ventilatory parameters were larger in patients with La-2.2 than in those without, and that both heart rate and respiratory reserves were smaller in those with La-2.2 than in those without. They concluded that pain from the surgical wound in the chest could be the most important limiting factor for patients without La-2.2 in the early postoperative period, even though subjective factors were not obtained in their study.

We performed a similar prospective study on 122 patients who were enrolled in our study from December 1999 through February 2001 and had undergone muscle-sparing thoracotomies and lung resections (segmentectomy/wedge resection, 15 patients; lobectomy, 88 patients; pneumonectomy, 19 patients) for lung carcinoma. Our exercise methodology consisted of a maximal symptom-limited, stair-climbing test that was administered at the time of the patient’s discharge from the hospital (mean, 8.2 ± 3.3 postoperative days). In our study, 24 patients (19.7% of our series) did not reach the empirical anaerobic threshold of La-2.2 Vo2. However, no significant differences were detected between this group of patients and the others who reached the La-2.2 Vo2, in terms of calculated work, maximal Vo2 (Vo2max; expressed as milliliters per minute, milliliters per minute per kilogram, or as the percentage of the predicted value), Vo2max/body surface area ratio, and O2 pulse. Moreover, hemodynamic variables (i.e., cardiac output, cardiac index, oxygen delivery, extraction ratio, and heart rate reserve), which were calculated by the Fick method, did not result in significant differences between the two groups of patients.

The only parameter that was significantly reduced in the group without La-2.2 vs the group with La-2.2 was the number of steps climbed (mean, 77.2 ± 30.8 vs 101.6 ± 33.2, respectively; p = 0.001 [Student’s t test]).

We performed also a subjective analysis concerning the main symptoms that limited the patients’ exercise, and we found no difference between the two groups of patients. In particular, only three patients in the group without La-2.2 stopped exercising because of chest pain at the surgical wound. Dyspnea was the predominant symptom, followed by leg pain and physical exhaustion, in both groups of patients.

Contrary to what was reported by Miyoshi et al.,1 we believe that in the early postoperative period the chest pain from thoracotomy is not the most important limiting factor for the patients who do not reach the empirical anaerobic threshold. However, differences in the type of thoracotomy incision and in the methodology of the exercise test have to be taken into account.

Since there was no difference in the values of ergometric variables between the two groups of patients in our series, a low level of arterial blood lactates at peak exercise in some patients may be explained by a previously reported intersubject variability of the anaerobic threshold value3 or may simply reflect the better fitness of some individuals for endurance (aerobic) exercise.4

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To the Editor:

We thank Dr. Brunelli and his colleagues for their interest in our article (August 2000).1 They performed a similar prospective study on lung cancer patients in the early postoperative period (mean ± SD) postoperative days, 8.2 ± 3.3), using a stair-climbing test. Although they observed patients without a venous blood lactate level of 2.2 mmol/L (La-2.2) [24 subjects; 19.7% of their series], as we did, there was no difference in the values of ergometric variables between patients with and without La-2.2. These findings were different from our results.

As has been pointed out, there were two major differences between our study and that of Brunelli et al. We employed a standard posterolateral thoracotomy in all patients studied, while Brunelli et al performed a muscle-sparing thoracotomy in their patients. This variation may produce differences in postoperative chest pain and compliance of the chest wall, especially in the early postoperative period. Patients are likely to be able to attain a larger maximum oxygen uptake (Vo2max) if they do not feel chest pain during exercise. The differences between these thoracotomy approaches were not detected by pulmonary function testing. A cardiopulmonary exercise test is a loading test for both cardiovascular and respiratory systems, and may be more sensitive when evaluating the differences between these thoracotomy approaches.

The other difference between the two studies is the methodology used for the exercise test. We utilized an incremental exercise test, which was designed to obtain an anaerobic threshold as well as Vo2max. A stair-climbing test is considered to be constant work under great stress and is not meant for detecting the anaerobic threshold. Although it is simple and useful for clinical use,3 a stair-climbing test has limitations when investigating the mechanism behind the results.

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Urinary Antigen Test for Pneumococcal Pneumonia

To the Editor:

We read with interest the editorial by Dr. Pesola1 (January 2001) and his comments on our study,2 which was recently featured in your publication. With a view to enabling better...
interpretation of the results of our study, we wish to clarify some points that we believe Dr. Pesola has misunderstood.

In our study, we evaluated an immunochromatographic membrane test (ICT) to detect Streptococcus pneumoniae in urine samples, in order to assess its utility in the diagnosis of pneumococcal pneumonia. The sensitivity of the test using urine samples from patients with a diagnosis of definite pneumococcal pneumonia (group 1) was 90.4% (41 of 51). In contrast, the sensitivity of the test using urine samples from patients with a diagnosis of presumptive pneumococcal pneumonia (group 2) was 43.7% (7 of 16). The specificity was 97.2%.

In order to make the performance of the test clearer, we explained in the “Results” section that results could be differentiated in three groups according to the color intensity reached: weak, medium, and very intense. Since the two false-positive results were from patients with a diagnosis of definite pneumococcal pneumonia (group 1) was 80.4% (41 of 51), Table 1 at best or during a research environment (efficacy). This was reduced down to 25% (4 of 16, Table 2) to 74.5% (38 of 51) if it is assumed that some of the weak intensity urinary immunochromatographic test (ICT) results are too difficult to determine if positive and should be thrown out so that interpretation would be unequivocal in the community environment (effectiveness). In the editorial, I suggested the same upper bound with a lower bound ranging from 12.5% (2 of 16, Table 2) to 74.5% based on excluding all weak intensity urinary ICT test results. As noted in their article, Domínguez et al. suggest that there are two types of weak intensity lines on the ICT test compared to control. They suggest in their letter that the weak intensity color lines compared to control are the “less colored lines” and “lesser colored lines.” The latter are more difficult to interpret and can be thrown out to make it easier for users to interpret the results. Therefore, from Table 2 of their article, only three of the weak intensity urinary ICT test results need to be thrown out and not five. This results in a slightly higher range for the lower bound of sensitivities, although it is unclear from Table 2 which weak tests to throw out.

The idea to throw out weak intensity lines on the urinary ICT also came up regarding the calculation of specificity results for group 3 of their data. The initial specificity of 97.2% (69 of 71) was raised to 100% when the two false-positive urinary ICT results were discarded due to a weak intensity color on the sample line that was difficult to interpret. Presumably the latter two were of the lesser colored lines and not less colored lines.

The authors to denoting two different weak intensity lines for the urinary ICT and throwing out one set is there is no proof in their article that doing so is valid. One would have to mix up the two types and instruct blinded judges to separate one type from the other. If the reproducibility and accuracy were there from this type of study, I might buy into the idea. Otherwise, it could be argued that the separation was one of convenience to maximize the end result (both specificity and sensitivity) of the study, and as long as no tests were done to challenge this, everyone would blindly go along. Most likely, the authors are correct and will come out with data to clearly show the difference to the reading audience on the differences between the two weak intensity lines. Until then, it is arbitrary, and I will stay with the current range of sensitivities.

It should be noted that the strength of the findings of Domínguez et al. have nothing to do with the possible less-than-ideal sensitivity of their data. It is the purported specificity of almost 100%. Although the urinary ICT result can be positive secondary to Streptococcus oralis and Streptococcus mitis, these organisms do not cause pneumonia. This translates clinically into suggesting that if the test result is positive in someone with pneumonia, the disease is present. The real question is, “Are their really no false-positive findings such that a diagnosis can be made?

To the Editor:

Domínguez et al. point out in their excellent article (January 2001) on the urinary antigen test for the detection of pneumococcal pneumonia, that the range of sensitivities to be considered for their test should vary from 44% (7 of 16, Table 2) to 80% (41 of 51, Table 1) at best or during a research environment (efficacy). This was reduced down to 25% (4 of 16, Table 2) to 74.5% (38 of 51) if it is assumed that some of the weak intensity urinary immunochromatographic test (ICT) results are too difficult to determine if positive and should be thrown out so that interpretation would be unequivocal in the community environment (effectiveness). In the editorial, I suggested the same upper bound with a lower bound ranging from 12.5% (2 of 16, Table 2) to 74.5% based on excluding all weak intensity urinary ICT test results. As noted in their article, Domínguez et al. suggest that there are two types of weak intensity lines on the ICT test compared to control. They suggest in their letter that the weak intensity color lines compared to control are the “less colored lines” and “lesser colored lines.” The latter are more difficult to interpret and can be thrown out to make it easier for users to interpret the results. Therefore, from Table 2 of their article, only three of the weak intensity urinary ICT test results need to be thrown out and not five. This results in a slightly higher range for the lower bound of sensitivities, although it is unclear from Table 2 which weak tests to throw out.

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every time the test result is positive in the presence of pneu-
monia?° Only time and more data will answer this question.

Domínguez et al 1 also note that their group 2 patients with a
presumptive diagnosis of pneumococcal pneumonia seen in
Table 2 might not all be due to Streptococcus pneumoniae, lead-
ing to the possibility of overrelence on this data to determine
sensitivities that would keep them lower. This may be true as the
phrase presumptive suggests but the diagnostic criteria for the
S pneumoniae diagnosis are not too shabby. All diagnoses in
Table 2 were supported by either a positive sputum culture
finding and/or latex agglutination finding for detecting S pneu-
moniae in the sputum and chest radiography findings consistent
with pneumonia. Of the sputum culture results that were positive
(13 of 16), all except one were supported by at least one other
positive test result consistent with S pneumoniae (excluding
radiography). Two indicators of the same diagnosis significantly
increase the likelihood of being correct. Of the latex agglutination
test results positive for S pneumoniae in the sputum (12 of 14),
only two were not supported by at least one other test result
(excluding radiography) consistent with S pneumoniae. The
false-positive rate for latex agglutination detection of S pneu-
moniae in sputum has been described as about 6%, 4 making it
possible but unlikely that some of the positive results from this
test are not true, particularly if other tests are consistent with
the same diagnosis. In addition, the sensitivity for latex agglutina-
tion detection of S pneumoniae in the sputum of patients with
S pneumoniae has been described as 86%, 4 which is identical to
the sensitivity seen in Table 2 of this study (12 of 14). Therefore,
the diagnosis of S pneumoniae in the group 2 patients in the study
is fairly solid and a tribute to the authors in being wise enough to
attempt to document this pneumonia by a variety of methods.

Finally, we all agree that this study is a significant step forward
in the attempt to make a diagnosis of S pneumoniae in a timely
matter. I congratulate the authors on a very interesting study.

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Pulmonary Artery Stenosis and Fibrous Mediastinitis

To the Editor:

We read with interest the report by Guerrero and colleagues
(March 2001)1 describing treatment of pulmonary artery stenosis
in a patient with fibrous mediastinitis. Unique safety issues are
present and should be addressed when considering intravascular
stents for pulmonary vascular compromise caused by fibrous
mediastinitis, also known as mediastinal fibrosis. Unlike most
other causes that occlude only the pulmonary arteries, such as
pulmonary embolism, mediastinal fibrosis may concomitantly
affect pulmonary veins, airways, or arteries, of one or both lungs,
in any combination. 5 Benefit would not likely ensue from the
restoration of arterial blood flow to a lung that has an obstructed
airway, and could even cause severe shunting. Alternatively,
restoring flow through the pulmonary artery of a lung in which
pulmonary veins are also obstructed could lead to pulmonary
edema. Pulmonary venous occlusion, especially in the presence
of concomitant arterial occlusion, can be difficult to verify and
should be meticulously evaluated by specialized techniques, such
as balloon wedge angiography and measurement of pressure
gradients. Since disorders causing large pulmonary vein occlusion
are uncommon in adults, those individuals with the most expe-
rience using relevant diagnostic approaches are often invasive
pediatric cardiologists who commonly encounter congenital pul-
monary venous disorders.

The authors 1 underestimate the prevalence of mediastinal
fibrosis. They report that only “fifteen other cases of fibrous
mediastinitis with pulmonary artery compression have been
described in the literature.” In one review 2 more than a decade
ago with 71 reported cases of mediastinal fibrosis, pulmonary
vascular involvement was described for 40 patients. Underreport-
ing probably occurs at present because the condition has already
been adequately described.

In the “Discussion” section, the authors state, “patients with
fibrous mediastinitis generally have a benign clinical course until
a mediastinal structure is compressed,” a statement that raises
the issue of the definition of mediastinal fibrosis. Although there
is no universally accepted definition, it surely should not include
all patients who have any fibrosis within the mediastinum. A
reasonable definition also requires the involvement of a major
vessel or airway, 2 so as to distinguish patients with true medias-
tinal fibrosis from patients with inconsequential limited scarring.
Our experience in the last decade with several dozen patients has
demonstrated that those with unilateral autoamputation of one
lung by mediastinal fibrosis usually have a relatively benign
course, even for many decades. 3 Exceptions to this observation
include the few patients in whom serious hemoptysis from
systemic vascular hypertrophy to the obstructed lung develops.

The character of the mediastinal fibrosis lesion is so dense, ie,
typically described by a surgeon as having the consistency of
concrete, it is surprising that balloon dilation and stenting can
successfully restore vascular patency in this condition. For our
first case using pulmonary venous stents for a young man with
bilateral pulmonary venous obstruction from mediastinal fibro-
sis, 4 because we suspected the fibrotic veins were too rigid to
successfully dilate, we were prepared to use a Rotablator tissue
extraction device (Boston Scientific; Natick, MA), but were
pleased to learn that it was not required. Our experience is
similar to that of the authors, with successful stenting of seven
vessels (four pulmonary arteries and three pulmonary veins) in
four patients with mediastinal fibrosis. 4

Further emphasis regarding different causes of mediastinal
fibrosis 5 is also important, as discussed by Sherrick et al in
reference 5 of the authors. 1 The patient in the current report has
mediastinal fibrosis typical for that which occurs as a late
complication of histoplasmosis, because it is focal, lymph node-
based, calcified, and invasive. Despite the authors’ suggestion
that steroids may be helpful here, this condition has never been
shown to respond to steroids or any other medical therapy. In
contrast, another form of mediastinal fibrosis may have features
that resemble retroperitoneal fibrosis and is characterized by

1750

Communications to the Editor

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