Response

To the Editor:

We appreciate the comments of Arias-Santiago and colleagues on our recently published article in CHEST (February 2009).1 We agree that there is confusion regarding various skin conditions that include the term “lupus” in their nomenclature. “Lupus pernio” (a form of cutaneous sarcoidosis), “lupus vulgaris” (a form of cutaneous tuberculosis), “lupus miliaris disseminata faciei” (a form of rosacea), and “chilblain lupus” are but a few examples.

Specifically, the authors expressed concern that chilblain lupus could be confused with lupus pernio. Although this is an obvious problem in terms of nomenclature, we feel that in clinical practice these entities are distinct and are unlikely to be confused for many of the reasons cited by Arias-Santiago and coauthors. Chilblain lupus is usually associated with systemic lupus erythematosus. It is extremely rare, with only 70 cases reported in a 2008 review.2 It involves primarily the toes and fingers; involvement of the ears or nose is rare.2 The lesions generally occur first during cold or damp periods. They are usually pruritic and later painful; such symptoms are extremely unusual with lupus pernio. Pathologically, chilblain lupus reveals vascular thrombosis and not granulomatous inflammation.2,3 Furthermore, although both systemic lupus erythematosus and sarcoidosis are systemic diseases, we disagree with Arias-Santiago and coauthors that they are difficult to distinguish clinically.

The problem with this nomenclature most probably stems from the word “pernio,” which refers to a localized inflammatory lesion of the skin resulting from an abnormal response to cold.4 Lupus pernio skin lesions have no relationship to cold exposure, but it is this unfortunate antiquated description that we believe has led to the confusion. It is probably more appropriate to refer to lupus pernio as “disfiguring facial sarcoidosis.” Nonetheless, we disagree that this confusion in nomenclature may lead to problems in distinguishing these entities clinically.

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The American College of Chest Physicians Evidence-Based Educational Guidelines for Continuing Medical Education Interventions

Estimating Effect Size

To the Editor:

We appreciate the commentary of Norman1 regarding the methodology of our evidence-based review of the effectiveness of continuing medical education (CME), and agree with him that reporting effect size can be extremely useful in reporting results.2 Synthesizing the results of educational interventions represents one of the methodological challenges to performing systematic reviews in health care.3 The studies in our review differed in many important ways (used nonstandardized definitions of CME and targeted multiple types of objectives across vastly different audiences and content areas), and often were flawed in the metrics they used and in how those metrics were reported.4 These limitations in the primary literature led to a qualitative synthesis of the evidence, as an aggregate estimate of effect size could have implied greater confidence in the results than would have been appropriate.

In conclusion, we strongly lend our voice to the importance of estimating effect size in systematic reviews of educational interventions and recommend that original studies of CME give more attention to using valid measures of effectiveness that would allow such estimates.

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References


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REFERENCES

Diagnosing Obstructive Sleep Apnea in the Setting of Comorbid Pulmonary Disease

To the Editor:

In a recent “Postgraduate Education Corner” report in CHEST (December 2008), Benson and Schwarz1 reported on a young woman with recurrent hemoptysis resulting from pauciimmune pulmonary capillaritis exacerbated by negative-pressure pulmonary hemorrhage secondary to presumed coexisting obstructive sleep apnea (OSA). The authors described a detailed evaluation regarding the patient’s pulmonary capillaritis that serves as a worthy example for pulmonologists in training.

In contrast, the approach taken regarding the patient’s tentative diagnosis of OSA was markedly flawed, and it is concerning that this could misinform trainees regarding the appropriate evaluation, diagnosis, and treatment of OSA. Although the authors presumed a high probability of OSA based on the patient’s physical examination, they did not report that they obtained a sleep history, which should be standard in the evaluation of OSA. The diagnostic test used in this patient, who had a waking oxygen saturation of 89% while breathing 8 L/min supplemental oxygen, was overnight oximetry. There is no description provided of the oximetry results. This may be because there has been no validation of this testing and no consensus on how to interpret overnight oximetry for the purpose of evaluating OSA in otherwise healthy individuals.2 In patients such as the one whose case was described, the utility of oximetry is even more problematic, because the false-negative rate increases with the administration of supplemental oxygen.

An evidence-based practice parameter guideline3 issued by the American Academy of Sleep Medicine has strongly recommended against the use of even multichannel unattended monitoring for OSA diagnosis in patients with underlying pulmonary disease. Similarly, recent practice parameter guidelines4 state that, as a standard of care, autotitrating positive airway pressure should not be used to determine continuous positive airway pressure (CPAP) levels in patients with underlying lung disease. In addition, in the case of the patient described by Benson and Schwarz,1 the use of hydrocodone predisposed her to increased risk for central apneas, which would also be a contraindication for autotitration. While the treatment of acutely ill patients who are too sick to undergo formal polysomnography is challenging, and the risks of empiric CPAP use in the short term are relatively low, it is clear that once this patient’s clinical status improved, an appropriate evaluation should have been performed to verify the diagnosis of OSA, given the burden imposed by lifetime CPAP therapy.

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REFERENCES

Pleural Depth in Medical Patients: A Radiological Assessment

To the Editor:

Patient obesity is an increasing phenomenon1 that presents a range of challenges to medical management, particularly invasive procedures. Pleural procedures are common in medical patients. Guidelines exist2 for their safe performance. Central to these techniques is an ability to enter the pleural space with a needle that allows the aspiration of air or fluid to confirm correct localization of the pleural space. Others have shown, in the setting of needle decompression of pneumothorax in trauma patients, that commonly used equipment is frequently of insufficient length to penetrate the chest wall due to the patients’ body habitus.

However, similar data for medical patients are limited. We assessed the chest wall thickness at two potential sites for pleural procedures in a group of predominantly unsolicited medical patients. Fifty-three sequentially performed, contrast-enhanced, thoracic CT scans that were performed at a tertiary referral center...