neophytes. There are, however, some minor additional items for consideration. First is the use of the terms "accelerating" and "decelerating" to refer to inspiratory flow waveforms. These terms have been introduced and propagated by ventilator manufacturers with little regard for linguistic accuracy. The terms "ascending ramp" and "descending ramp" are more appropriate than "accelerating" and "decelerating flow." The term "ramp" is borrowed from electronic engineering and is preferred for three reasons. First, the name "ramp" gives a more obvious visual image of actual shape of the waveform. Second, "ramp" has been described mathematically and used universally in other disciplines for much longer than mechanical ventilators; have been in existence. Third, the analogy of something accelerating or decelerating is misapplied. For example, when a car is moving we say it has a certain speed, ie, speed = δdistance/δtime. If the speed increases with time, then we say that the car accelerates, ie, acceleration = δspeed/δtime, not that the speed accelerates. The speed of moving gas is expressed as a flow rate, ie, flow rate = area of tube × δdistance/δtime. If the flow rate increases, we would properly say that the gas accelerates, ie, acceleration = δflow rate/δtime, not the flow accelerates. In scientific terms, "the acceleration of a particle is the rate of change of its velocity with time."9

Finally, the usage of the symbol Paw for "mean airway pressure" seems inappropriate. The mathematical convention of using a bar above a letter signifies the mean of the variable the letter represents. A bar above "aw" thus symbolizes "mean airway," but there is no such thing as a "mean airway." What we want to express is mean pressure in the airways. Thus, the symbol should be Paw. This convention has been adopted by the medical journal Respiratory Care.

In conclusion, I hope the authors do not interpret my comments as a personal attack. They are meant only to stimulate open, rational discussion about a topic that affects everyone writing and reading about the subject of mechanical ventilation.

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REFERENCES
2 Chatburn RL. A new system for understanding mechanical ventilators. Respir Care 1991; 36:1123-55
3 Chatburn RL. Classification of mechanical ventilators. Respir Care 1992; 37:1009-25
4 Consensus statement on the essentials of mechanical ventilators-1992. Respir Care 1992; 37:1000-08
5 Chatburn RL. Two vs four breath types for ventilator classification [letter]. Respir Care 1993; 38:207-09
6 Marini JJ. Pressure-targeted mechanical ventilation of acute lung injury. Semin Respir Med 1993; 14:262-69

To the Editor:

We thank you for the opportunity to respond to Mr. Chatburn's comments. In his enthusiasm for his newly proposed ventilator classification scheme (Respir Care 1991;36: 1123-55 and Respir Care 1992; 37:1009-25) and redefined nomenclature for breath types (Respir Care 1993; 207-09), he may have overlooked several important issues. First, we did not "raise the issue of terminology" as he says. Rather, because of confusion, some of which results from terminology, our intent in writing the article was to clarify the physiologic and mechanical principles specifically involved with pressure-preset ventilation (Chest 1993; 104:900-99 and Chest 1993; 104:904-12). In Part 1 (Chest 1993; 104:900-99), we differentiated what terms we preferred to use for our discussion—terms that are commonly used and well-known. We think this was an especially appropriate strategy because of the nature of our article—it is a review article. By definition, a review article is an analysis of previously published scientific works on a particular subject. Incorporating novel classification schemes or new nomenclature into such a discussion, even if it were logical or clear, could create further confusion. With this in mind, we feel Mr. Chatburn's assertion, that we "have contributed to the confusion," is, at best, overstated and perhaps self-serving. On a more practical note, early drafts of our manuscript have been completed and were undergoing editorial revision by the time Mr. Chatburn began to promote his recommendations. Therefore, none of the authors had knowledge of them. Further, none of the five impartial experts in the field of pulmonary physiology and mechanical ventilation who reviewed our manuscript, two from other departments at the University of Florida and three selected by Chest editors, suggested that we read the articles he cites or expressed concerns about our terminology.

While it is extremely unlikely that our use of the term "exponentially decelerating flow waveform" confused anyone, we do agree with Mr. Chatburn that flow does not decelerate per se. The term "descending ramp," however, is equally, if not more, misleading—except when describing volume-controlled ventilation. A ramp waveform describes a linear decrease in flow over time, that is, a first order function. This is precisely the type of pattern a volume-control ventilator produces when it is set to decrease flow throughout inspiration. The term "ramp," however, fails to describe accurately the flow profile produced in pressure preset ventilation. The exponentially diminishing flow pattern in this type of ventilation is often curvilinear, a higher order function—in fact, it is both visually (Fig 1) and physiologically

**FIGURE 1.** Inspiratory flow waveforms produced during volume-controlled ventilation (dashed line) and pressure preset ventilation (solid line). These waveforms are typical of those produced under the following constant conditions: tidal volume of 600 mL, inspiratory time of 1.6 s, respiratory system compliance of 0.025 L/cm H2O, and airway resistance of 16 cm H2O/L/s.
detectable from a simple ramp. Further, as mentioned, because it was not our purpose to rectify terminology problems in the field of mechanical ventilation, we intentionally used conventional terms.

Mr. Chatburn writes passionately and convincingly concerning others' errors in linguistic accuracy, but in doing so he himself falls victim to errors of other types as well as linguistic errors. For instance, flow and volume are not inverse functions: volume is determined by integrating a flow signal, not inverting it. Furthermore, all of the equations cited in our paper were not derived from the equation of motion—many were derived from Ohm’s law. Another point involves the article cited by Mr. Chatburn titled “Consensus Statement on the Essentials of Mechanical Ventilators” (Respir Care 1992; 37:1000-08). This Consensus, which was actually formed among 15 persons, denotes agreement among 8 to 15 people, not agreement of the entire community of professionals who are concerned with these issues. To imply this “Statement” was a general consensus of the entire community is an overstatement. Finally, Mr. Chatburn’s statement that “preset... implies that the ventilator attempts to achieve a particular value at one point in time, as in preset tidal volume” shows his bias. Our use of the term implies nothing beyond its simple definition: “to set (something, esp. the controls of an automatic apparatus) beforehand.”

While we did not take any of Mr. Chatburn’s comments personally and think that some of his ideas presented elsewhere may have some merit, a more straightforward approach to presenting his ideas may be a better way to stimulate open, rational discussion about them.

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REFERENCE

Detecting Pulmonary Aspiration of Enteral Feeding in Intubated Patients

To the Editor:

We read with interest the article by Potts et al., which appeared in the January 1993 issue of Chest. The conclusions of the authors are the same to those expressed by us in a similar work published in 1992. In our work, we also performed a prospective study to compare the utility of glucose oxidase test strip readings in tracheal secretions vs the direct visualization of tracheal secretions over 70% of methylene blue to enteral feeding formulas. In 38 intubated, mechanically ventilated, adult patients receiving enteral nutrition via nasogastric catheter, we performed 448 measurements of glucose concentration in bloodless tracheal secretions with the same method used by Potts et al; we found glucose readings ≥20 mg/dL in 52% of the specimens analyzed. On the other hand, and equal to the results described by Potts et al, the appreciation of blue tracheal secretions was minimal (≤1%). These results could corroborate with the conclusion of Potts et al. “the addition of blue dye to enteral formulas with inspection of tracheal secretions for blue discoloration is inadequate for the detection of enteral feeding aspiration and should be replaced by glucose oxidase test strip methods.”

Nevertheless, in the work of Potts et al, the glucose oxidase method was applied after the addition of blue to the enteral feedings. To do so would result in a false elevated reading of glucose concentration in tracheal secretions from the coloring agent. For example, in our experience, a similar setup (Abbott Laboratories) has a glucose oxidase test strip value of 20 mg/dL in “basal conditions” but a twofold value (40 mg/dL) when 1 mL of 1% methylene blue is added to 500 mL of diet. Although the value needed to define positive glucose reading is the same in the article of Potts et al and in our work (≥20 mg/dL), in our opinion it seems more correct to define “positive glucose reading” according to the glucose reading of the diet administered. “Positive glucose reading” would be defined as a tracheal secretion specimen, which has a glucose concentration equal to or superior to the glucose concentration of the diet used. Table 1 of the paper by Potts et al clearly shows the differences in glucose concentrations between the four enteral formulas they studied.

Another aspect of interest is the variation of results related to the use of different glucose meters. In our previous work, we used the Glucometer II, 5631 system, (Bayer Diagnostic), but according to current hospital policy, we are using the Accutrend (Boehringer) glucose meter. Surprisingly, the glucose readings obtained by the two systems for each diet analyzed in our ICU are not the same, probably because we are using the glucose meters for a purpose different to the original.

With the criticisms above expressed and according to our experience, we request the conclusions of Potts et al and await for the acceptance of a method of diagnostic help in detecting aspiration of enteral feeding in intubated patients and possible therapeutic implications.

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REFERENCES

To the Editor:

We appreciate the comments of Dr. Montejo-Gonzalez and colleagues and agree that inspection of tracheal secretions for blue discoloration is inadequate for the detection of enteral feeding aspiration in intubated adults. Dr. Montejo-Gonzalez questions the threshold glucose concentration (≥20 mg/dL) used in our study to identify contamination of tracheal secretions by enteral formulas (Chest 1993; 103:117-21). In their apparently similar study (Nutri Hosp 1992; 7:145-49), Dr. Montejo-Gonzalez et al reported a 20 mg/dL increase in glucose concentration as measured using a Glucometer II device and glucose oxidase test strips