LETTERS TO THE EDITOR

November 22, 1982

Dear Editor:

I enjoyed reading Doctor Francis D. Moore’s historical sketch of the early years of the Harvard Medical School (Ann Surg 1982; 196:525–535). I should like, however, to call attention to an error of fact. The date of the founding of Brown University Medical School was given as 1821, presumably derived from Thacher. The Medical School was, in fact, founded in 1811 and survived until 1827. Graduates were listed for every year from 1812 through 1827. The school was without a Professor of Anatomy and Surgery from the death of Dr. John M. Eddy in 1817 until the advent of Dr. Usher Parsons in 1822, but the school continued to function during that period.

SEEBERT J. GOLDSWY, M.D. Providence, Rhode Island

References


December 13, 1982

Dear Editor:

I appreciate the comments of Seebert Goldowsky. Each medical school has special details about its founding and its effective date of beginning. Maybe we can get some of the standard date lists properly corrected.

Certainly his dates for Brown University School of Medicine seem to be impeccable and unassailable. With many thanks.

FRANCIS D. MOORE, M.D.
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November 23, 1982

Dear Editor:

We are writing in response to the article by H. David Reines et al. (Ann Surg 1982; 195:451) regarding the effect of chest physical therapy on the prevention of atelectasis in children following cardiac surgery. The study design and results were acceptable; however, the misleading conclusion and title of the article are of major concern. The operational definition of chest physical therapy, as implied by the authors, is both narrow and ambiguous. The reader is led to believe that chest physical therapy means only gravity-assisted positioning with chest percussion and vibration. This narrow definition which is often found in scientific literature fails to acknowledge the multifaceted scope of chest physical therapy practice. Participation of a qualified physical therapist in the study might have prevented the utilization of an erroneous operational definition.

The authors presume that chest physical therapy means only Trendelenburg positioning with percussion and vibration. The control group, in fact, also received a therapeutic regimen of breathing exercise, suctioning, and coughing, which falls under the scope of chest physical therapy. The only difference between the management of the two groups was that the experimental group received secretion removal techniques such as gravity-assisted bronchial drainage, chest percussion, and vibration. Physical therapists involved with therapeutic chest care, through critical review of literature and clinical experience, reserve secretion mobilization techniques only for those surgical patients with secretion retention and not as a prophylactic approach. The majority of the patients in the Reines et al. study appeared to be in the 3- to 5-year age range. Children of these ages generally benefit most from deep breathing through blowing games, cough instruction, bed mobility, upper extremity and thoracic mobilization exercises, and ambulation. Positioning for bronchial drainage with chest percussion and vibration should not be part of their postoperative chest physical therapy program, unless a significant pulmonary history or secretion retention dictates otherwise. We agree with the authors that these vigorous techniques could increase pain with subsequent splinting of the thorax and lead to decreased chest mobility and hypoventilation.

Therefore, we feel criticism of the authors’ choice of terminology is imperative. A more accurate title of their article would be “Prophylactic Use of Bronchial Drainage Techniques Fail to Prevent Postoperative Atelectasis in Children after Cardiac Surgery” or “Utilization of Bronchial Drainage Techniques Other than for Purposes Designed Can Be Deterrent.”

There is evidence that chest physical therapy as a multifaceted approach aids in decreasing postoperative pulmonary complications, especially in the high-risk surgical patients, including those with cardiac lesions.

Little has been done with the pediatric surgical patient, and we applaud the authors for their research efforts in this area. However, accuracy in operational definitions of treatment regimens, particularly when one is the main focus of an investigative study, is essential. Care must be taken not to mislead the scientific community.

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References

2. Connors AF, Halmon WE, Martin RJ, Rogers RM. Chest physical therapy; the immediate effect on oxygenation in acutely ill patients. Chest 1980; 78:559–564.

December 17, 1982

Dear Editor:

We carried out our investigation to answer an important question pertinent to the practice of surgery. Though physical therapists might find our use of the term “chest physiotherapy” narrow and ambiguous, we doubt that many surgeons were misled. As Ms. McCarthy and co-workers point out, our definition of chest physiotherapy is often found in scientific literature and is widely understood by surgeons, so it is appropriate in the context in which we used it.