especially in medical patients with respiratory failure. Because of these data limitations, we do not believe that undue reliance on formulas and guidelines alone will prevent nutritional complications. Our case report illustrated the problem of weaning difficulties due to nutritionally associated hypercapnia in a patient population not commonly thought to be susceptible to this problem. We feel that long term ventilator patients from any cause, as well as some severe COPD ventilator patients, are candidates for this complication even with the use of nutritional guidelines. How frequently this occurs is unknown. We hope the "carry home" message of our case report, by documenting for the first time what has previously been a clinical observation, would be to encourage physicians to consider whether hypercapnia-associated difficulties could be due to nutritional support even when basic guidelines have been utilized.

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REFERENCE


Role of Right Heart Catheterization

To the Editor:

Connors and colleagues' have added valuable morphologic documentation of complications of right heart catheter monitoring. As an autopsy study, the startling frequency of certain complications might be overstated, yet they cite literature that supports their results.

I have no direct criticism of this work. However, I must take strong exception to the statement that "a control group was not possible". Invasive instrumentation is, at best, a mixed blessing, but its relative importance in harming as well as helping patients has never been critically ascertained—an investigation requiring appropriately-designed controlled observations. It is shocking that what the authors describe as "aggressive support" has long been the rule in many centers without carefully investigating 1) its net result, ie, risk:benefit ratio, and 2) for whom it is contraindicated or unnecessary. Indeed, observations in a large series at the University of Massachusetts Medical School (presented at last year's annual scientific session of the American College of Chest Physicians) indicate very strongly that pulmonary artery catheterization does not result in net overall benefit (ie, for the series). Indeed, conceding that there must be patients whom it "obviously" helps, these may well be balanced or overbalanced by those it harms. Moreover, the harm isn't necessarily the appalling structural damage demonstrated by Connors and colleagues. It has been shown repeatedly that the mere presence of intravascular instrumentation alters patients' responses to physiologic challenges and neural stimuli. Connors and colleagues' and the University of Massachusetts' reports add important new data supporting prospective, controlled investigations of the use of invasive monitoring.

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REFERENCES


To the Editor:

We appreciate Dr. Spodick's interest in our report of the complications of right heart catheterization. The lack of a control group in our study was a result of the manner in which critically ill patients are cared for in our hospital. We simply could not identify sufficient numbers of hemodynamically-unstable patients who came to postmortem examination and had not received right heart catheterization at some time during their hospital course. For this reason, a control group was not possible. We share Dr. Spodick's concern regarding the importance of determining the balance between the benefits and risks of right heart catheterization. Recently, Robin has added his voice to the call for a controlled trial of right heart catheterization in critically ill patients. While right heart catheterization is clearly useful in evaluating and treating hemodynamically unstable patients, it has never been shown that the information provided by the procedure improves outcome. In fact, the adverse effects of this procedure may outweigh the presumed benefits. A multicenter, randomized, controlled trial could resolve this issue.

However, before initiating such a study we need to define more clearly the patient groups for whom right heart catheterization is most appropriate. A patient will only benefit from right heart catheterization if new information about the hemodynamic status is obtained which was not available using other, less invasive means. Undoubtedly, many patients receive right heart catheterization whose hemodynamic status is predictable on the basis of the available clinical information. The benefits of the procedure are negligible in these patients, yet the risks are unchanged. It is important that we define the patient population whose hemodynamic status can be reliably predicted from clinical information and distinguish these patients from those in whom the pulmonary artery catheter provides new information not otherwise available. Prospective study of well-defined patient populations is needed to make these distinctions. The information from such a study will help the clinician decide when right heart catheterization is indicated and allow more rational selection of patients for the proposed clinical trial of this procedure.

Once this issue is addressed, a carefully planned, multicenter, randomized, controlled clinical trial of right heart catheterization should yield information which will allow clinicians to make more informed decisions regarding the monitoring of their critically ill patients.

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REFERENCES

Amiodarone Pulmonary Toxicity

To the Editor:

We fully agree with Dr. Martin et al about the diagnostic value of bronchoalveolar lavage fluid (BALF) data in assessment of pulmonary damage induced by amiodarone (Chest 1985; 88:630-3). Moreover, this is the case for many drug-related pneumonitis. Nevertheless the nature of these data merits discussion.

In their case report of amiodarone pneumonitis, Drs. Martineau et al point to the presence of foamy alveolar macrophages under light microscopic examination and the presence of lamellar inclusions by electron microscopic examination: this is in accordance with the first observations published.4 In addition, they state that these "characteristic" inclusions "may reflect an adverse effect of the drug." However, these inclusions seem to be related to drug impregnation and not necessarily to pulmonary damages. In fact, such inclusions have been described in other phospholipidosis occurring spontaneously or induced by different drugs.4 Moreover, these inclusions have been observed in at least 14 patients on amiodarone treatment without pneumonitis.4,5

On the other hand, Dr. Martin et al mention an increase in total cell count in BALF of their patient; surprisingly, the differential cell count was normal. Indeed, in eight patients with amiodarone lung, no changes in differential cell count have been found,6 but in 11 such patients a high grade lymphocyte alveolitis, mainly due to OKT+ lymphocyte increase has been reported.4 Similarly, we have underlined that in four out of six such patients, lymphocyte alveolitis had been previously reported.7 Furthermore, in nine patients on amiodarone treatment but without pneumonitis, there was no differential cell count change in BALF,9 so these changes seem to be a good indicator of amiodarone-induced pulmonary damages.

In summary, in amiodarone lung the presence of lamellar inclusions appear to be relevant to drug impregnation and not to its pulmonary harmful effect; on the other hand, differential cell count changes, never observed in patients on amiodarone treatment with no lung damage, seem to be a more reliable marker for this amiodarone side effect.

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REFERENCES


To the Editor:

We appreciate the comments of Dr. Akoun et al regarding our report on bronchoalveolar lavage in amiodarone pulmonary toxicity. Several points warrant further discussion.

We agree that bronchoalveolar lavage will likely be useful in the assessment of amiodarone pulmonary toxicity. As noted in our report, however, the clinical utility of finding phospholipid inclusions in alveolar macrophages remains unclear. It seems likely that some patients receiving amiodarone will demonstrate inclusions in lung cells without exhibiting any evidence of pulmonary toxicity. It may be premature, however, to dismiss this finding as unrelated to the mechanisms of pulmonary toxicity.

For example, a recent in vitro study from our laboratory suggests that there is a narrow range of safety between amiodarone concentrations which induce these characteristic inclusions in lung cells and concentrations of the drug which result in lethal cell injury.7 An important finding from this model of amiodarone pulmonary toxicity is that inclusions can form within lung cells using concentrations of amiodarone equivalent to therapeutic serum levels (1 to 3 μg/ml). Amiodarone, however, can be concentrated by lung tissue several-fold above serum levels,9 suggesting the potential for amiodarone to be directly toxic to the lung parenchyma.

Alternatively, as Doctor Akoun et al have noted, some patients with amiodarone toxicity exhibit an increase in OKT+ lymphocytes as determined by bronchoalveolar lavage. This is consistent with findings in our patient population, but it would appear the percentage of patients with a lymphocytic reaction may be less than previously described.6 Approximately one third of our patients (five of 14) demonstrated an increased percentage of OKT+ lymphocytes consistent with a hypersensitivity pneumonitis. The absence of an OKT+ lymphocytosis by bronchoalveolar lavage, however, does not exclude the diagnosis of an adverse reaction. A prospective study of patients receiving amiodarone would provide insight into mechanisms of pulmonary toxicity and the relevance of bronchoalveolar lavage findings to the pathogenesis of the disorder.

As yet, the pathogenesis of amiodarone lung toxicity remains unclear. There is evidence to suggest a role for the direct toxicity of the drug, as well as a role for hypersensitivity reactions in certain patients. It is likely that amiodarone toxicity in different patients may result from fundamentally different mechanisms, yet the clinical presentation may be quite similar. As further studies are initiated we hope that such mechanisms will be clarified.

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REFERENCES