care, preventative and occupational medicine, drug screening, and epidemiologic and public health issues (eg, norovirus, severe acute respiratory syndrome). Issues specific to the many crew nationalities and cultures that we treat are always a challenge. Critical decisionmaking regarding the most appropriate and logistically feasible means of evacuation is often required.

The physician functions in a very tight social system, requiring an understanding of the hierarchy of the officers and the function of numerous departments. The cruise lines are not in the medical business, they are in the travel business. If you think patient satisfaction is an issue in your emergency department (ED), you have never worked on a cruise ship. The Americans With Disabilities Act has created quite a challenge, attempting to meet the needs of respirator-dependent and other passengers with disabilities while not compromising safety.

There is no residency in cruise medicine. Our physicians are independent contractors. We do not control how they practice or their medical decisionmaking. However, I am fortunate, as chairman of an ED that has a residency program, to disseminate cutting-edge information to our physicians. Additionally, in 2002, we started the annual Institute of Cruise Ship Medicine in Miami, Florida, which is currently open to all of our ships' physicians.

Continuity encourages many cruise ship lines to prefer that physicians work for extended periods. The crew gets to know the physician, and the physician can build confidence. Unfortunately, the US system has rarely provided quality physicians who are willing to work for 6 to 8 months or longer. I predict the continued deterioration of reimbursement and the medical care system in our country will lead to an increasing number of highly qualified emergency physicians who are willing to work more than the occasional 2-week tenure. I would encourage physicians who are looking for a tremendous challenge, one requiring a significant broadening of their emergency skill set, to consider time as a ship's physician to be a worthwhile experience, not only socially and culturally, but also intellectually.

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Tissue Oxygen Saturation Monitoring in Diagnosing Necrotizing Fasciitis of the Lower Limb: A Valuable Tool but Only for a Select Few

To the Editor:

I read with interest the article by Wang and Hung[1] published in the September 2004 issue of *Annals*. Necrotizing fasciitis is perhaps the deadliest soft tissue infection known to humankind, and early diagnosis and aggressive debridement have been proven to improve survival.[2-4] I think this article would contribute to the development of a focused and targeted approach in the management of necrotizing fasciitis by helping in the detection of early cases of necrotizing fasciitis. While I applaud this effort, I would like to raise some points that I hope will help in the potential application of this work.

The first matter is regarding terminology. The authors used the term fasciotomy as a treatment of necrotizing fasciitis. Fasciotomy is a treatment for compartment syndrome where there is acute or chronic elevation of intracompartment pressure. It is doubtful that increased intracompartment pressure has any role in necrotizing fasciitis. The authors themselves mentioned this in passing in their discussion. The pathological process in necrotizing fasciitis is liquefactive necrosis with thrombosis of the perforating vessels supplying the skin. Although the authors mentioned that several factors may be responsible for their clinical observation, I believe that this is the primary reason for their observation of decreased tissue saturation. The correct term should be excisional debridement of the necrotic fascia. Aggressive removal of all infected tissue, especially the superficial fascia, not fasciotomy, is the only way to halt and control the infection.

Another issue that critically compromised the utility of this article clinically is the patient selection for the study. All patients with chronic venous stasis, peripheral vascular disease, shock, and systemic hypoxia were excluded from the study. This is understandable because most patients with these conditions would have impaired tissue perfusion and oxygen saturation and, thus, would give a false-positive result. However, most patients who develop necrotizing fasciitis have underlying predisposing conditions that make them susceptible to the development of this condition. In my review of 89 consecutive patients, predisposing conditions such as diabetes, peripheral vascular disease, or chronic liver disease were present in 87% of patients. In addition, patients presenting with necrotizing fasciitis with multi-organ failure and shock (eg, streptococcal toxic shock syndrome) would not have interpretable results. Therefore, a majority of patients susceptible to necrotizing fasciitis would have been excluded from this study. This is a pity, because this is a group of patients in whom early diagnosis would profoundly affect outcome. Still, in the select group of patients (namely healthy patients), tissue oxygen saturation monitoring may potentially be a valuable diagnostic adjunct.

Our group has been interested in the early recognition of necrotizing fasciitis. We compared laboratory tests for patients with necrotizing fasciitis and severe soft tissue infections and analyzed routinely performed tests for the assessment of severe soft tissue infections (ie, CBC count, electrolytes, erythrocyte sedimentation rate, C-reactive protein). A numeric score based on the relative significance of the laboratory parameters called the laboratory risk indicator for necrotizing fasciitis (LRINEC)

score was devised.[5] We think this is capable of detecting even nascent cases of necrotizing fasciitis and can potentially be a valuable diagnostic adjunct in the assessment of potential necrotizing fasciitis.

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In reply:

I appreciate Dr. Wong's comments. As to the terminology, I agree that "excision debridement of the necrotizing fascia," which describes more informatively the surgical procedure, would be a preferred term instead of the term "fasciotomy," which we used in our article.[1] The other issue raised by Dr. Wong is that the patient selection might critically compromise the clinical implications of our study. We excluded patients with chronic venous stasis, peripheral vascular disease, shock, and systemic hypoxia and might raise the question of whether tissue oxygen saturation also works well to differentiate necrotizing fasciitis from cellulitis or other soft tissue infection among patients with these underlying diseases. I still have no definite data to answer this question. However, the above exclusion criteria have been used to find a homogenous study population. It does not mean that tissue oxygen saturation did not play any role in early diagnosis of necrotizing fasciitis for these patients. Theoretically, tissue oxygen saturation should become lowest over the limbs involved in necrotizing fasciitis even when the patient has concomitant presence of the conditions for exclusion. However, I believe the cut-off value of tissue oxygen saturation would be lower than 70%, which we concluded in the present study. The possible problem that should be considered is that peripheral vascular disease or other conditions can result in a low tissue oxygen saturation that falls below the cut-off value we used to detect fasciitis.

Tissue oxygen saturation provides repeated noninvasive measurements; therefore, dynamic changes, such as continuous decline at the target limb areas, can still provide clinical clues for early diagnosis of fasciitis even in those with peripheral vascular diseases.

I have also reviewed the article concerning the laboratory risk indicator for necrotizing fasciitis (LRINEC) score developed by Wong et al.[2] I think the LRINEC score is an important finding in diagnosing necrotizing fasciitis. The LRINEC is reported to have high positive and negative predictive values. Accordingly, the sensitivity and specificity should also be high. However, when I try to apply the scoring to the subjects in our study, the positive predictive value is 40% and the negative predictive value is 95%.

One explanation is that our cohort population are victims of necrotizing fasciitis at a very early stage. To my knowledge, the score was developed according to the clinical data from a retrospective population and then proved in the consecutive validation cohort. The limitation will be that a retrospective population may provide a higher specificity and a lower sensitivity of the scoring. The laboratory data used in developing the scoring should therein be critically limited in those measured in an early phase of fasciitis. It may be difficult to define the phases with a retrospective review of the medical record. It is also difficult to apply the randomization process to a retrospective population. A scoring system is best validated in consecutive patients with no predetermination or diagnosis. The sensitivity, specificity, and positive and negative predictive values would be drawn by analyzing the differences between presumed and final diagnoses. Otherwise, the clinical implication of the model will be critically limited to full-blown diseases instead of early diagnosis. I believe that the LRINEC score will be a good indicator for full-blown necrotizing fasciitis, but its role in early detection should be determined in advance. Some other reports demonstrated the pitfalls in diagnosing necrotizing fasciitis by conventional blood tests and even suggested that the diagnosis depends on frozen section biopsy.[3-6] We have the same difficulties and thus have tried to use tissue oxygen saturation to resolve the dilemma. The combination of tissue oxygenation saturation data and the predictive scoring system such as the LRINEC score may be a consideration to the issues mentioned above.

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