Catamenial pneumothorax: Some commentaries

To the Editor:

We read with interest the article by Korom and associates¹ reporting 3 new cases of catamenial pneumothorax and with an excellent review of the literature. The reports outline the frequent lack of recognition of diaphragmatic endometriosis as the causative factor of catamenial pneumothorax, even in patients submitted to surgical exploration with video-assisted thoracoscopy. In a previous prospective study on catamenial pneumothorax,² we pointed out that exploration of the whole thoracic cavity should be mandatory in these patients. We suggested that patients should be positioned in the posterolateral decubitus position to correctly explore the whole hemidiaphragm, which is often incompletely visualized if the patient is positioned as for a lateral thoracotomy.

We would like to comment on 2 points concerning the article by Korom and associates.¹ First, they state that not all of a patient's menses coincide with a pneumothorax, but each episode of a catamenial pneumothorax is associated with the menstrual flow. Definition of catamenial pneumothorax implies a strict temporal relationship with menses, and the condition has been often defined as a recurrent pneumothorax occurring within 72 hours from the onset of menstruation.² However, the recurrent character in relation with the immediate premenstrual period has also been reported.^{3,4} It has been recently shown that a recurrent endometriosis-related pneumothorax might be observed also in the intermenstrual period.⁵ Therefore it has been suggested that thoracic endometriosis should be suspected in any case of recurrent pneumothorax in women of reproductive age, even when episodes occur out of the menstrual period.⁵

Second, Korom and associates¹ proposed plication of the portion of diaphragm involved by endometriosis rather than excision and repair. Furthermore, in their opinion hormonal therapy should be considered as a secondary therapeutic option to be used in case of failure of surgical intervention: they adopted this strategy in one of the 3 patients. We do not agree with these suggestions. In our opinion resection of all the visible lesions (when technically feasible) should be carried out not only to optimally treat the cause of pneumothorax but also to avoid further intrathoracic spread of the disease.² In the absence of a hormonal treatment, endometrial

implants (which are left in place by simple plication) would continue to undergo cyclical changes, with possible consequences in terms of partial diaphragmatic disruption and formation of new holes. We think that patients should be offered the best chance of cure when the disease is first diagnosed. For this reason, we proposed surgical excision of the diseased structures, pleurodesis, and a short course of hormonal treatment.² Pleural symphysis takes some weeks to be effective after surgical intervention, and if menstruation occurs before pleurodesis is achieved, a recurrence of pneumothorax will be likely.

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Reply to the Editor:

My colleagues and I thank Dr Alifano for his interest in our work and would like to respond to the comments brought forward concerning our recently published review on catamenial pneumothorax (CPT).¹ First, they debate the classic description of CPT. As a time-honored working definition, this phenomenon has been described on the basis of its temporal relationship with the menstrual cycle. In the majority of published cases, CPT has been reported to reach clinical significance (culminating in the diagnosis of spontaneous pneumothorax) within 72 hours of the beginning of the

menstrual flow. Yet as Dr Alifano has pointed out, there are some reports on CPT being diagnosed beyond this strict time-frame.² Because we believed the same, we have refrained from quoting this arbitrary 72-hour definition but rather stated that each episode of CPT is associated with the menstrual flow. Being aware of the pathophysiologic background, it is obvious that the individual relevance of a developing pneumothorax might be subject to a wider chronologic margin.

Second, Dr Alifano criticizes our therapeutic approach in CPT. He suggests "excision of all visible lesions (when technically feasible)." Similarly, we stated the following: "All accessible lesions should be excised, and plication is recommended to seal and strengthen this area." In contrast to our approach, Dr Alifano adds a 6-month course of hormonal therapy to his surgical therapy in all cases. Describing this therapeutic regimen in a recent publication, Alifano and associates³ cite only one (early) recurrence out of 8 treated patients; however, in this study the mean follow-up time is 6.6 months, with a range from 2 to 15 months. In contrast, on the basis of our experience and the world literature, abrogating the ovarian steroid genesis in mature women with CPT constitutes a significant alteration of the hormonal equilibrium in these patients. Without data on the long-term follow-up with these courses, we believe that abrogating the ovarian steroid genesis in a young woman is not a justifiable first-line approach for this clinical picture because excision and plication of the diaphragm might prevent recurrence of pneumothorax, as reported in several cases.

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Hematocrit trial

To the Editor:

We read with interest the report by Ootaki and associates,¹ as well as the correspondence by Shuhaiber² in reference to the article by Habib and colleagues.³ Ootaki and coworkers¹ applied a transfusion protocol in which blood was not transfused during cardiopulmonary bypass unless hematocrit was less than 15%. They found that patients with a hematocrit of less than 20% had a higher lactate level than patients with a higher hematocrit but imply that this has no functional significance.

In their critique of the article by Habib and associates,³ which emphasized the value of the lowest hematocrit as a predictor of outcome, Shuhaiber² implied that a hematocrit of 20% is a useful transfusion trigger. They also called for a prospective randomized study of hematocrit, as did Habib's group.

At the 2002 meeting of the American Association for Thoracic Surgery, we presented the results of a prospective randomized trial of 2 hemodilution strategies.⁴ This study was shut down by the Data and Safety Monitoring Board of the National Institutes of Health because of a strongly positive outcome. Infants who had a mean hematocrit of $27.8\% \pm 3.2\%$ ($n = 73$) had significantly better motor skills at 1 year of age relative to patients whose lowest hematocrit on bypass was $21.5\% \pm 2.9\%$ ($n = 74$). A significantly greater percentage of patients at 1 year of age were classified as developmentally delayed with respect to motor skills relative to patients perfused at a higher hematocrit. The lactate level 1 hour after bypass was significantly lower with the higher hematocrit.

The findings of our prospective randomized study are consistent with several previous reports derived from our laboratory work in this area.⁵⁻⁷ Studies using near-infrared spectroscopy suggest that acute hemodilution during cardiopulmonary bypass results in cerebral hypoxia. It is important to remember, before discarding the significance of our clinical trial as being irrelevant to adults because it was performed in infants, that the mature brain is significantly more sensitive to

hypoxic injury than the neonatal and infant brain. Nevertheless, we strongly endorse the call for a prospective randomized trial of hematocrit in adults undergoing cardiopulmonary bypass, including sensitive end points such as assessment for cognitive dysfunction. In the meantime, we strongly recommend that a hematocrit of at least 25% and preferably closer to 30% should be used during cardiopulmonary bypass.

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doi:10.1016/j.jtcvs.2004.09.039

Randomized prospective trial for blood transfusion during adult cardiopulmonary bypass surgery

To the Editor:

Hemoglobin dilution is an expected physiologic response during cardiopulmonary bypass (CPB) surgery. Current controversy, however, centers around this ques-

tion: what is a safe hematocrit level during CPB before the patient sustains less than an expected outcome? The main reason for the lack of consensus regarding blood transfusion may stem from the lack of a direct cause (hematocrit level) and effect (morbidity and mortality) relationship or an association or both causality and association. Although it has been found that low preoperative hemoglobin levels are correlated with poorer outcome,¹ it does not mean that correcting this number will result in improved outcome. This same argument holds true for intraoperative hematocrits with the understanding that new-onset intraoperative anemia is reversible and mainly caused by dilution, whereas preoperative anemia is pathologic and mainly caused by nondilutional processes. Also, despite understanding the reversibility concept, most decisions of intraoperative transfusion stem from personal and institutional experience, with no defined dimensions. In May 2004, the National Heart, Lung, and Blood Institute working group published an executive summary regarding future directions in cardiac surgery.² Creating a cardiovascular surgery clinical research network was one of the pillars, and I hope that the working group and the National Institutes of Health-sponsored workshop for neurocognitive changes after cardiac surgery will consider this trial an important direction toward filling an existing critical gap.

Why Is a Hematocrit Trial Timely?

The patient's physiologic status must be the underlying cause for a transfusion, and the outcome of the transfusion (effect) must also be considered. The practicing surgeon, including those in training, is currently confused with the paradigm of cause and effect that seems to argue that mortality is higher among patients with low hematocrit ($<25\%$ in women and $<23\%$ in men^{3,4}) and high hematocrit ($>34\%$).⁵ Randomization is lacking in the adult cardiac surgery group thus far, despite a clear benefit of increased hematocrit for neurologic outcome in pediatric heart surgery (mean intraoperative hematocrit: 27.8% vs 21.5%).⁶

Moreover, despite the benefit of increased oxygen-carrying capacity with increased hematocrit, we must be aware that blood transfusions expose patients to a variety of potential cellular and hu-