cytosis in 33% and 18%, respectively, of their pulmonary tuberculosis patients. We observed thrombocytosis in only 11% of pulmonary tuberculosis patients. Moreover, we found thrombocytosis in only 1 of 10 patients with active pulmonary tuberculosis in whom we were able to examine serum VEGF levels at the end of tuberculosis treatment. Thrombocytosis is a consequence of the general inflammatory state of the body against the infection. Also, VEGF is known to be involved in inflammation and wound healing. Therefore, in our view it is hard to explain the increase of serum VEGF levels in patients with active pulmonary tuberculosis by using thrombocytosis alone.

The second important issue raised by Dr. Ferrero is the standardization of the sampling process. In our study setting, all samples were drawn and processed by the same biochemist. The separation of sera from blood cells was performed in 30 to 45 min. Few studies in the literature concerning VEGF levels in tuberculosis patients have determined serum VEGF levels. Moreover, intense angiogenesis was shown ultrastructurally in active pulmonary tuberculosis lesions.11 The expression of VEGF in alveolar macrophages around active tuberculosis lesions was shown by immunohistochemistry.7

Finally, the percentage of thrombocytosis present in our active pulmonary tuberculosis patients is not high, and the sample preparation procedures are well-designed and standardized. Although the type of sample that should be used in VEGF measurements is still a matter of debate, all studies regarding VEGF levels in pulmonary tuberculosis patients were performed using serum samples. However, further research is needed to compare the relationship of VEGF levels in serum and plasma, and different anticoagulant agents, since the existing studies comparing serum and plasma VEGF levels were performed with cancer patients.

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Troponin in Septic and Critically Ill Patients

To the Editor:

With great interest, we read the recent review by Roongsritong et al1 (May 2004) on the causes of elevations in troponin level in patients who had not experienced myocardial infarction, particularly the section about troponin levels in septic and critically ill patients. As the authors state, the exact mechanism of increased cardiac troponin levels in patients with sepsis remains unknown.

The authors suggest several mechanisms for troponin release in septic patients. One of their suggestions is that cytokines or endotoxin might cause myocardial injury. Circulating mediators such as tumor necrosis factor (TNF)-α and interleukin-1β have indeed been shown to cause myocardial depression in patients with sepsis.2 Cytokines might increase the permeability of the myocyte membrane with the leakage of free cardiac troponin I (cTnI) from the cytoplasm while the myocyte-contraction complex remains intact. We could not confirm this using a human endotoxin model.3 Our volunteers showed a septic profile after being injected with endotoxin. Troponin levels were not measurable, although TNF-α levels were high enough to cause myocardial depression. In these volunteers, we also measured increased levels of death hormones, TNF, and tumor necrosis factor-related apoptosis-inducing ligand treatment, suggesting active apoptosis.4 Therefore, we think that it is unlikely that cTnI levels are elevated due to the leakage of free cytoplasmic cTnI from the cytoplasm while the myocyte-contraction complex remains intact. We concluded that the mechanism and meaning of the elevation of troponin levels in patients with sepsis still has to be elucidated, but that the confusion exists only with marginally elevated troponin levels. Higher levels of troponin (ie, >10 ng/mL),
especially when used in the context of the clinical suspicion of myocardial ischemia, and negative troponin results are still valuable in excluding myocardial damage.

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To the Editor:

We appreciate the interest in and comments on our recent article in CHEST (May 2004) by van Bockel et al. Unfortunately, the reports that they cited had not yet been published at the time of our review and thus were not included. However, Ammann et al recently reported higher levels of tumor necrosis factor-α, its soluble receptor, and interleukin-6 in troponin-positive critically ill patients compared to those who were negative for troponin. Therefore, we believe that controversy still exists, at the present time, regarding the role of cytokines in troponin release in this subset of patients.

We also maintain our opinion on the prognostic value of elevated troponin levels in sepsis and/or critically ill patients. In addition to the several reports cited in our review, two recent studies by Ammann et al and Wu et al found troponin to be an important risk factor for mortality in these patients. As stated by Dr. van Bockel and colleagues, a study by Kollef et al found clinically recognized cardiac dysfunction but not elevation of troponin levels to be an independent predictor of mortality in their patients. However, Kollef et al defined clinically recognized cardiac dysfunction as the presence of cardiac arrest, congestive heart failure, unstable angina, or myocardial infarction. We believe that their study design may have underestimated the prognostic value of troponin levels.

Finally, we agree that further studies are needed, and we laud van Bockel and colleagues for their effort at elucidating the mechanism and importance of the elevation of troponin levels in septic and critically ill patients.

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ICU Transport

Interhospital Transport of Critically Ill Patient With Dedicated Intensive Care Ventilator

To the Editor:

The use of inhaled prostacyclin to enable the safe interhospital helicopter transport of a patient with ARDS complicated by life-threatening hypoxemia was described by Reily et al (April 2004). Their efforts to optimize the patient for transport to provide optimal care should be acknowledged. However, their choice for an unconventional ARDS therapy like inhaled prostacycline, not initially administered during treatment in their department, could be debated. Instead of a pharmacologic...