially since I began my career as an “inhalation therapist.” Intermittent positive-pressure breathing with a handful of medications was the predominant treatment. Today, respiratory therapists (RTs) utilize a wide range of drugs and aerosol devices supported by evidence-based research. What has not changed is our primary choice of interfaces: mouthpiece or mask. Disposables aside, there is little difference between a 1970 and 2008 era mouthpiece or mask.

One “interface” between the nebulizer and the patient has undergone dramatic changes: the RT. RT education has transitioned from “on-the-job oxygen orderlies” to associate and bachelor of science degree programs, with a few graduate-level schools. For my purpose, it is the RT who chooses the appropriate interface for an infant. Unfortunately, infants are not familiar with the current literature, they don’t know that a mouthpiece is the best interface, nor do they care that a “well fitting” mask is the next best. Infants come with a wide variety of temperaments; a few, with a modicum of care, will let you put a mask on their face and will even tolerate it for the time it takes to deliver the medication. However, for a variety of reasons, a substantial number 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. Fortunately, RTs are familiar with the literature that supports an alternative delivery method: blow-by.2-8

The delivery and measurement of drug deposition in an infant lung model or in vivo is as much art as science, as reflected by the wide range of results in the literature. Estimates for blow-by range from negligible to greater than 100% of a mask-delivered dose,5 the wide range due to differences in nebulizers, blow-by technique, distance from the patient, and measurement methods. The results of the research support the use of blow-by via T-piece or corrugated tubing held half an inch (1.27 cm) or less from the face, as a technique in those infants for whom a mask is not practical.2-5,7

Delivery of aerosolized medication to pediatric patients will continue to be a challenge that requires further research into the best techniques, interfaces, and the variables that the RT can control at the bedside. It is critical that RTs and physicians maintain familiarity with the current literature on treatment techniques and medications. However, for a specific patient, research can only provide guidance as to the appropriate technique. It is the role of the RT to evaluate the efficacy of the treatment regimen: Is the patient’s work of breathing reduced? Are there fewer reiterations? Is the respiratory rate lower? Are breath sounds improved? It is the RT at the bedside making a post-
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.

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

REFERENCES

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
LETTERS TO THE EDITOR

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. 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 nebulizer. 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

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. 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_{t} 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_{low}, the tidal volume generated could be much larger than the clinician-set target tidal volume...” Without proof that target tidal volume is not a value that is set when using APRV.

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 this is related to the re-establishment of functional residual capacity by APRV, which thus begins spontaneous inspiration from a higher lung volume. In reference to the article that Myers and MacIntyre quoted 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. 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 are of concern to me. A careful read of that article indicates that the study’s findings are unrelated to 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.” 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. 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

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 extoll 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_spigh 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-re-recruitment 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 al2 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.

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