were CEA negative and Leu-M1 or B72.3 positive have been reported. These latter two antibodies were not used by the authors. Furthermore, ultrastructural studies have been found useful by a number of authors, but were not employed in the study by Beauchamp et al. Some of these tests may not have been performed due to the limitation of the sample size. This merely serves to underscore the diagnostic limitations of closed-needle biopsy of the pleura.

It has been my experience that needle biopsy of the pleura is only occasionally sufficient to make an unequivocal premortem diagnosis of malignant mesothelioma, and then only after careful correlation with clinical and radiographic features. The study by Beauchamp et al does not refute that position, and the readers of Chest should not conclude that closed-needle biopsy in most cases provides the pathologist with sufficient material to make this oftentimes difficult diagnostic distinction.

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To the Editor:

We appreciate the comments of Dr. Roggli and agree with him that closed-needle biopsy is not always sufficient to make the diagnosis of malignant mesothelioma of the pleura.

There is no single diagnostic test, pathologic or otherwise, for malignant mesothelioma of the pleura. Some biopsy samples will require more pathologic testing than others to help separate malignant mesothelioma from adenocarcinoma. However, not all biopsy samples require all pathologic tests to be performed on them for a diagnosis to be made.

The histochemical stains Alcian blue and colloidal iron can be positive in both malignant mesothelioma and adenocarcinoma. In malignant mesothelioma, Alcian blue and colloidal iron can stain positive due to their reaction with hyaluronic acid. In adenocarcinoma, Alcian blue and colloidal iron can stain positive due to their reaction with chondroitin sulfate. Hyaluronidase can be used to help separate the two. Exposure of the tissue to hyaluronidase will digest hyaluronic acid present in a mesothelioma; on subsequent exposure to Alcian blue or colloidal iron, the mesothelioma will fail to stain. However, hyaluronidase will not digest the chondroitin sulfate present in adenocarcinoma; on subsequent exposure to Alcian blue or colloidal iron the adenocarcinoma will still result in a positive reaction.

There are many antibodies available for use in immunohistochemical testing to differentiate malignant mesothelioma from adenocarcinoma. Of these antibodies, CEA is the most useful. However, no single antibody is diagnostic. Although some authors have found electron microscopy to be useful, others have found this not to be the case. At the time of our diagnoses of malignant mesothelioma of the pleura, electron microscopy was not available at our institution.

In our study, we were able to make a diagnosis of malignant mesothelioma of the pleura in 20 cases utilizing some of the pathologic tests available. In these 20 cases, tissue was obtained at closed-needle biopsy of the pleura in 12. In 10 of these 12 cases a diagnosis of malignant mesothelioma of the pleura was made without subjecting the patient to an open pleural biopsy.

These findings show that the yield of malignant mesothelioma of the pleura by closed-needle biopsy, in their study, is higher than previously recorded. We suggest that this finding may be due to the improvement in pathologic tests that have become available in the past 10 years.

In light of these findings, we suggest that a closed-needle biopsy be performed before proceeding to open pleural biopsy when a diagnosis of malignant mesothelioma of the pleura is considered. The advantage to the patient, as well as the considerable reduction of hospital costs, in making a diagnosis by closed-needle biopsy is obvious. However, should a closed needle biopsy be nondiagnostic, we surely do not suggest that the investigation stop there. On the contrary, if a closed-needle biopsy is nondiagnostic for malignant pleural mesothelioma, open pleural biopsy or thoracoscopic study should be performed.

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Discordance Between Cardiopulmonary Physiology and Physical Therapy

To the Editor:

In the June 1992 issue of Chest, Dean and Ross provided an interesting noncritical review of a selection of literature relating to cardiopulmonary physiotherapy (CPP) and the lack of clinical trials demonstrating its efficacy. However, this narrow review does not reflect current practice or discuss the literature that clearly supports CPP interventions. I was unclear as to who was the target audience for this article, since it does not provide physiotherapists with new information and ignored many important aspects of CPP.

Current CPP practice does not have a primary focus on removal of secretions unless they are the only pathologic change. Positioning and mobilization are integral components of CPP and will frequently be the only intervention required. I am not sure to whom Dean and Ross were referring when they cautioned against "primarily attributing the underlying mechanism of atelectasis to
When which chain and pathology) other system. While it may be seen that atelectasis is the result of a number of different mechanisms. One cause of atelectasis is mucus plugging, and in this situation it is appropriate to use secretion removal techniques, alone or in combination with other CPP treatments. The authors' statement that "a primary focus on secretion removal cannot be supported" is misleading, since this is not the primary focus of CPP, but rather one aspect of treatment. In order to optimize the oxygen transport system, CPP must be focused on the underlying disease. The treatment chosen will depend on whether the disease is the result of inadequate ventilation, mucociliary clearance impairment, or respiratory muscle dysfunction.

In addition, Dean and Ross state that there is mounting evidence failing to support the use of secretion removal techniques. A noncritical review by Murray cited by Dean and Ross to support their position in fact concludes that trials of physiotherapy should be given if they successfully result in significant sputum production. While years ago physiotherapists may have thought it possible to effect a change through the use of "conventional" secretion removal therapy (CSRT) techniques, such as postural drainage, percussion, vibration, and coughing, on patients with minimal or no secretions, this does not reflect current practice. It is well accepted that patients without significant secretions (eg, those with COPD or cardiac pathology) do not respond to CSRT. However, there is significant support for the use of CSRT in patients with impairment of mucociliary clearance (eg, the critically ill and patients with cystic fibrosis and bronchiectasis) and as a first-line treatment for lung collapse. In addition, CSRT improves thoracic compliance, intrapulmonary shunt, and specific airway conductance.

Dean and Ross state that evidence fails to support percussion and vibration as efficacious procedures. Research in this area is inadequate and nonuniform, making it impossible to draw definite conclusions concerning the efficacy of specific techniques. In the future, treatments such as the forced expiratory technique, use of the positive expiratory pressure mask, and autogenic drainage may prove to be effective, alone or in combination with CSRT. A review of these techniques was carried out by Lapin in 1990.

The authors focus on the importance of the oxygen transport chain and assert that CSRT addresses only one part of the chain. However, CPP goals must also address other complications of retained secretions, namely, (1) preventing mucus plugging and lung collapse, which can result in cardiorespiratory compromise as a result of mediastinal shift; (2) reducing the potential for nosocomial pneumonia; and (3) improving pulmonary compliance and reducing the work of breathing.

The article discusses the association of CSRT with adverse effects. When CSRT is appropriately administered, however, patients are stable or improved with treatment.

Many studies have attempted to address the relationship between secretions and pulmonary function. Newton and Stephenson, cited by Dean and Ross, demonstrated an inconsistent relationship between secretions and pulmonary function. Other studies, however, revealed an improvement in pulmonary function. Although there is a real need for longitudinal studies on the benefits of secretion removal, it is dangerous to suggest that this is not an integral aspect of physiotherapy in some populations.

While current physiotherapy practice would not include CSRT interventions for stable COPD, it is interesting to note that some authors have shown benefits in this patient population.

The importance of a CPP focus on optimizing steps of the oxygen transport system is repeatedly emphasized by Dean and Ross. While physiotherapists would always have this as a treatment goal, the authors focused on only one aspect of the oxygen transport system. The goals of positioning, mobilization, and CSRT are to reduce physiologic shunting and to improve ventilation-perfusion matching, gas exchange, and pulmonary function. The potential effect of physiotherapy intervention on the partial pressure of arterial carbon dioxide, and thereby the affinity of hemoglobin for oxygen, and the beneficial effects of exercise on oxygen transport and delivery, for example, are not discussed.

In summary, until research clearly establishes the efficacy of different CPP interventions, it is premature to make strong conclusions. While it is accurate to point to the lack of research to support all aspects of CPP, it is incorrect to imply that physiotherapists do not keep abreast of research or base practice on sound physiologic principles. In my experience with undergraduates and qualified therapists, it is clear that physiotherapy practice is based upon sound reasoning, a comprehensive knowledge of physiologic principles, response to treatment, and critical application of existing research.

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To the Editor:

We appreciate having an opportunity to respond to Ms. O'Callaghan's response to our article, for it is only with such dialogue that the field can advance.

Ms. O'Callaghan concluded from our review that the work "does not reflect current practice or discuss the literature that clearly supports CPF interventions." Ms. O'Callaghan's conclusion was dead on; we were delighted that the message of the article and its focus was clear. However, as we read further, it became apparent that Ms. O'Callaghan viewed these attributes as liabilities rather than as the strength and significance of the contribution of this work to the advancement of the field.

First, the primary reason for the publication of the work and the urgency in having it published was indeed the fact that there is considerable discordance between the literature and cardiopulmonary physical therapy practice. The considerable demands placed on all health professions today to be accountable and cost effective must provide the stimulus for evaluating what it is that we do. It behooves those of us in the field to reconcile this very serious clinical dilemma and to avoid the danger of dismissing evidence that is contrary to conventional wisdom as bias or a narrow view. Without an unbiased mind and an openness and willingness to examine contrary evidence, particularly if it contravenes our own belief system, a field is doomed and can never advance. An opposing view that is based on the literature does not de facto constitute bias or a narrow view. We would argue that selective attention to the literature, or lack thereof, has permeated the practice of conventional chest physical therapy over several decades and that such a practice constitutes bias and a narrow view. For a field to grow, it is essential that one not only "look where the light shines" and selectively attend to literature that supports one's point of view. For a review to advance the field, it must critically evaluate the literature in a balanced manner, synthesize this literature, identify discrepancies, and attempt to explain these on a physiologic and scientific basis. This was our objective.

Second, the virtues of classic chest physical therapy have been extolled for several decades, often indiscriminately, which has contributed to the difficulty of evaluating them in an unbiased fashion. This influence within and outside the profession has been detrimental to the critical evaluation of the application of classic chest physical therapy. For example, many practitioners often have difficulty appreciating that a patient is being appropriately treated unless the clapping sounds of a tried-and-true conventional chest physical therapy emanate from behind the curtain. Such an attitude continues to be a considerable obstacle to scientific advancement and progression of the field.

We sincerely hope that with open minds, a willingness to engage in dialogue about opposing views based on their scientific merit, and receptiveness to examination of the evidence in an impartial manner, cardiopulmonary physical therapy will ascend to its rightful place as a clinical specialty in the physical therapy profession based on its established efficacy and cost effectiveness.

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