CORRESPONDENCE

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Cost Savings in the Operating Room

To the Editor.—Because of the growing costs of medical care, we have been asked to modify our practices to be more fiscally responsible. In our area, the operating room, we have undergone periodic operations improvement (OI) efforts to reduce unnecessary expenses. Nurses have been replaced with technicians, and physicians have been asked to work "more efficiently."

We have found a simple way to significantly reduce expensive operating room time without jeopardizing patient care. Rather than moving patients on the count of three ("1-2-3" move) as had been our practice, we now count only to two ("1-2" move). Because for every case, each patient is moved to and then from the operating room table we now save 2 s per patient. We have 30 operating rooms, each with an average of 5 operations per day, so our projected savings are 180 s or 3 min per day. Approximately 600 min can be saved over the course of a year by this simple maneuver. Our operating room time costs $20/min. Thus, we can save $12,000 per annum by counting only to two. More importantly, the additional 10 h of operating room time is sufficient for another three to five cases to be performed.

With the acceptance and success of the "move-on-two" maneuver, we have initiated a pilot study of a "move-on-one" maneuver. Initial reports suggest that this can be just as safely and successfully done and will lead to a doubling of efficiency (i.e., saving time and money) over the next fiscal year.

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Efficacy of Tracheal Gas Insufflation during Expiration in Reducing $\text{Pa}_\text{CO}_2$

To the Editor.—We read with great interest the article by Kalton et al.,1 which demonstrated the efficacy of tracheal gas insufflation (TGI) during expiration (respiratory washout) in reducing $\text{Pa}_\text{CO}_2$ without increasing the prescribed tidal volume. The authors presented an effective method of enhancing mechanical ventilation in patients with severe respiratory failure. The use of distal airway pressure monitoring, as used by the authors, is of clinical importance during TGI administration because it provides accurate airway pressure measurement.

There are, however, certain comments we would like to make. Although gas insufflation was synchronized with expiration, it resulted in a significant increase in peak, plateau, and mean airway pressures, signifying an increase in end-expiratory lung volume and possibly in minute ventilation as well. The measurement of respiratory volumes and auto-PEEP during TGI is problematic and, at present, not feasible at bedside. The assessment of tidal volume during TGI is problematic because it is difficult to say whether the gas entering the trachea and the smaller airways adds to the tidal ventilation or participates only in airway flushing during expiration. The measurement of auto-PEEP is problematic because it requires accurate timing of the closure of the inspiratory and the expiratory valves and the cessation of TGI flow. For these reasons, it is difficult to draw conclusions about the efficacy of the ventilatory mode when improvement in ventilation is associated with a significant increase in airway pressure.

To resolve similar problems encountered when trying to compare TGI with two different gases, helium and oxygen, we devised a "coefficient of efficiency" to assess the effect of TGI at different flow rates and with different gases". The change in $\text{Pa}_\text{CO}_2$ divided by the change in peak airway pressure. This coefficient, with its easily measured variables ($\text{Pa}_\text{CO}_2$ and peak airway pressure), helped us to compare different strategies of ventilation according to their ability to clear $\text{CO}_2$ with minimal lung distention.

In addition, we would like to suggest that the term TGI be used in the future to describe all modes of ventilation in which $\text{CO}_2$ clearance is enhanced by intratracheal gas flow during part or all of the respiratory cycle.

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This letter is featured in "This Month in Anesthesiology." Please see this issue of Anesthesiology, page 4A.
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Expiratory Washout in Patients with Severe Acute Respiratory Distress Syndrome

To the Editor.—We read with interest the report by Kalfon et al. regarding the use of expiratory washout in patients with severe acute respiratory distress syndrome.1 Although interesting and stimulating, we believe that several points should be discussed.

In our opinion, the indications for the use of expiratory washout (EWO) or tracheal gas insufflation (TGI) can be summarized as situations wherein there is profound hypercarbia, resulting in hemodynamic or acid–base compromise, associated with high airway pressures that cannot be safely increased for fear of barotrauma (e.g., severe ARDS) and secondly wherein there are high airway pressures that cannot be reduced as a result of the contraindication of hypercarbia (e.g., lung injury in association with head injury). In the first situation, the aim of EWO is to reduce $P_{acO_2}$ but at the same airway pressures. In the second situation, the aim is to decrease airway pressures without allowing a resultant increase in $P_{acO_2}$.

In their study, Kalfon et al. have used EWO for none of the previous indications. Their findings of $P_{acO_2}$ decrease could easily have been reproduced by simply increasing tidal ventilation. This would have provided CO$_2$ decrease and would have led to similar airway pressure changes, i.e., increased peak, plateau, and mean tracheal pressure. We are concerned that an uncritical reading of this paper will bias readers against TGI because of the significant increases in airway pressures associated with the mode in which EWO has been used by the authors. To most clinicians, these significant changes in airway pressure would be unacceptable.

Some of these changes in airway pressures could have been avoided by the use of pressure-controlled ventilation (PCV) rather than volume-controlled ventilation. They have recently completed a similar study of EWO in an animal model, in which we used EWO at a flow of 6 l/min combined with PCV.2 We observed a 14% reduction in $P_{acO_2}$ but with no increase in peak airway pressure. Mean airway pressure increased by a nonsignificant amount as a result of intrinsic PEEP caused by EWO. We had the capability of measuring this increase in PEEP, unlike Kalfon et al., by the synchronized sup-

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