
Erratum

To the Editor:

I apologize for the typographical error in my paper, “Right ventricular volumes by thermodilution in the adult respiratory distress syndrome,” published in the July issues of Chest (1985; 88: 34-39). The formula given on page 35 should be:

\[ K = C_1/C_2 + C_2/C_3 + \ldots + C_n/C_n-1 \]

n = 1

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Optimal Use of MDIs

To the Editor:

In his article, “Spacer devices used with metered-dose inhalers” (Chest 1985; 88:276-84), Peter König has provided an excellent review of the state-of-the-art with respect to these MDI add-on units. In table 6, which compares clinical trials using the various devices, we would like to stress the point that the dose delivered to the lung using the Aerochamber plus MDI was similar to the dose delivered when the MDI was used optimally, i.e., held 3 cm in front of the widely opened mouth with the aerosol bolus inhaled from FRC to TLC, followed by a ten second breath hold. This method of using the MDI delivers about twice as much aerosolized medicine to the lung than when the MDI is discharged between closed lips. Therefore, the latter method was used for MDI aerosol delivery in the Breathecan (tube spacer) study, it is not surprising that pulmonary deposition with the device was greater than when the unaided MDI was used. The closed mouth MDI administration technique was also used as the “gold standard” in the InspriEase study.

As one of the co-authors of the study suggesting possibly less efficacy when steroid aerosols are administered via the Aerochamber, it should also be stressed that this information appears in abstract form only and will not be published since patients, misunderstanding the instructions for Aerochamber use, sprayed several puffs consecutively into the device before inhaling. Since 20 ml of freon and drug are ejected with each puff, substantial amounts of the total dose were lost. In a more recent study without these limitations, the beclomethasone MDI with Aerochamber was found superior to the unaided MDI in the delivery of medication with respect to both improved efficacy (twice as many patients could discontinue systemic steroid therapy while using MDI plus Aerochamber) and decreased incidence of Candidiasis infection (22 percent in the MDI only group vs no infection in the MDI plus Aerochamber group).

It appears from this comprehensive article that there is little difference in efficacy between the various devices. Selection of an MDI add-on device in clinical practice is probably best determined by ease of use, assurance of drug delivery, portability and cost. However, these considerations should not detract from the importance, in general, of spacer devices in the therapeutic regimen of obstructive airways disease for many patients. In our opinion, the major importance of MDI with add-on devices for the delivery of aerosol therapy to children continues to be grossly underestimated. Spacers are simple to use, inexpensive and independent of an external gas or power supply, unlike nebulizers, whose per treatment cost is much higher and incurs the added potential of nosocomial infection. Thus, add-on devices are neither “break-through nor gimmick,” but a valuable adjunct to therapy in selected patients.

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REFERENCES

To the Editor:

The letter by Newhouse and Dolovich makes a number of points. It is possible that they are correct in their explanation of better results in some studies with tube spacers because they were compared with the "closed lips" technique of MDI use. However, an equally distinguished group of investigators considers the "closed lips" technique better than "open lips." In my opinion, it is still not certain which method works better for adults, but in children, because of the possibility of faulty aim when holding the inhaler 3 to 4 cm in front of the mouth, I prefer to use the "closed lips" technique.

With regard to the possible reduced clinical efficacy of corticosteroids with aerochamber, the footnote in table 6 clearly stated that it was based on a single abstract, and I am grateful for their clarification of possible problems in that study.

I quite agree with Drs. Newhouse and Dolovich that spacers are underused and are useful in selected patients, as I concluded in the summary of my review article.

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REFERENCES
1 Newman SF, Clarke SW. The proper use of metered dose inhalers. Chest 1984; 86:342-43
Snoring and Sleep Apnea

To the Editor:

I read with some interest the editorial entitled “Sleep apnea: when does better become benefit?” (Chest 1985; 88:320-21) by Drs. Sanders and Rogers. I agree with their summary conclusions that much study is needed in this area.

I take exception to their implicit presumption that patients with mild sleep apnea or “silent sleep apnea” might not benefit from treatment.

As an academic otolaryngologist working closely with sleep disorders groups in two University communities, I have carefully looked at these issues. Patients who present for correction of socially disruptive snoring, both with positive and negative polysomnogram results, appear to benefit from uvulopalatopharyngoplasty. In my practice, these patients are among the happiest post-operative patients. The operation does an excellent job ameliorating snoring. In addition, these patients tell me that they feel the quality of sleep is improved.

I have also come to believe that sleep apnea syndrome is a broad spectrum disease. Patients do not suddenly become sleep deprived pre-pickwickian. There is a gradual progression from mild to severe sleep disorders obtainable just by history from patient and spouses. My impression is that surgery on patients with mild sleep apnea may not only correct mild symptoms, but may prevent them from disease progression to the point where the disease destroys their life.

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To the Editor:

There is reasonable evidence that snoring and upper airway occlusion during sleep are pathophysiologically related, although on different ends of a spectrum of airway dysfunction.1 The point at which the process becomes a disease is controversial.2 We agree with Dr. Colman that the amelioration of snoring is beneficial in the select population of individuals who are troubled to the degree that they will accept a surgical procedure. Although much remains to be learned about the physiologic impact of snoring, Dr. Colman is treating a social problem. It is clear that the distinction must be made between treating the cosmetic and physiologic consequences of upper airway dysfunction. The indications for therapeutic intervention and the criteria for therapeutic success are obvious in the former but not so nearly well-defined in the latter. In this regard, it is also important to remember that relief of snoring does not necessarily translate into improved breathing during sleep.3 Our editorial concerned the uncertainties regarding the therapy of sleep apnea per se, not its audible manifestations.

While patients with sleep apnea syndrome frequently report years of snoring prior to the onset of sleep apnea-related syndromes, we know of no data indicating the frequency with which snoring progresses to the sleep apnea syndrome. Similarly, we know of no information indicating how frequently, if ever, surgical correction (uvulopalatopharyngoplasty) of mild, asymptomatic sleep apnea prevents the development of more severe sleep-disordered breathing later in life. It is currently not justified to recommend an operative procedure in a snorer or an asymptomatic sleep apnea patient based on the assumption that it will prevent the evolution of severe sleep-disordered breathing.

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References


Treatment of Pleural Effusion

To the Editor:

We read with interest the paper of Dr. Reshad and colleagues (Treatment of malignant pleural effusions, Chest 1985; 88:393-97) on the treatment of malignant pleural effusions. In their conclusion, they state that "treatment of malignant effusions is mostly palliative for alleviation of the symptoms, the method selected for treatment should be highly effective and with low morbidity and mortality." We strongly agree that the palliative treatment of a terminal condition should have low morbidity and close to zero rate of mortality. Our current experience has reinforced the data reported by Rubinson,1 Wallach,4 and Sahn,2 in that, with proper technique, tube thoracostomy drainage and tetracycline instillation is effective treatment (87 to 100 percent) for symptomatic pleural effusions. The paper by Reshad et al therefore prompted the following comments:

With adequate tube drainage in the properly selected individual, tetracycline instillation leads to excellent symphysis of the pleural space, eliminating the persistance of malignant effusion, which apparently was present in Dr. Reshad’s patient group, since serial thoracenteses were performed in the majority of the patients.

Furthermore, the authors never comment on systemic side effects of the anti-neoplastic agents that were instilled into the pleural space; we suspect that this must be of concern. In addition to constitutional symptoms, interference with radiation or systemic chemotherapy may be a problem. Chest pain, the most common adverse effect of intrapleural tetracycline, usually is minimal in premedicated patients with malignant effusions. We would like to note that the expense of intrapleural instillation of tetracycline (20 mg/kg) is much less than the cost of any of the chemotherapeutic agents reported.

We find it remarkable that the average duration of chest tube drainage was 16 days, when in our experience effective tube drainage of the pleural space is usually achieved within 24 to 48 hours. Subsequent intrapleural instillation of tetracycline allows tube withdrawal after an additional 24 hours. This marked difference in duration of a chest tube placement has clinical (increased morbidity, prolonged hospitalization, increased risk of pleural space infection),

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