know this entity and include Sjögren's syndrome in their differential diagnoses of cough of undetermined origin.

One of our patients with transbronchial lung biopsy specimen showing lymphocytic infiltrates with follicular pattern is another example. She had normal pulmonary function, including DLCO. We included her in the diffuse interstitial disease group on the basis of this biopsy result, but if we only had pulmonary function study results she would have been considered normal. What is the clinical significance of these lymphocytic infiltrates? We consider them quite significant since they show involvement of pulmonary interstitium, and this fact alone classifies this patient as a case of the extraglandular form of Sjögren's syndrome, with everything that accompanies it. Close follow-up would determine if she develops clinically and functionally obvious interstitial lung disease. So far, two years later, she has not. Could this suggest that the patchy lymphocytic infiltrates of Sjögren's syndrome in the pulmonary interstitium do not interfere with diffusion, at least in early stages?

What is the significance of small airways involvement? Probably limited, we think, and thus the six flow/volume curves shown in our paper are only mildly deformed. Still, this involvement has been previously emphasized as indicating that "human airways should be considered another target organ of Sjögren's syndrome" in small series of patients and we thought that we should evaluate its significance in a larger series. Lack of specificity of the tests for small airways is one of the reasons why it is better to evaluate patients with autoimmune rheumatic diseases in comparison with normal control subjects and not with predicted values alone.

Finally, we think that if we consider pulmonary disease "clinically significant" only in the presence of incapacitating symptoms or very heavily impaired pulmonary function, the percent of pulmonary involvement in Sjögren's syndrome is probably lower than the 9 percent reported by Dr. Strimlan in 1976. In our report, though, we did not comment on whether the pulmonary involvement was clinically significant. We only wanted to define this involvement using as many parameters as possible exclusively in patients with primary Sjögren's syndrome. Most studies do not make a clear distinction between primary and secondary Sjögren's syndrome with the result that many respiratory manifestations attributed to Sjögren's syndrome may, in fact, be the result of rheumatoid arthritis in patients with secondary Sjögren's syndrome. For example, pleural effusion, mentioned in most pulmonary text(boks as a frequent manifestation of Sjögren's syndrome, was absent in all 36 patients of our series. Our conclusions remain the same: pulmonary involvement is frequent in primary Sjögren's syndrome and it extends from the pharynx and the trachea to the pulmonary interstitium. Whether it is "clinical significance" remains to be elucidated with follow-up studies involving patients with primary Sjögren's syndrome alone or in comparison with patients with other rheumatic disorders and healthy controls. Certainly, from our results we do not suggest that Sjögren's syndrome produces the incapacitating pulmonary manifestations of progressive systemic sclerosis or the life-threatening complications of systemic lupus erythematosus.

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References

Spacer Devices

To the Editor:
I appreciated Dr. König's excellent review of spacer devices which appeared in Chest (1985; 88:277). I would like to make two additional points. I have seen several patients who, because of local oropharyngeal and laryngeal irritation, could never tolerate inhaled beclomethasone but are now able, with spacer devices, to tolerate it without difficulty. The second point is that, as Dr. König has shown so well, the studies are contradictory. This may be because all patients are not equal and some may respond better to spacer devices than others. Therefore, in the difficult-to-manage asthmatic patient, I strongly recommend a trial of a tube spacer device.

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Contraindications for Chest Physiotherapy

To the Editor:
The excellent review entitled "Does chest physical therapy work?" (Chest 1985; 88:436-44) by Kirillof and colleagues was noted with great interest. This comprehensive and authoritative review goes far to debunk many of the myths surrounding this often overused modality.

Unfortunately, steadfast resistance is often encountered by those who attempt to limit the use of chest physiotherapy to only those patient groups in which it has been shown to be efficacious. When individual clinicians are approached to solicit their reasons for ordering chest percussion, they often express their belief that, even if chest percussion might not add to the therapy of their patients, at least it does not detract (except from the patients' pocketbooks). No one, including patients, should be required to pay for needless therapy. Of course, with the advent of DBG's, administrators are becoming much more vigilant in their effort to minimize or eliminate useless and costly activities. Furthermore, clinicians should not blithely assume that chest percussion might merely be neutral in its effects. As pointed out by Kirillof et al, chest percussion and/or vibration has been shown to produce bronchoconstriction in chronic bronchitis patients.
Although the bibliography for Kirilloff's review was extensive, one very important paper was not cited; the report by Reines et al documents that chest percussion might actually be contra-indicated in another category of patients. Reines studied fifty consecutive children between the ages of three months and nine years who had undergone cardiac surgery for congenital heart disease. The children were randomized into two groups. The first group (CPT) received routine post-operative therapy comprised of deep breathing/suctioning/coughing/postural drainage/vibration/percussion. The second group (NCPT) received deep breathing/suctioning/coughing without postural drainage/vibration/percussion. Response to therapy was evaluated by a radiologist who was unaware of the treatment group to which each patient was assigned. CPT was associated with significantly more frequent and more severe atelectasis than NCPT. The authors speculated that the reason why CPT might actually be harmful is that the procedure causes pain which subsequently triggers splitting and reductions in functional residual capacity. Another notable finding of this study was the observation that atelectasis was not significantly associated with temperature spikes. Thus, the knee-jerk institution of respiratory therapy (with or without chest physiotherapy) secondary to the observation of a spiking temperature was not indicated in these patients. Thus, although the study has not been performed in adults, clinicians should remain skeptical regarding the routine use of respiratory therapy in open heart patients and especially skeptical with regard to the use of chest percussion in this group in the presence or absence of fever.

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REFERENCE


To the Editor:

We appreciate the comments of Mr. Demers regarding our article. The article cited in his letter provides excellent support for the need to objectively evaluate the effects of chest physical therapy, rather than to assume benefits. Further, the letter provides strong support for judicious use of this treatment from a highly-respected member of the respiratory therapy profession.

Our article focused on the effects of chest physical therapy in adults because this is the population served in our Division. We are delighted to highlight additional evidence which supports the need to identify specific indications for use of this treatment. The argument that "even if chest physical therapy might not add to patient therapy, at least it does not detract" is encountered frequently but, as noted by Mr. Demers, is not necessarily correct.

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Loss of Fiberoptic Laser Ferrule

To the Editor:

I found the recent report of the "Loss of Fiberoptic Laser Tip" by Doctor Mehta (Chest 1985; 88:798) particularly timely since I recently experienced a similar complication. In view of the structure of the quartz fiber system used with the Nd-YAG laser, this may be an underreported complication of the procedure.

A 72-year-old white man was referred because of total atelectasis of the left lung. He had enjoyed good health until 1979, when he was found to have an adenocarcinoma of the rectum. This was resected and he did well until 1982, when a small left lung mass was found. A diagnosis of adenocarcinoma was made by percutaneous thoracic needle aspiration. He received intermittent chemotherapy with 5-FU and did well until 1984, when he developed a left-sided pneumothorax. This was treated with a McSwain dart, but the lung failed to completely reexpand. The McSwain dart was removed and further evaluation was not performed at that time. Over the ensuing months, he developed progressively worsening dyspnea and malaise. A follow-up chest x-ray examination revealed total atelectasis of the left lung and bronchoscopic examination revealed total obstruction of the left mainstem bronchus with tumor. He then underwent Nd-YAG laser photoresection using a fiberoptic bronchoscope and local anesthesia. The bronchial lumen was recanalized using energy levels of 40 watts with 0.5 sec duration. The fiber was frequently removed for cleaning and inspection during the procedure. A total of 12,150 jouls was delivered during the procedure. Near completion of the procedure, the fiber was removed for cleaning and it was noted that the metal ferrule was missing from the tip of the laser fiber. The bronchoscope was reintroduced and the bronchi carefully searched for the metal ferrule, but it could not be found. The bronchoscope was next examined and the ferrule was found lodged in the proximal aspiration port. It was then easily extracted from the bronchoscope and replaced on the tip of the laser fiber. There had been no fires at the tip of the laser fiber during the procedure and the patient suffered no ill effects.

I assume by the description of Doctor Mehta's case report that he is referring to the loss of the metal ferrule from the tip of the laser fiber when he refers to loss of the fiber tip, as happened in the preceding case report. Although the metal ferrule is small, its retention in a bronchus could cause problems over the long run, particularly if the procedure is being performed for a benign lesion. Potential complications could include granuloma formation around the ferrule, atelectasis and postobstructive pneumonia, as well as hemothysis. It is also frustrating to try to replace the metal ferrule in order to complete the procedure.

The function of the metal ferrule is to hold the fiber tip in the center of the gas stream which is blown continually through the catheter containing the laser fiber. This helps keep the fiber tip clean as well as clearing the working surface of smoke and debris. Potential problems with this system were first alluded to by Hetzel, who pointed out that tissue or mucous can adhere to the fiber tip and result in absorption of energy from the emerging light, raising the tip to very high temperatures. This results in the expansion of the metal ferrule, as well as breakdown in the glue holding the ferrule. This may then result in difficulty withdrawing the catheter system from the bronchoscope, or even dislodgement or loss of the metal ferrule from the tip of the laser fiber.

I certainly agree with Doctor Mehta that the best way to avoid this complication is to keep the laser fiber tip meticulously clean. This can best be accomplished by keeping the laser fiber tip at least 0.5 cm from the working surface, as well as frequently removing the laser fiber for inspection, cleaning and cooling of the tip. Finally, if resistance is met in trying to withdraw the catheter system from the bronchoscope, additional time should be given for the tip to cool before applying further traction on the laser catheter system. If this becomes a frequently experienced problem, then perhaps further modification in the catheter system will be necessary.

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