misdiagnosed as diffuse alveolar damage or fibrosing alveolitis. Dr. Carrington has re-reviewed the slides and agrees that this is an example of eosinophilic pneumonia with an unusual degree of bronchiolitis obliterans. The patient has also been included in another report\(^4\) as an example of acute fibrosing alveolitis.

It is recognized that patients with rheumatoid arthritis and eosinophilia are more likely to have systemic complications of rheumatoid disease, including lung complications.\(^5\) The levels of eosinophils in the blood are not recorded in the report by Geddes et al\(^6\) of rheumatoid bronchiolitis, but two patients had pulmonary infiltrates and in one this was transient. No coherent hypothesis can be presented, but it appears that there is an overlap between rheumatoid disease, eosinophilic pneumonia and bronchiolitis obliterans. It also appears that while the lung infiltrate of eosinophilic pneumonia is rapidly reversible, the airway lesions are not. The development of chronic airflow limitation and the disappearance of lung infiltrates is not a common feature of fibrosing alveolitis and this occurrence should suggest an unusual condition.

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To the Editor:

We are grateful to Dr. Thurlbeck for his comments.

The purpose of our paper was to report on the development of an obstructive pattern of ventilatory function following an “interstitial lung disease” of undetermined etiology characterized by a restrictive ventilatory pattern.

The histologic appearances reported on the patient showed alveolar changes compatible with those reported by other authors of papers on diffuse pulmonary fibrosis.\(^1\) In the light of undetermined etiology, it is hardly surprising that there is a diversity of pathologic descriptions in this group of diseases. We do not feel that the purpose and intent of our report is invalidated by the additional descriptive qualifications suggested by Dr. Thurlbeck.

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REFERENCE


IPPB, SMI, Gloves: Good or Bad?

To the Editor:

It is with disappointment that we read the report by Jung et al in the July, 1980 issue of Chest.

Our disappointment is twofold. First, the method of administering all three types of respiratory care was poorly designed. Second, despite this poor methodology, we feel the results of this study will be interpreted as further evidence for the lack of effectiveness of these respiratory care procedures.

The following questions occur to us about the methodology of the respiratory care maneuvers compared:

1) Why was the IPPB administered in a manner whereby peak pressure is arbitrarily set at 15 cm H\(_2\)O and volumes are not even monitored? The authors recognized the importance of some volume goal with the incentive spirometer; why not with the IPPB? The importance of this volume-oriented approach is recently detailed by several authors.\(^1\)\(^-\)\(^3\)

2) Why were the sustained maximal inspiration (SMI) maneuvers performed on a four times a day basis, supervised by a technician? It is our understanding that a major proposed advantage of SMI is that the patient can use it very frequently without the need for costly supervision.\(^4\)

3) The resistance breathing (blow-glove) technique is subject to the same questions we raise about SMI. In addition it has a more questionable physiologic basis being a maneuver emphasizing exhalation instead of inspiration.\(^4\)

We understand that the purpose of this study was to compare these maneuvers, as administered at the author’s institution; however, we feel strongly that these methods, as described, do not reflect current thoughts on their proper application. We do not feel this type of study sheds new light on the relative efficacy of postoperative respiratory care procedures. It does point out the tremendous differences in the application of these procedures.

The question over determining the most effective method of applying these procedures, as well as identifying specific criteria patients should meet in order to receive them must be answered prior to further comparisons of relative efficacy. It is refreshing that this point of view was expressed in recent discussions at the National Institutes of Health-sponsored conference on “In-Hospital respiratory care of the non-critical care patient” (Sugarloaf II) held in Atlanta, Ga.

We do feel research as conducted by Jung et al has shown “routine” administered therapy to probably be ineffective. The real questions to be answered are: What is the “correct” way to administer respiratory care procedures to achieve specific clinical goals? What are the criteria a patient should meet to receive therapy? How can we objectively evaluate therapy to see if it meets our clinical goals?

If we answer questions similar to those above, then we can answer the question: Does correctly applied respiratory
care to patients who meet specific criteria alter the morbidity and mortality in a cost-effective manner?

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To the Editor:

We appreciate the interest and reactions to our paper expressed by Mr. Welch and Mr. Mercurio. We would urge them, however, to review the purpose, which we outlined, for doing the study and for selection of the methods.

Although we heartily agree that the way we administered each of the modalities does not agree with the current concept of their application, we deliberately designed our protocol to duplicate the current community standard of practice, not our institution's practice. Sustained maximal inhalation (SMI), based upon our review of many hospitals in our community, is being given in a haphazard manner, without standardization, much as has IPPB in the past, despite Welch and Mercurio's feelings about how this should be done. We at least tried to assure that patients took SMI and IPPB in a consistent manner, which, despite Welch and Mercurio's feelings to the contrary, also is not reality in the community.

We fully agree with the questions raised concerning the correct methods of administration, criteria for patient selection, etc. We are forced to conclude, however, that since this is not yet being done, studies such as ours do emphasize that the mere substitution of one modality with another will not necessarily achieve the objectives desired, if none of the modalities is properly administered. Welch and Mercurio might be interested to know that we, indeed, have a standard protocol for administering SMI at our institution which is considerably different from that used in our paper. We also rarely use IPPB postoperatively, and resistance breathing devices, although still ordered, are more often found in patients' bedside tables than in their mouths.

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Toxic Shock Syndrome

To the Editor:

Toxic shock syndrome (TSS) is a poorly understood ill-

ness occurring primarily in previously healthy young women and carrying an 8 to 10 percent mortality. Apparently increasing in incidences, it has recently been linked to the use of certain brands of tampons in menstruating women and to the presence of penicillin-resistant Staphylococcus in the vagina. Clinically, it has been characterized by fever, rash, hypotension, and the hepatic, hematologic, renal and central nervous systems may also be involved. We wish to report the first case of the occurrence of pulmonary involvement, specifically, adult respiratory distress syndrome (ARDS), in toxic-shock syndrome.

CASE REPORT

A 20-year-old white woman presented with history of myalgias, fever, and diarrhea for several days. These symptoms began during her menses, and she had been using a popular tampon which has subsequently withdrawn from the market. Examination and laboratory evaluation were initially unremarkable except for vaginal cultures that grew Staphylococcus aureus, and when hypotension developed, she was transferred to our hospital for further evaluation. On admission, her respiratory rate, chest x-ray film, and gas exchange parameters were all normal. Tachypnea and cyanosis gradually developed, however, and her chest x-ray film showed progressive, diffuse infiltrations. A Swan-Ganz catheter was placed and revealed normal pressure. Copious pink-tinged frothy secretions (100 ml per hour) were suctioned from her endotracheal tube. Effective compliance was reduced, and the A-a gradient was 614 mm Hg. Treatment with PEEP, colloid and steroids resulted in improvement in her A-a gradient and chest x-ray film findings, but multi-system failure supervened, and the patient died on the fourth hospital day.

We feel this patient represents a case of shock-lung precipitated by the so-called toxic shock syndrome. Although not previously described as a complication of TSS, other entities in the differential diagnosis were carefully excluded. In addition, precedent exists for the development of ARDS in other forms of bacterial toxemia.8 9 Although the specific etiologic of TSS remains to be elucidated, it is possible that either circulating toxin or circulating immune complexes are responsible for the disruption of capillary permeability. Because this apparently new and often devastating illness usually occurs in previously healthy young women, it is important to add it to the differential diagnosis of ARDS.

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