Communications to the Editor

Communications for this section will be published as space and priorities permit. The comments should not exceed 350 words in length, with a maximum of five references; one figure or table can be printed. Exceptions may occur under particular circumstances. Contributions may include comments on articles published in this periodical, or they may be reports of unique educational character. Please include a cover letter with a complete list of authors (including full first and last names and highest degree), corresponding author's address, phone number, fax number, and email address (if applicable). An electronic version of the communication should be included on a 3.5-inch diskette. Specific permission to publish should be cited in the cover letter or appended as a postscript. CHEST reserves the right to edit letters for length and clarity.

A Voice From a Modern Hospital

To the Editor:

It rang twice, but my nurse is busy
Waiting for the next dose is not easy,
My smothering is the same, but my pain is worse,
Lord, how about sending me a nurse?
Now I’m gasping for air, and hurting is bad,
Everybody thinks, I have gone mad.
My “Doc” is busy in an HMO meeting
Lord, would you be my doctor today?
IV pumps and heart monitors can’t speak
But one noisy word: Beep, Beep, Beep,
In this jungle, my whisper is hard to hear,
Lord, without a pager, would you hear me please?

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Low-Molecular-Weight Heparin for Unstable Angina

To the Editor:

In an excellent review article on the clinical use of low-molecular-weight heparin (LMWH), Aguilar and Goldhaber (May 1999) discussed its use in patients with unstable angina and cited the Efficacy and Safety of Subcutaneous Enoxaparin in Non-Q-Wave Coronary Events (ESSENCE) study. For the sake of completeness, I would like to mention three additional studies that have been published on LMWH in patients with unstable angina. They are the study by Gurfinkel et al., the Fragmin during Instability in Coronary Artery Disease (FRISC) study, and the Fragmin in Unstable Coronary Artery Disease (FRIC) study.

These three studies all concluded that treatment with LMWH of patients with unstable angina is at least as effective as, and potentially more effective than with, unfractionated heparin. Therefore, in deciding the heparin of choice, it is no longer merely a matter of “weight watching.”

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REFERENCES

6 Cheng TO. Weight watching in heparin [letter]. Arch Intern Med 1999; 159:1142

Classic, Abbreviated, and Modified Light’s Criteria

The End of the Story?

To the Editor:

Heffner and colleagues (April 1997) performed a meta-analysis involving 1,448 patients in order to determine the appropriate decision thresholds and diagnostic accuracies for pleural fluid (PF) tests that discriminate between exudative and transudative pleural effusions. In this study, cutoff points derived from the receiver operating characteristics (ROC) analysis differed from previously reported values in PF for lactate dehydrogenase (LDH-PF) (> 45% of the laboratory's upper limit of normal instead of more than two thirds of the upper limits of normal), protein (P-PF) (> 2.9 g/dL instead of > 3 g/dL), and cholesterol (CHOL-PF) (> 45 mg/dL instead of higher values). Their meta-analysis concluded that Light’s criteria provide good discriminative properties, although the PF cutoff point should be set at > 45% of the laboratory’s upper limit of normal for serum LDH
Table 1—Diagnostic Accuracy for Test Combinations that Identify Exudative Pleural Effusions*

<table>
<thead>
<tr>
<th>Test Combinations</th>
<th>N</th>
<th>Sensitivity, % (95% CI)</th>
<th>Specificity, % (95% CI)</th>
<th>+PV, % (95% CI)</th>
<th>−PV, % (95% CI)</th>
<th>LR+</th>
<th>LR−</th>
<th>OR, % (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Light’s criteria</td>
<td>358</td>
<td>98.3 (96.6–100)</td>
<td>76.1 (65.1–87.1)</td>
<td>94.7 (92.0–97.4)</td>
<td>91.1 (82.7–99.4)</td>
<td>4.1 (6.0–5.9)</td>
<td>0.02</td>
<td>182.3</td>
</tr>
<tr>
<td>Abbreviated Light’s criteria</td>
<td>348</td>
<td>96.1 (93.7–98.5)</td>
<td>78.8 (68.2–90.4)</td>
<td>95.1 (92.4–97.8)</td>
<td>82.5 (72.3–92.7)</td>
<td>4.5 (5.0–9.5)</td>
<td>0.05</td>
<td>91.5</td>
</tr>
<tr>
<td>P-PF or LDH-PF or CHOL-PF</td>
<td>324</td>
<td>98.9 (97.5–100)</td>
<td>55.0 (38.3–71.7)</td>
<td>94.0 (91.1–96.9)</td>
<td>88.0 (73.3–100)</td>
<td>2.2 (2.0–2.3)</td>
<td>0.02</td>
<td>114.5</td>
</tr>
<tr>
<td>LDH-PF or CHOL-PF</td>
<td>301</td>
<td>98.1 (96.3–99.9)</td>
<td>69.4 (53.0–78.5)</td>
<td>95.9 (93.4–98.4)</td>
<td>83.3 (68.3–98.3)</td>
<td>3.2 (3.0–3.4)</td>
<td>0.03</td>
<td>118.2</td>
</tr>
<tr>
<td>Modified Light’s criteria</td>
<td>360</td>
<td>99.3 (98.2–99.8)</td>
<td>66.7 (54.9–78.5)</td>
<td>92.6 (89.5–95.7)</td>
<td>95.8 (89.1–100)</td>
<td>3.0 (3.0–3.1)</td>
<td>0.01</td>
<td>289.0</td>
</tr>
<tr>
<td>Modified P-PF or LDH-PF or CHOL-PF</td>
<td>332</td>
<td>99.7 (98.9–100)</td>
<td>43.2 (27.4–59)</td>
<td>92.0 (88.8–95.2)</td>
<td>95.0 (82.9–100)</td>
<td>1.8 (1.6–2.0)</td>
<td>0.01</td>
<td>218.1</td>
</tr>
<tr>
<td>Modified LDH-PF or CHOL-PF</td>
<td>323</td>
<td>99.7 (98.9–100)</td>
<td>45.5 (29.6–61.4)</td>
<td>92.3 (89.2–93.4)</td>
<td>95.2 (89.7–100)</td>
<td>1.8 (1.6–2.0)</td>
<td>0.01</td>
<td>239.2</td>
</tr>
</tbody>
</table>

*Modified* refers to the adoption of the cutoff values suggested by Heffner et al.

*CI* = confidence interval; *PV* = predictive value; *LR+* = likelihood ratio positive; *LR−* = likelihood ratio negative; *OR* = odds ratio.

("modified" Light’s criteria). Likewise, LDH-PF values could be removed from Light’s criteria ("abbreviated" Light’s criteria) without affecting diagnostic accuracy. Finally, if physicians choose to avoid obtaining a serum sample, paired (LDH-PF or CHOL-PF) or triplet (LDH-PF or CHOL-PF or P-PF) test combinations making use of the new cutoff values are suitable alternatives to Light’s criteria.

Although we have previously observed that changing the classic Light’s criteria with different cutoff points offers no advantages for discriminating between transudative and exudative pleural effusions, we decided to determine whether a similar conclusion could be obtained following the recommendations of Heffner and colleagues. For this purpose, we retrospectively collected data from 435 consecutive adult patients undergoing thoracentesis at the University Hospital Arnau de Vilanova during the last 4 years. Ninety-one patients were excluded from the study due to nondefinitive diagnoses or to the existence of more than one potential etiology for the PF accumulation. The remaining 364 pleural effusions consisted of 72 transudates (19.8%) and 292 exudates (80.2%). There were 210 men (57.7%) and 154 women (42.3%) in the study, with a mean age of 57.5 years (range, 15 to 97 years). The causes of the transudates were congestive heart failure (48 patients), liver cirrhosis (14 patients), and nephrotic syndrome (10 patients), whereas exudates were secondary to neoplastic conditions (104 patients), pneumonia (94 patients), tuberculosis (54 patients), and other etiologies (40 patients). The diagnoses of the patients were defined by predetermined criteria. The results of applying the meta-analysis ROC cutoff points and the previously reported cutoff points are shown in Table 1. We chose 50 mg/dL as the cutoff for CHOL-PF based on our previously reported data. The number of test results available for each of the PF parameters was as follows: P-PF, 357; LDH-PF, 350; CHOL-PF, 171; PF/serum P ratios, 347; and PF/serum LDH ratios, 342.

Several conclusions can be drawn from this investigation. First, the abbreviated form of Light’s criteria has an accuracy similar to the classic Light’s criteria, but the former offers no advantages over the latter since LDH-PF is required to calculate the LDH ratio anyway. Second, the application of the alternative cutoff values suggested by Heffner and colleagues produced a relatively large decrease in specificity for an insignificant gain in sensitivity in our study. Thus, Heffner’s findings probably cannot be generalized to a particular practice setting, and so we think that each laboratory must determine its own best cutoff points.

Finally, we reinforce the concept that classic Light’s criteria remain the benchmark test combination for differentiating exudates and transudates. To paraphrase an editorial in CHEST 3 years ago, it is time to move on in the clinical investigation of the pleural space and to stop focusing our efforts on finding ever more perfect criteria for separating transudates and exudates.

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**REFERENCES**


To the Editor:

We appreciate the interest shown by Drs. Porcel and Vives in our meta-analysis of tests that discriminate between transudates and exudates (April 1997). We also found interesting their retrospective data, presented in their letter and their previous report, that compare different testing strategies in an effort to...
improve the accuracy of pleural fluid categorization. Several comments in their letter merit further discussion.

We performed our meta-analysis to address the analytic errors commonly committed by studies that attempted to “best” Light’s criteria with new pleural fluid tests. These studies almost always incorrectly analyze their data with hypothesis-testing statistics (comparision tests for proportions) rather than with analytic techniques recommended for diagnostic test research (eg, receiver operating characteristics [ROCs] analysis). Additionally, these studies report underpowered, small data sets of < 500 patients and frequently recommend new tests on the basis of their higher specificity compared with Light’s criteria. This latter recommendation fails to recognize that multiple tests combined in “or” rules (eg, Light’s criteria) always have a higher sensitivity but lower specificity compared with noncombination single tests when each of the test components of the combination and the “new” single test have similar discriminative properties.

The use of hypothesis-testing statistics runs the risk of incorrectly concluding that one test or test combination is “better” than another when no differences exist, or vice versa. Porcel and Vives run afoul of this error in their original report. They noted no differences among various test combinations in this underpowered study of a small sample of 195 patients analyzed with hypothesis-testing statistics (Z distribution test for comparing proportions). Interestingly, they contend in their letter, without employing any statistical analyses, that the “lactate dehydrogenase (LDH)-[pleural fluid] PF or [cholesterol] CHOL-PF” rule has a higher specificity than the “modified LDH-PF or CHOL-PF” rule. Examination of the overlapping 95% confidence intervals for the specificity results of these two combinations (Table 1 in their article), however, indicates that no statistically significant differences exist. Had they provided appropriate statistical analyses, no differences would have been noted among most, if not all, of the studied test combinations.

We should emphasize that the lack of differences among various test combinations in this underpowered study of a small sample of 195 patients analyzed with hypothesis-testing statistics (Z distribution test for comparing proportions) is exactly the point of our meta-analysis. The ROC analysis that we performed demonstrated no differences among any of the test combinations examined, excluding those that used pleural fluid bilirubin. We concluded that Light’s criteria have excellent discriminative properties but that other available tests and test combinations also have those properties. Investigators, therefore, might slow their rush toward trying to find even better tests for this indication. We did not suggest that clinicians should use any of the new test combinations based on any differences in their diagnostic accuracies; indeed, we emphasized that no differences existed. We did state, however, that alternative testing strategies with similar diagnostic accuracies might provide benefits in some clinical settings unrelated to their diagnostic performance. For instance, the combination rule of “P-PF or LDH-PF or CHOL-PF” avoids the need for blood test results because the calculation of the ratios of pleural fluid to serum is unnecessary.

We did propose in our meta-analysis that clinicians could use an abbreviated form of Light’s criteria (ie, eliminate either the LDH-PF or the LDH-PF/LDH serum ratio). This recommendation was based on the principles of diagnostic testing, which argue against the use of two tests in “or” rule combinations if the tests are highly correlated. Because the “LDH-PF” and “LDH-PF/LDH-serum” in Light’s criteria are “mathematically linked” (and highly correlated in our meta-analysis) since each includes “LDH-PF,” clinicians can abbreviate Light’s criteria by excluding either “LDH-PF” or “LDH-PF/LDH-serum” without sacrificing diagnostic accuracy. Indeed, Porcel and Vives prove this point in Table 1 in their article by showing nearly identical point estimates and overlapping 95% confidence intervals for the diagnostic performances of the standard and abbreviated forms of Light’s criteria.

The use of the abbreviated form of Light’s criteria is more than a purist’s attempt to avoid cluttering diagnostic rules with unnecesssary elements; clinicians can obtain good diagnostic accuracy using two components of Light’s criteria (ie, “LDH-PF or [protein] P-PF/P-serum”) by excluding LDH-PF/LDH-serum when a serum LDH level is unavailable. Often clinicians need such options for the care of their patients.

We are interested in the efforts of Porcel and Vives to validate or disprove the cutoff points recommended in our meta-analysis of 1,448 patients by an examination of their much smaller data set (n = 501 to 360 patients) without the assistance of statistical analyses. Based on their conclusion that differences existed among test combinations, they state that the cutoff points suggested in our study “cannot be generalized to a particular practice setting,” and we think that each laboratory must determine its own best cutoff points.” There are several problems with this recommendation.

First, the confidence intervals in Table 1 of their article indicate that no differences existed among test combinations that were the same except for the use of different cutoff points. Second, the small sample size of their report (and the experimental design discussed below) probably accounts for the appearance of differences in point estimates, as suggested by the wide confidence intervals, which are especially notable for their odds ratios. Third, the application of diagnostic tests for all disorders might be enhanced if every institution performed rigorous test evaluations on their local population, but this goal is not often feasible. Fourth, large sets of patient-level data analyzed in a meta-analysis based on high-quality primary studies from multiple institutions are more likely to provide generalizable recommendations than are small data sets, such as those reported by Porcel and Vives, except when the characteristics of the patient populations differ markedly among examined populations. As emphasized in our meta-analysis, however, we found multiple design flaws in the primary studies that limit our recommendations. The studies by Porcel and Vives, unfortunately, share these flaws (see below). Fifth, the “best” cutoff points are often figments of our imagination. Cutoff points are selected on the basis of what we want a test to do and on characteristics of the tested population. If we do not want to miss cases (screening approach), we adjust cutoff points to maximize sensitivity and to sacrifice specificity, and vice versa. Our meta-analysis used a Bayesian technique for determining cutoff points that considered prevalence and misclassification costs. The technique used by Porcel and Vives selected cutoff points that were 1 SD from the mean of the sample that they were using to validate their recommendations. Deriving a new cutoff point from a small validation sample and comparing its performance to a general recommendation from a larger, independent sample biases the sample toward the new cutoff point (which is circular logic). Their approach does not consider misclassification cost, which is an important element in selecting cutoff points.

Our meta-analysis also attempted to point out methodological weaknesses of studies that assess the performance of pleural fluid testing strategies. The most important flaw in these investigations is their retrospective design, which allows clinicians who determine disease classification to have knowledge of test results that are under evaluation (in this instance, Light’s criteria). Such designs use circular logic and bias results (review bias). The Porcel and Vives studies were retrospective without efforts to prevent or control for this source of bias. An additional weakness is the exclusion of patients who have indeterminate disease classifications, which is a major source of bias. We note that 91
of the 455 evaluated patients reported in their letter were excluded from evaluation without an effort to estimate the impact of these exclusions on the study results.

We thank Drs. Porcel and Vives for initiating this dialogue. We think such discussions are important to improve the investigation of diagnostic tests, which has been shown to be in need of improvement in pulmonary research.6 We should conclude that little is gained by parsing cutoff points, sensitivities, or specificities in the specific instance of studying tests that evaluate exudative and transudative effusions: our meta-analysis indicates that clinicians already have many acceptable approaches for making these determinations. We do not think, however, that the contention of Drs. Porcel and Vives that we have had enough research in this area is important. We argue that we need better research that conforms to diagnostic test evaluation standards. Pertinent to the evaluation of exudates and transudates, however, we still teach the Light’s criteria approach for pleural disease research, which has several advantages over other strategies, to medical students. It works. It is venerable. And it recognizes the contributions of a giant in the field of pleural disease research.

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REFERENCES

1 Heffner JE, Brown LK, Barbieri C. Diagnostic value of tests that discriminate between exudative and transudative pleural effusions. Chest 1997; 111:970–979

Should a Cytologic Study Be Ordered in Transudative Pleural Effusions?

To the Editor:

The criteria of Light1 are widely used in clinical practice as a first step in determining whether a pleural effusion is an exudate or a transudate. Traditionally, it has been thought that further diagnostic studies are necessary if the effusion is a transudate. However, at many institutions all undiagnosed pleural fluids are studied cytologically, probably because there is still concern about misleading malignant disease. The original study of Light and colleagues4 involved 43 malignant effusions, 1 of which was classified as a transude. This patient with breast cancer was in congestive heart failure, and the effusion was completely resolved with diuretics. Few studies have attempted to determine the distribution of transudates and exudates in pathologically proved malignant pleural effusions.3–7 and opposite recommendations regarding the necessity for cytologic evaluation in transudative pleural effusions are evoked by the authors of those studies. Whereas some authors favor the routine cytologic evaluation of all pleural effusions even if they are transudates,5–7 others do not recommend it at all when the effusion is transudative.3 We wish to report the results of our study to justify our intermediate position.

We retrospectively reviewed the medical records of 120 consecutive patients with cytologically proved malignant pleural effusions who had been seen at our institution during the previous 3 years. Twenty patients were excluded because of incomplete data on pleural fluid analysis. Of the 100 patients enrolled in the study, there were 58 female and 42 male patients, who had a mean age of 66 years (range, 28 to 91 years). The distribution of malignant tumors was as follows: lung cancer (n = 37); breast cancer (n = 35); ovarian carcinoma (n = 9); unknown primary tumor (n = 5); mesothelioma (n = 3); and miscellaneous tumors (n = 11). Two patients met the criteria of Light for a transude. The first was a 61-year-old man with lung cancer producing atelectasis, a known cause of transudative pleural effusion. The second was a 75-year-old man who presented with a lung mass that proved to be malignant, metastases to the brain and liver, and a bilateral pleural effusion. Because of sufficient clinical clues, the diagnosis of an underlying malignancy would not be misleading despite the presence of pleural fluid with transudative characteristics. However, premature death precluded the evaluation of potential causes for this transude.

As summarized in Table 1, malignant pleural effusions may be transudates in 1 to 10% of patients. The conclusions of several studies can be criticized because they do not report whether there were other explanations for the condition of their patients with transudates cytologically proved to be malignant.5–7 In the largest series from Ashchi et al4 8 patients with transudative malignant pleural effusions among 171 patients were identified, and all except 1 had a satisfactory explanation for the transude.

To conclude, we suggest an intuitive approach, ie, performing a cytologic study in transudates, only when clinical judgment

<table>
<thead>
<tr>
<th>Study</th>
<th>No. of Malignant Pleural Effusions</th>
<th>Transudates, No. (%)</th>
<th>Sensitivity for Malignant Exudates, % (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Molyman et al5</td>
<td>101</td>
<td>7 (6.9)</td>
<td>93.1 (87.6–98.5)</td>
</tr>
<tr>
<td>Ashchi et al4</td>
<td>171</td>
<td>8 (4.6)</td>
<td>95.3 (91.9–98.8)</td>
</tr>
<tr>
<td>Assi et al5</td>
<td>98</td>
<td>1 (1)</td>
<td>98.9 (96.5–100)</td>
</tr>
<tr>
<td>Castro et al6</td>
<td>122</td>
<td>13 (10.6)</td>
<td>89.3 (83.4–95.2)</td>
</tr>
<tr>
<td>Foresti et al6</td>
<td>106</td>
<td>4 (3.7)</td>
<td>96.2 (92.1–100)</td>
</tr>
<tr>
<td>Current series</td>
<td>100</td>
<td>2 (2)</td>
<td>98 (94.8–100)</td>
</tr>
</tbody>
</table>

*CI = confidence interval.
dictates that the pleural effusion is not related to the few conditions associated with transudates.

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REFERENCES
1 Light RW. Pleural diseases. 3rd ed. Baltimore, MD: Williams & Wilkins, 1995; 36–74

Substitution of Arm Span for Standing Height Is Important for the Assessment of Predicted Value of Lung Volumes in Elderly People With Osteoporosis

To the Editor:

We read with great interest the article by Aggarwal and coworkers (February 1999) concerning the interpretation of spirometric data in relation to anthropometric indexes. The authors have demonstrated that height estimated from arm span can be substituted for actual height in patients in whom height cannot be measured reliably. This is very important not only for patients with lung disease and spinal deformity, but also for older adults with osteoporosis.

Although respiratory function is considerably affected by age, height and anthropometric indexes also are affected by age. Osteoporotic vertebral fractures increase in number and incidence with age, and changes in the shape of the thorax lead to increased dorsal kyphosis and anteroposterior diameter. The reported prevalence of vertebral crush fractures in the United Kingdom is 2.5% for women >60 years old and reaches 7.5% for those 80 years old. The kyphosis and deformation of the thoracic cage secondary to osteoporosis impair pulmonary function, particularly vital capacity, in aged woman. Thoracic kyphosis as measured by Cobb’s angle is significantly associated with the FVC in women referred for osteoporosis evaluation. Because height is considerably affected by the vertebral fractures, Leech et al have suggested that arm span should be used for predicting lung function instead of height. Further, we also found that thoracic kyphosis as measured by Cobb’s angle was significantly associated with maximal inspiratory pressure (Pimax) and the ratio of residual volume to total lung capacity in elderly people. Because Pimax is influenced by the curvature of the diaphragm, the kyphosis-related alteration of diaphragmatic shape may reduce the pressure on inspiratory muscles.

Because an understanding of the normal progression of changes in respiratory function is important in assessing the loss in pulmonary reserve for elderly people with lung disease, height estimated from arm span may be important for the assessment of lung function in osteoporotic patients with or without lung disease.

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REFERENCES
1 Aggarwal AN, Gupta D, Jindal SK. Interpreting spirometric data: impact of substitution of arm span for standing height in adults from North India. Chest 1999; 115:557–562

To the Editor:

We appreciate the interest shown by Teramoto and colleagues in our article (February 1999) and we fully agree with their observations on osteoporosis-induced height changes in the elderly. In fact, we had concluded that arm span is a reasonable surrogate for standing height in patients in whom height cannot be reliably measured, provided that a proper relationship between height and arm span is not available for that population. It may be worthwhile to evolve such a relationship for elderly patients. This would be useful not only for interpreting pulmonary function data, but also for other clinical assessments requir-
Carcinoid-Related Intrapulmonary Shunting May Be Associated With Increased Production of Nitric Oxide

To the Editor:

In a recent issue of CHEST, Lee and Lepler (April 1999)1 rationally discussed the mimicking of hepatopulmonary syndrome (HPS) by the severe intrapulmonary shunting that is caused by carcinoid syndrome.

HPS is caused by hypoxemia in patients with chronic liver diseases in the absence of intrinsic lung disease, and so carcinoid-related intrapulmonary shunting and hypoxemia are not exactly the same as HPS. However, HPS may occur secondary to a functional right-to-left shunt because of intrapulmonary vascular dilatation. Thus, the shunting in that situation may mimic the pathogenesis of HPS in carcinoid syndrome. Furthermore, recent evidence suggests that nitric oxide (NO) is an important mediator of impaired oxygenation in patients with cirrhosis (ie, HPS).2,3 The increased production of NO by metastatic carcinoid tumors in the lung may be another cause of extraordinary vasodilation and intrapulmonary shunting, resulting in severe hypoxemia in carcinoid syndrome.

Patients with carcinoid syndrome are known to produce the vasoconstrictor 5-hydroxytryptamine. However, NO, a major vasodilator, also may be produced in patients with carcinoid syndrome.4 Although carcinoid-related pulmonary shunting may be responsible for hypoxemia in the case of the patient reported by Lee and Lepler who has metastatic carcinoid tumors, the pathogenic mechanism of severe hypoxemia may not be simple in carcinoid syndrome. Because many neurohumoral vasoactive substances can be released by tumors, the complex regulatory mechanism may work in the lungs of the patients. Further analysis of vasodilators and vasoconstrictors may be important to elucidate the mechanism of hypoxemia in the carcinoid syndrome.

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References

Chronic Diaphragmatic Hernia

To the Editor:

I read with interest the article published by Seelig et al (January 1999).2 Chronic diaphragmatic hernias have a high rate of mortality and morbidity, as has been stressed by the authors. They also represent a diagnostic and therapeutic challenge for surgeons. There are two aspects of this report that I would like to emphasize. First, the diagnosis of traumatic injuries of the diaphragm should be performed ideally at an early stage (ie, in the acute setting of the trauma). Diagnosis may be easy if diagnostic ancillary studies are conclusive or if the patient requires an emergency operation. Unfortunately, there are a significant number of patients that have neither of these, and injuries to the diaphragm will go undetected. Murray et al2 reported recently a prospective study in which 24% of patients with penetrating trauma to the left lower chest had occult diaphragmatic injuries. In order to reduce this number of patients with occult diaphragmatic injuries, they proposed performing a diagnostic laparoscopy or thoracoscopy. Minimally invasive procedures are currently being assessed in several trauma centers and may become the “gold standard” diagnostic modality for ruptured diaphragms in the near future.

The second aspect I would like to point out is the surgical approach for chronic diaphragmatic hernias. In my experience, and in that of others,3,4 thoracotomy has a significant advantage over laparotomy. There are two fundamental reasons for this. Diaphragmatic hernias do not have a true hernia sac, and the reduction of larger portions of viscerum (spleen, stomach, and bowel) from the chest can be hindered by adhesions between the abdominal and thoracic structures,5 and there can be an increased risk of causing iatrogenic injuries due to the awkward exposure through the abdomen. On the other hand, patients with complications such as gangrenous or perforated viscerum will rapidly develop empyema, which requires complete drainage and complete reexpansion of the lung. The latter is achieved by taking down adhesions and decorticating the lung. This is best accomplished through the chest, as occurs with other traumatic empyemas.6

I congratulate Dr. Seelig and colleagues for bringing attention to this difficult surgical problem.

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Reference
1 Aggarwal AN, Gupta D, Jindal SK. Interpreting spirometric data: impact of substitution of arm span for standing height in adults from North India. Chest 1999; 115:537–562
In view of these data, it is not surprising that the measurement of pH has proven to be a good early predictor of organ dysfunctions and failures, of death, or of both. It is also not surprising that measures directed toward preventing a fall in gastric pH and restoring the gastric pH to normal in a timely manner in anaerobiosis are associated with improved outcomes and reduced ICU and hospital stays and costs. In this respect, it is important, in terms of monitoring, to note that using a measure of normal gastric pHi as a goal in resuscitation is significantly better in achieving a normal pHi in a timely manner than is using the empirical goals of global oxygen delivery and consumption, which are often redundant or ineffective.

Despite this logic and the weight of these clinical data, Dr. Fink dismissed the indirect measurement of pHi because it appeared to underestimate the magnitude of the tissue acidosis present in low-flow and no-flow states in his and our earlier validation studies due, Dr. Fink claims, to the bicarbonate assumption being invalid and to several other variables that he cites. Not only is his conclusion unwarranted, even in no-flow and low-flow states as previously argued, but it is clinically irrelevant whether the indirect measurement of pHi underestimates the severity of acidosis or not. Moreover, the measurement has proved to be remarkably robust in clinical practice despite earlier concerns.

The gastric pH measured in healthy volunteers who were given H2-receptor antagonists falls within a narrow range, and a fall below these tight statistical limits of a normal range establishes the presence of an intramucosal acidosis and provides the means to measure the degree of tissue acidosis and, hence, the degree to which oxidative phosphorylation has been compromised. In septic patients, a fall in gastric pHi appears to be a particularly sensitive indication of the degree of global anaerobiosis present because pHi can be reversed in a dose-related manner, and when it is elevated, arterial lactate can be reduced in parallel by incremental increases in oxygen delivery induced by dobutamine.

There are three other reasons why gastric pHi is an indicator of the degree of systemic anaerobiosis. First, the superficial layers of the gut mucosa are the “canary” of the body, being among the first parts of the body to be compromised in low-flow states. Second, gut mucosal injury releases substances into the circulation that may further compromise, either directly or indirectly, tissue oxygenation. Third, the incorporation of the arterial bicarbonate level into the indirect measurement of pHi allows it to reflect the systemic contributions of anaerobiosis and to add to the predictive value of intramucosal PO2 for adverse clinical events.

Systemic metabolic indexes of anaerobiosis, which are the product of tissue washout, are inevitably less sensitive and potentially more specific predictors of outcome in patients with advanced disease, these indexes being paradoxically more abnormal in patients with perfused tissues than in those with unperfused tissues. The intramucosal PO2 alone is not as good a predictor of outcome as the gastric pH7,14. The more specific indexes of regional tissue “perfusion” and/or “dysxia,” namely, pHi-gap and PO2-gap, also have proven to be less sensitive and accurate, but in patients with severe sepsis they are more specific predictors of outcome and in patients with advanced cardiac pulmonary failure their accuracy may be superior to that of Mg-ATP. In this respect, it is worthwhile noting that the pulse, a poor predictor of outcome in hemodynamic stability, is a most accurate predictor of impending death, a patient having no pulse in cardiac arrest and a pulse not in arrest. The deficiencies in these indexes of regional tissue “perfusion” and/or “dysxia” in predicting outcome relative to gastric pHi is not surprising, because the gastric pHi may fall in anaerobiosis not only as a result of a rise in tissue PCO2 above arterial PCO2 in low-flow

References


Monitoring of Tissue pH

The Critical Measurement

To the Editor:

Dr. Fink’s editorial on tonometry in CHEST (September 1998) addresses the ability of tonometry to monitor tissue perfusion to the exclusion of its ability to monitor the adequacy of tissue oxygenation. Indeed, his editorial confuses the two in referring to the intramucosal pH (pHi) as a measure of tissue perfusion rather than of tissue oxygenation. Most surprising is Dr. Fink’s failure to address the predictive value of the different measurements and the impact that they have not only on patient outcome, but also on costs, when used to assist in routine patient management. Surely, these are the most critical issues in recommending any form of monitoring for routine patient care.

Dr. Fink’s editorial focuses on the physiologic basis of the measurements. He concedes that knowing that the tissue pH is normal is enough for him to conclude that “perfusion is sufficient to meet the metabolic demands of the cells in that tissue.” He appears to acknowledge that the severity of tissue acidosis in anaerobiosis is related to the degree of impairment of oxidative phosphorylation and to the associated unreversed adenosine triphosphate (ATP) hydrolysis2 whether it is due to the retention of protons from increased glycolytic ATP turnover or to ATP breakdown,3 which is the degree to which oxidative phosphorylation, which consumes the protons released by ATP hydrolysis, has failed to replenish depleted ATP stores and, hence, to maintain the “adequacy of tissue oxygenation.” This is consistent with the conclusion of the National Institutes of Health (NIH) Task Force on Cardiopulmonary Dysfunction in Critical Care Medicine in 1994 that gastric pHi is a “metabolic signal of tissue hypoxia.”4

Maintaining tissue acid-base balance and adequate aerobic ATP synthesis by oxidative phosphorylation is essential for all bodily activities, including tissue repair, wound healing, bacterial phagocytosis, and cellular survival. The accumulation of protons in anaerobiosis compromises nonessential functions, such as lactate efflux from muscles and acid secretion, and essential functions, such as myocardial contractility. The continued accumulation of protons and later net ATP degradation compromises essential ATP-dependent functions, such as the sodium pump, and cells swell and die. ATP degradation allows free radicals to be generated on resuscitation.2,3

In view of these data, it is not surprising that the measurement of pH has proven to be a good early predictor of organ dysfunctions and failures, of death, or of both. It is also not surprising that measures directed toward preventing a fall in gastric pH and restoring the gastric pH to normal in a timely manner in anaerobiosis are associated with improved outcomes and reduced ICU and hospital stays and costs. In this respect, it is important, in terms of monitoring, to note that using a measure of normal gastric pHi as a goal in resuscitation is significantly better in achieving a normal pHi in a timely manner than is using the empirical goals of global oxygen delivery and consumption, which are often redundant or ineffective.

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Systemic metabolic indexes of anaerobiosis, which are the product of tissue washout, are inevitably less sensitive and potentially more specific predictors of outcome in patients with advanced disease, these indexes being paradoxically more abnormal in patients with perfused tissues than in those with unperfused tissues. The intramucosal PO2 alone is not as good a predictor of outcome as the gastric pH7,14. The more specific indexes of regional tissue “perfusion” and/or “dysxia,” namely, pHi-gap and PO2-gap, also have proven to be less sensitive and accurate, but in patients with severe sepsis they are more specific predictors of outcome and in patients with advanced cardiac pulmonary failure their accuracy may be superior to that of Mg-ATP. In this respect, it is worthwhile noting that the pulse, a poor predictor of outcome in hemodynamic stability, is a most accurate predictor of impending death, a patient having no pulse in cardiac arrest and a pulse not in arrest. The deficiencies in these indexes of regional tissue “perfusion” and/or “dysxia” in predicting outcome relative to gastric pHi is not surprising, because the gastric pHi may fall in anaerobiosis not only as a result of a rise in tissue PCO2 above arterial PCO2 in low-flow
states but also as a result of a fall in tissue bicarbonate in conjunction with a rise in PCO₂ or of a fall in bicarbonate alone in perfused tissues.

The pHᵢ correlates poorly with tissue perfusion (flow) and was never intended to be a measure of flow, as Dr. Fink suggests. The PCO₂ and especially the PCO₂-gap, is a regional measure of tissue perfusion (flow) to the degree that it allows the intramucosal PCO₂ to equilibrate with that in arterial blood. But Dr. Fink used proton accumulation in anaerobiosis, not proton consumption, by oxidative phosphorylation in restoring the tissue acid-base balance in his consideration of the fate of tissue CO₂. The relative contributions of intramucosal PCO₂ and arterial bicarbonate to pH depend on the degree of CO₂ removal by flowing blood and by exhalation from the lungs.

On-line tonometric monitoring of tissue PCO₂ in less invasive extra-enteric locations, such as the esophagus, the sublingual space, or even the urinary bladder, is appealing for simplicity, ease of access, and absence of variables such as acid secretion, especially as an aid in the detection of acute circulatory failure. Caution is due because of the unique role of the gut as the “canary” of the body, as Dr. Fink acknowledges, and, hence, it is the preferred choice as the monitoring site for the early detection of low flow when small changes may be clinically significant. Caution is also due with the indwelling sensors that Dr. Fink considers, because all sensors may become “poisoned,” may drift, and may not be able to be recalibrated in vivo. Even if the sensors are drift free, as Dr. Fink claims, the question is whether there is any outcome or cost benefit to making tonometric measurements any more often than routine blood gas analyses.

Dr. Fink concludes that “in contrast to the complexity of sepsis, patients with elevated tissue PCO₂ values during a major surgical procedure or during resuscitation from hemorrhagic or cardiogenic shock have a relatively straightforward problem: oxygen delivery in the microvasculature is inadequate to meet metabolic demand.” Agreed. But the detection of a fall in pH in perfused tissues caused partially or wholly by a fall in bicarbonate might be delayed or missed if one relied on the measurement of intramucosal PCO₂ and even PCO₂-gap alone, in monitoring patients, especially in an extra-enteric site. He adds: “The solution to the problem is equally straightforward: improve perfusion or the oxygen content of arterial blood.” Is he that sure? Beware the development of unimpaired ATP hydrolysis in perfused tissues that is reflected in a fall in bicarbonate during and after conventional resuscitation, the adverse effects of inotropes that increase oxygen consumption, and the assumption that increases in arterial oxygen content improve capillary oxygen content especially when increased with transfusions of RBCs >10 days old. This is exactly where we have been misled in the past by global measurements of oxygen transport, consumption, and extraction.

Contrary to Dr. Fink’s statement, manual gastric balloon tonometry can be easily incorporated into routine clinical care, as has been the case in Argentina and at the University of Miami, and this inclusion has been rewarded with substantial improvements in outcomes, seemingly even in patients with advanced disease in whom reductions in treatment costs also were observed. The failure of gastric tonometry to have been incorporated into routine clinical care is surprising given its long-established physiologic and clinical credentials. In my view, it has not been because it is cumbersome and too expensive, as Dr. Fink claims. Indeed, when considered in terms of incremental dollars spent per life-year saved, pHᵢ-guided resuscitation with manual measurements that are performed in all ICU patients would seem infinitely more cost-effective than any other form of established management protocol in adequately ventilated patients.

Finally, let us not lose sight of the overriding importance of mitochondrial ATP synthesis by oxidative phosphorylation in all bodily activity at all times. The NIH Task Force on Cardiopulmonary Dysfunction in Critical Care Medicine recognized the need for monitoring alterations in the “energetic state of the tissues.” The intramucosal pHᵢ is a physiologically appropriate and clinically validated measurement that, despite its imperfections, appears to do this very simply and better than I ever would have thought. Most importantly, when incorporated into routine care, gastric pHᵢ measurement has improved our understanding of disease processes, has aided in management decisions, and has helped to improve outcomes and to reduce costs substantially. Let us use pHᵢ in conjunction with measures of PCO₂, PO₂, and PCO₂-gap to audit and direct care until better and more representative measures of tissue energetics emerge and have been shown in routine clinical practice to improve outcomes and to reduce costs further.

I have had no business dealings with the Tonometrics Division of Datex-Ohmeda (Helsinki, Finland), the company commercializing silicone balloon tonometry in both its manual and on-line forms. I am not receiving financial support from any company. Furthermore, due to the indirect nature of my contracts, I have absolutely no idea whether I still have any financial interest in any viable company remotely related to the commercialization of tissue gas analysis, including Datex-Ohmeda. I have absolutely no legal or political agenda. I simply remain concerned that the opportunity to improve outcome and reduce costs on a large scale is not being realized.

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REFERENCES

3 Dennis SC, Gevers W, Opie LH. Protons in ischemia: where do they come from; where do they go. J Mol Cell Cardiol 1991; 23:1077–1086
To the Editor:

I appreciate the opportunity to respond to the very long letter by Dr. Fiddian-Green. I will keep my comments brief.

Tissue Pco₂ is a function of a number of parameters, including the rate at which CO₂ is produced by oxidative metabolism in the tissue, the rate at which CO₂ is produced by titration of bicarbonate anions (HCO₃⁻) by protons in the tissue, and the rate at which CO₂ is removed from the tissue by blood flowing through its capillary network. Intraluminal Pco₂ in certain locations, like the stomach or colon, is a function of mucosal Pco₂ but also can be influenced by other factors, including titration of secreted HCO₃⁻ by secreted protons and production of CO₂ as a result of the fermentation of various fuels in food or feces by microbes. Finally, in all locations perfused by arterial blood, tissue Pco₂ is directly influenced by arterial Pco₂. In view of this information, it seems reasonable to conclude that tissue Pco₂ and intraluminal Pco₂ are measures of neither the adequacy of tissue perfusion nor the adequacy of tissue oxygenation but, rather, are clinically useful parameters that can be influenced in complex ways by a number of factors.

Tissue pH and tissue Pco₂ tend to be inversely related, and both of these parameters often are determined by the “adequacy” of tissue oxygenation (ie, tissue Po₂). In most circumstances, a low tissue pH or a high tissue Pco₂ can be interpreted as evidence of low tissue Po₂. In sepsis, however, systemic and/or tissue acidosis can occur despite a high tissue Po₂. Accordingly, it is not valid to state that intramucosal pH (pHi) always provides unambiguous information about tissue oxygenation.

Tissue Pco₂, whether used directly or used to calculate a derived variable (eg, gastric pHii) is a very appealing index for monitoring critically ill patients. Yet, despite the publication of hundreds of articles and abstracts regarding the value of tissue Pco₂ (or pHii) monitoring, the use of gastric tonometry or related approaches remains largely a research tool. Certainly, routine monitoring of tissue Pco₂ is currently used in very few ICUs or operating rooms in the United States. There are probably many reasons for why clinicians have been reluctant to adopt this technology. Certain issues, however, such as unfamiliarity, inconvenience, and perceptions about the acquisition costs for commercially available devices seem likely to be playing a role. It is my view that wide adoption of tissue capnometry is unlikely to occur until inexpensive devices that provide a continuous readout, analogous to that provided by a pulse oximeter, become available. It is my understanding that one such device is being developed by a commercial entity (Optical Sensors, Inc; Minneapolis, MN). I have no connection with or financial interest whatsoever in this company.

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REFERENCES


Screening of Tuberculosis

Is It a Real Prediction Model?

To the Editor:

We read with great interest the article by Tattevin et al (May 1999) regarding the diagnosis of tuberculosis and the predictive model for diagnosis that they propose. In recent years, and certainly motivated by the increasing number of cases and the appearance of nosocomial outbreaks, measures directed to diagnose tuberculosis early and to isolate patients with active disease have been promoted. We agree with Tattevin et al about the disappointing results that the application of predictive models for diagnosis have had: no model is yet feasible for application in our clinical practice.

On the other hand, the greater is our concern about nosocomial outbreaks, the higher is the number of patients isolated and the more numerous are the samples for which acid-fast bacillus (AFB) smear and culture are requested. Are all those samples really necessary? Should a specialist screen the AFB smear and culture are requested. Are all those samples really necessary? Should a specialist screen the AFB smear and culture requests before they are performed? For this purpose, we performed a survey in our hospital that was designed to answer both questions. For a 60-day period, two pulmonologists independently evaluated every request for an AFB smear and determined whether it was adequately ordered or not. During that period, 134 smears were requested, and 81% of smear results were negative. According to the opinions of both pulmonologists, the requests for AFB smear in 99 samples (73%) were not justified. Disagreement between the two specialists occurred only in 13 samples that ultimately yielded negative results. None
of those samples that tested positive escaped the screening of both specialists (100% of sensitivity). When evaluating the economic impact of those unnecessary samples, and even considering the costs derived from the specialist’s consultation, we determined that nearly $12,000 of unnecessary costs had been incurred. Moreover, the social and personal costs incurred by every patient who was unnecessarily isolated cannot be calculated. Because of these findings, we wonder whether it would be cost-efficient for one specialist to screen every patient with suspected tuberculosis on their arrival at the hospital. Considering the application of a predictive model to identify active cases early, it would require trained clinicians for adequate performance. Thus, the use of trained personnel to evaluate these patients could save money and avoid worrisome social and personal situations.

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REFERENCES

Bacteremic Community-Acquired Pneumonia in an Immunocompetent Adult Due to Burkholderia cepacia

To the Editor:

We report what we believe is the first case of community-acquired pneumonia (CAP) due to Burkholderia cepacia (formerly Pseudomonas cepacia) described in an immunocompetent adult. Although well recognized as an opportunistic pulmonary pathogen, particularly in patients with cystic fibrosis, the only prior report of pneumonia due to B cepacia in an immunocompetent host occurred in a 14-year-old boy.1 Although no underlying pulmonary disease was apparent, the possibility of underlying cystic fibrosis was raised.2

A 32-year-old African-American man presented to our hospital with a 1-week history of fever, cough productive of thick yellow-green sputum, dyspnea, nausea, and vomiting. Mild hypertension, treated with fosinopril, was the patient’s only prior medical condition, and he denied alcohol or cigarette consumption. He admitted to occasional marijuana use but denied any other illicit drug use. He was employed as a cook and lived with his wife and child. There was no family history of any pulmonary illness, and he had had no recent contact with anyone with any pulmonary illness.

On examination, the patient had a temperature of 38.4°C, a heart rate of 90 beats per minute, a respiratory rate of 22 breaths per minute, and a BP of 110/68 mm Hg. Oxygen saturation was 97% by oximetry. He had no clubbing but had coarse crackles at the left posterior lung base on chest auscultation. There were no other abnormal findings. Laboratory studies showed a total WBC count of 25 × 10⁶ cells/L, with normal levels of electrolytes, and normal results of renal and hepatic function tests. A chest roentgenogram showed consolidation in the left lower lobe.

The patient consented to an outpatient pharmaceutical trial comparing two quinolone antibiotics. A sputum culture, including a specific culture for Legionella species, did not grow any pathogens. Results of a Legionella urinary antigen test were negative. Throat and nasal swabs were also negative for Chlamydia and Mycoplasma species. Results of paired acute and convalescent serology for Legionella pneumophila (serogroups 1 to 6), Chlamydia pneumoniae, Chlamydia psittaci, Chlamydia trachomatis, and Mycoplasma pneumoniae all showed no evidence of infection. The single blood culture grew an unusual form of B cepacia that was fully sensitive to ofloxacin (including both study drugs), ceftaxime, and piperacillin.

The recovery of the patient was uneventful, and he resumed normal activities, including competitive basketball, within 14 days of starting antibiotics. The results of a physical examination and spirometry 1 month after starting therapy were completely normal.

In the 6 months preceding this patient’s presentation, only one other isolate of B cepacia was cultured at our hospital. This isolate was resistant to ofloxacin, ceftaxime, and piperacillin. On the basis of the scarcity of cultures and the marked differences in antibiotic sensitivities between the two isolates, we are confident that the positive result of the culture was not due to laboratory contamination. The absence of any other microbiological diagnosis despite extensive testing also supports the identity of the pathogen as B cepacia.

There is no evidence of cystic fibrosis or any other chronic lung disease in our patient. He had no known exposure to any source of B cepacia, and the fact that the strain was unusually sensitive to drugs is evidence against him having acquired it from someone with chronic lung disease. B cepacia was first identified in agricultural produce,3 so, inasmuch as one of the patient’s job responsibilities is food preparation, he may have acquired it at work. Concern has been raised about the potential use of B cepacia as a biopesticide in the agricultural industry,4 but we have been unable to confirm whether it is being used in our region.

In conclusion, we have reported a case of bacteremic CAP due to Burkholderia cepacia in an immunocompetent host who has no underlying lung disease. Unlike most cases involving B cepacia, the most likely source of this infection is from a nonhospital environment, probably from agricultural produce.

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Dr. Waterer has been supported by a grant from the Methodist-LeBonheur Healthcare Foundation and by the Athelstan and Amy Saw Medical Research Fellowship from the University of Western Australia.

REFERENCES
2. Cohen MS, Knowles MR, Yankaskas JR. Infection due to...
Perioperative Cardiopulmonary Evaluation and Management

Are We Ignoring Obstructive Sleep Apnea Syndrome?

To the Editor:

As pulmonary physicians, we greatly appreciate the publication of an exhaustive and comprehensive review of the cardiopulmonary issues relevant to the perioperative period (May 1999). However, being somnologists too, we could not help noticing that there was no mention of obstructive sleep apnea syndrome (OSAS) as a perioperative risk factor. Was that because OSAS is classified as a sleep disorder rather than a respiratory disease? More likely, the reason is a lack of published data other than a few case reports on this subject, which undermines the importance of OSAS as a risk factor. In preliminary observations, we have noted that serious and life-threatening complications may occur in patients with unrecognized OSAS. Forced supine posture and narcotic analgesics may increase the frequency and severity of upper airway obstructions postoperatively. In contrast to some cardiopulmonary complications, these events may be easily preventable by perioperative use of continuous positive airway pressure therapy. Urgent steps are needed to increase awareness of the potential for postoperative complications as an important consequence of OSAS in addition to widely recognized consequences such as daytime sleepiness and increased cardiovascular risk.

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REFERENCES

3 Allen L. The peritoneal stomata. Anat Rec 1936; 67:89–103

Catamenial Pneumothorax and Its Relation to the Peritoneal Stomata of the Diaphragm

To the Editor:

The majority of catamenial pneumothoraces (90–95%) are right-sided, and the pathogenesis remains controversial. In 1936, Allen defined peritoneal stomata of the diaphragm as normal openings of the underlying lymphatics. He also reported that intraperitoneal particles and solution are sucked through these openings in mice and that similar stomata also have been recognized in humans. Diaphragmatic lymphatics represent the major pathway for peritoneal fluid drainage (about 90%) in cats, and absorption tends to be more extensive on the right side. In addition, the subdiaphragmatic lymphatics communicate with the pleural surface, and thus, even though not all cases have been proven to have diaphragmatic endometriosis and/or defects, the pathogenesis seems mainly to be due to the absorption of intraperitoneal endometrial tissue from the stomata of the right hemidiaphragm. The same phenomenon also seems to play a role in other right-side-predominant diseases (i.e., Meigs’ syndrome).

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REFERENCES

3 Allen L. The peritoneal stomata. Anat Rec 1936; 67:89–103

Safety of Neuraxial Anesthesia in Patients Receiving Perioperative Low-Molecular-Weight Heparin for Thromboprophylaxis

To the Editor:

I read with interest the recent article by Aguilar and Goldhaber (May 1999) on the subject of low-molecular-weight heparins (LMWHs). LMWHs are widely employed in the perioperative period for the prophylaxis of deep venous thrombosis (DVT) and are considered to be safe and effective. However, the decision to use LMWHs may not be made without some trepidation. When deciding to perform neuraxial blockade on a patient who is already receiving an LMWH, the anesthesiologist may believe that the benefits of regional anesthesia for this specific patient are greater than the risk of developing intraspinal bleeding, even assuming that the patient has had an alteration in hemostasis.

So, what should be done to minimize the very low risk of developing neurologic injury while maintaining the antithrombotic effectiveness of LMWHs? I would like to propose a checklist (Table 1) and to summarize some recommendations

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to improve the safety of neuraxial anesthesia in patients who have received or will receive thromboprophylaxis with LMWHs.\textsuperscript{4–7}

1. **Postoperative administration of LMWH.** Except in some specific patients (very high-risk patients or those having surgery for a hip fracture), it is possible to perform thromboprophylaxis with LMWH starting 10 to 12 hours after surgery without a loss of effectiveness. This would be the best choice for improving the safety of neuraxial anesthesia.

2. **Preoperative LMWH.** If LMWH has to be administered before surgery, spinal anesthesia should be delayed at least 10 to 12 hours after the last LMWH dose, with the next dose being given 2 to 4 hours after that initial dose.

3. **LMWH dosing.** It has to be the minimal effective dose. Sometimes an LMWH dose can be adjusted for body weight to avoid overdosing. Furthermore, it is of primary importance to know the differences in drug regimens between Europe and the United States: the current American dosing practice is to administer 50% more than is called for by the current European practice (eg, enoxaparin, 30 mg twice daily vs 40 mg once daily, respectively).

4. **Continuous catheter technique.** There is no problem in using it if the LMWH is started postoperatively or at least 12 hours before. It is not safe to place a catheter for postoperative analgesia when LMWH has been administered < 10 to 12 hours before. However, frequent monitoring for signs of neurologic impairment is needed in all cases. The removal of this catheter has to be delayed until 10 to 12 hours after the last dose of LMWH (ideally, 24 hours).

5. **Remaining questions.** If the decision is made to perform neuraxial anesthesia despite the prior administration of LMWH, the following procedures are recommended:

   - Do not use the continuous-catheter technique. A single-dose spinal anesthesia may be the safest regional anesthesia technique.
   - Use small-gauge needles.
   - Use short-acting anesthetics.
   - Use the midline approach.
   - Observe the patient closely in the early postoperative period for any sign of cord compression, and if signs are noticed, obtain immediate radiographic confirmation and treat the patient without delay.

In conclusion, the decision to perform regional anesthesia in patients receiving LMWH for prophylaxis of DVT can be made safely in selected patients. Some anesthesia and thromboprophylaxis guidelines should be followed to minimize the risk of spinal bleeding. However, the final decision should be made by the clinician after estimating the benefits and risks of the simultaneous use of LMWH and regional anesthesia.

**References**


**Allergic Bronchopulmonary Aspergillosis or Bronchopulmonary Aspergillosis With Asthma**

**Which One Is More Appropriate?**

To the Editor:

The term “allergic bronchopulmonary aspergillosis” (ABPA) has not been well defined since its first use in 1952.\textsuperscript{1} ABPA is a condition caused by Aspergillus colonization occurring in a patient with asthma that manifests itself as an allergic response that is apparent from the presence of peripheral eosinophilia (> 1,000/\(\mu\)L), an increased amount of total and Aspergillus-specific IgE and IgG, and the presence of immediate wheal and flare response of type-I hypersensitivity,\textsuperscript{2} with good clinical response to steroid therapy.\textsuperscript{3} But, there are other clinical findings that cannot be explained by allergy or type-I hypersensitivity. ABPA features tissue destruction, which is evident from occasional mucopurulent sputum, and even small instances of hemoptysis, with fever that often causes confusion with bacterial pneumonia.\textsuperscript{4} Radiologically, there is evidence of consolidation and atelectasis and finger-like shadows on radiographs of patients in the acute state,\textsuperscript{5} and there are features of fibrosis and proximal bronchiectasis, and even extensive lung destruction in chronic cases.\textsuperscript{6,7} Serologically, the IgG level is raised and the serum precipitin test is positive for Aspergillus.\textsuperscript{8} ABPA has a spectrum of clinical variations, with some patients showing progressive lung damage despite steroid therapy,\textsuperscript{9} and there have also been cases of amyloidosis secondary to ABPA.\textsuperscript{8} Hence, there must be elements of other types of hypersensitivity reactions concomitant with the type-I reaction.

So, use of the generalized term “allergic” in “allergic bronchopulmonary aspergillosis” (ABPA) should be avoided. Instead, it is more appropriate to use the term “allergic bronchopulmonary aspergillosis” (ABPA) without contamination.

**Table 1—Central Nerve Block-LMWH Administration Safety Checklist\textsuperscript{*}**

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<th>Procedure</th>
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<td>LMWH administration</td>
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<td>Preoperative (≥ 12 h)</td>
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<td>Postoperative (≥ 12 h)</td>
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<td>Next dose administered at least 4 h after surgery</td>
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<td>Is the LMWH dose the minimal effective dose?</td>
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<td>Catheter placement</td>
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<tr>
<td>Preoperative (≥ 12 h) LMWH administration</td>
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<tr>
<td>Postoperative (≥ 12 h) LMWH administration</td>
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<tr>
<td>Catheter will be removed at least 12 h after the last LMWH dose</td>
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</tbody>
</table>

\* Yes = central nerve block is safe; No = need to estimate benefits and risks to perform central nerve block.

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monary aspergillosis” is an oversimplification of the condition. As long as ABPA is not further classified on a more stringent scientific basis, a term such as “bronchopulmonary aspergillosis with asthma” would perhaps be more appropriate and descriptive.

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REFERENCES