identified. This may represent a technical problem, as all of those lesions were in the apical segments.

Our article convincingly showed that in this particular circumstance the addition of endobronchial ultrasound to conventional bronchoscopy (not the replacement) can be very helpful and certainly can be recommended. It avoids aborting an otherwise nonpromising bronchoscopy by providing an acceptable yield, does not expose the patient to unnecessary radiation, and is less invasive than primary surgical procedures.

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REFERENCES


Is Intensive Insulin Therapy Safe in the Critically Ill?

To the Editor:

Hamdulay et al described two similar cases of severe reversible cardiac depression, temporally related to exposure to chemotherapy agents for the treatment of lymphoma or prior to haploidentical bone marrow transplantation. Both cases required continuous veno-venous hemofiltration, which had been reported to result in the reversal of septic shock and hemodynamic improvement over time because of the plasma clearance of myocardial depressant cytokines. It could be argued that the hemodynamic recovery witnessed was not related to insulin-glucose infusion but was explained by time-dependent plasma clearance of inflammatory cytokines because of the earlier initiation of continuous renal replacement therapy.

There are several studies that have indicated that indiscriminate intensive insulin therapy to maintain a blood glucose level at < 6.1 mmol/L (110 mg/dL) can result in attributable mortality. A large randomized controlled trial in patients with acute myocardial infarction reported that insulin therapy at a blood glucose level of < 7 mmol/L (126 mg/dL) increased the mortality rate to 8.3% (control mortality rate, 6.6%; p < 0.01). Murcia et al reported that the cumulative risk for total mortality including cardiovascular mortality and morbidity increased with insulin treatment in diabetic patients with acute myocardial infarction and left ventricular failure. In a recent study by Van den Berghe et al, intensive insulin therapy in patients with a short length of stay in the ICU and low severity of illness had a much higher mortality rate (27%) compared to patients receiving conventional insulin therapy (19%) [ie, a relative increase in mortality of 42%; p = 0.045]. The premature and indiscriminate use of intensive insulin therapy in the ICU, which is based on 2004 recommendations without robust scientific evidence, may have resulted in preventable death across the United States. The early resolution of stressors related to the acute illness and minimizing the iatrogenic interventions that exacerbate hyperglycemia rather than prescribing intensive insulin therapy in critically ill patients is the safest method for improved glycemic control and survival.

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REFERENCES


To the Editor:

We thank Dr. Rady on his comments regarding our review on glucose-insulin and potassium infusions in septic shock.1 However, we disagree on his suggestion that the hemodynamic improvement that occurred in our patients could be attributed to the continuous veno-venous hemofiltration (CVVH).

CVVH has been widely used for the treatment of critically ill patients with acute renal failure, and the effects of CVVH on inflammatory responses have been aggressively investigated.2–4 Although circulating inflammatory cytokines were removed by ultrafiltration and adsorption, studies failed to show a decrease in plasma cytokine levels,4–6 even with an aggressive high-volume hemofiltration.5 Having said that, high-volume hemofiltration can significantly improve hemodynamic instability and decrease the vasopressor dose in septic shock patients.6 Nearly all of our patients with sepsis and renal failure are receiving CVVH, yet such dramatic reductions in...
vasopressor support that we described are unique to the patients in whom we used glucose-insulin-potassium infusion (GIK).

Dr. Rady mentioned that driving glucose to lower levels has been associated with adverse effects on mortality, and therefore therapies that intensively use insulin are to be avoided. It is important to differentiate between the use of GIK as an adjunct to vasopressor in hypodynamic septic shock and the much-discussed tight glucose control in intensive care. Our case reports and literature review make the case that high doses of insulin used in combination with glucose loading may yet have a role in improving hemodynamics. Finally, we agree that GIK should not be used indiscriminately, and further studies to establish its utility as an adjunct to the traditional vasopressors in patients with hypodynamic septic shock should be carried out.

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We conducted a prospective cohort study investigating 108 children for the cause of their chronic cough, using an adult-based approach followed by treatment as appropriate and a defined timeframe to response of 2 weeks, given the placebo and period effect of cough. Dr. Rubin has stated that the pathway used was “radically different” from that used by Irwin and colleagues. As we have stated in the article, the pathway has been modified for children as instinctively treating young children will never be the same as treating adults because children cannot tell you they have reflux symptoms as adults can, and, as Dr. Rubin himself states, diagnoses such as cystic fibrosis and tracheomalacia are an “essential part of the evaluation in children.” We felt it more important to ensure a thorough and complete investigation of the causes in children than to stringently adhere to the adult protocol, which was designed and tested some decades ago.

We thank Dr. Rubin for highlighting the important new diagnosis of protracted bacterial bronchitis (PBB) but feel it necessary to point out that he has misquoted the diagnostic criteria, which are, in fact, a history of chronic moist cough, the presence of at least a single species of pathogenic bacterial organism at a growth of $\geq 10^5$ cfu/mL, and the resolution of cough with antibiotic therapy in a 2-week period. PBB was not diagnosed based on the presence of increased neutrophils in BAL fluid and was not diagnosed based on the presence of viral or nonpathogenic bacterial organisms. This is a new diagnostic entity, and much is still to be learned about the clinical features, airway inflammatory profile, and causative factors. We look forward to being able to shed further light on this condition in the near future.

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REFERENCES

To the Editor:

We thank Dr. Rubin for his kind comments about our study (May 2006), which he stated was “one of the most complete studies” of its kind in the pediatric population. For readers unfamiliar with the literature on the common symptoms of cough, it is necessary that some points should be clarified, which are summarized in Table 1.

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2 Rubin BK. Pediatricians are not just small internists. Chest 2006; 129:1118–1121
To the Editor:

I appreciate Dr. Marchant and Chang’s comments on my editorial1 pointing out that although their study2 was an important evaluation of cough in a large number of young children, the protocol that they chose was quite different from that validated in adults as described by Irwin and colleagues.3 They included a number of children with a cough of < 8 weeks duration, and more importantly, they did not evaluate for upper airway cough syndrome, asthma, or gastroesophageal reflux as the major diagnosis noted in adults with chronic cough. These were also noted to be common causes of chronic cough in pediatric studies as well.4 Because they chose a radically different approach to the diagnosis, it is impossible to know whether the children in their study had gastroesophageal reflux, asthma, or upper airways cough syndrome. By choosing bronchoscopy as their principal diagnostic test in these patients, the authors determined that 40% of the children had “prolonged bacterial bronchitis” (PBB), although these children did not have increased airway secretions. This is in contradiction to a report5 that in adults PBB is associated with a large amount of airway secretions. This suggests that the “moist cough” they heard in these children could have been from upper airway secretions in the back of the child’s throat.

These authors are internationally recognized experts regarding the evaluation and treatment of chronic cough in children. I am looking forward to further studies that might better answer the question “Are children with chronic cough really that different from adults?”

Bruce K. Rubin, MEng, MD, MBA, FCCP
Wake Forest University School of Medicine
Winston-Salem, NC

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Table 1—Common Symptoms of Cough*

<table>
<thead>
<tr>
<th>Editorial Comment</th>
<th>Authors’ Clarification of Comment</th>
</tr>
</thead>
<tbody>
<tr>
<td>“The authors suggest that FB may be the diagnostic test of choice in evaluating chronic cough in young children”</td>
<td>We wish to clarify that at no time did we suggest or state this. The use of FB in our protocol was to obtain BAL fluid and assess large airway anatomy, but we stated that “we utilized a protocol not widely available and indeed we are not advocating its use . . .”. The utility and limitations of bronchoscopy have also been discussed in the ACCP guidelines for children.4</td>
</tr>
<tr>
<td>“The term moist cough is subjective and carries no physiologic significance”</td>
<td>A discussion on the physiologic significance is available.4 This term has been validated in children, whereby parents were accurate, and it related to the presence of airway secretions seen during bronchoscopy.5 This term has been used widely in the pediatric literature.</td>
</tr>
<tr>
<td>“Results must be replicated in a prospective study of children with a true (&lt; 8 weeks duration) chronic cough”</td>
<td>Our study commenced in 2002 when the ACCP guidelines stated “chronic cough of &lt; 3 weeks duration.” The new 2006 ACCP clinical practice guidelines in pediatrics define chronic cough as being of &lt; 4 weeks duration.4 The reasons for this were discussed in the ACCP guidelines. Irrespective of this, only 15 of 108 children had cough of between 3 and 8 weeks duration on study enrollment.</td>
</tr>
<tr>
<td>“We strongly urged that this supposition (big three causes of cough in adults also common in children) be validated by well-controlled randomized clinical trials”</td>
<td>We agree and have stated so in our article (“ideally an RCT is necessary for assigning treatment effect”), but it should be noted that adult diagnostic algorithms were not evaluated as RCTs.3 Additionally, we used a timeframe for treatment response of 2 weeks, to decrease the placebo and period effects of cough, in combination with methods used in adult studies. Clearly, there are differences in North American and Australian parental expectations. Furthermore, we feel that investigation followed by appropriate therapy has clearer scientific merit when comparing these approaches. The lack of evidence for the approach suggested by Dr. Rubin is well-documented (see the North American guidelines for GER in children and a Cochrane review on cough and GERD).</td>
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<tr>
<td>“In North America, where a 1-month course of reflux therapy is thought to be preferable to conducting a pH probe study”</td>
<td>We agree that a placebo-controlled RCT is needed to prove the effectiveness of antibiotic therapy in PBB. Our definition of PBB is based on factors other than just the response to antibiotics as described. Follow-up bronchoscopy would be scientifically valuable but is unethical.</td>
</tr>
<tr>
<td>“The ‘response’ to a 2-week course of oral antibiotics should not be considered evidence of PBB without either a follow-up bronchoscopy or a placebo-controlled arm”</td>
<td>We recruited 95% of the children who presented to our tertiary practice, and thus the study represents the population who presented with chronic cough in Australia, the majority of whom are of preschool age. Should this be different in North America, we look forward to the results of studies there that assess chronic cough in an older pediatric population.</td>
</tr>
<tr>
<td>“Because most of subjects . . . were &lt; 3 yr old, these results probably cannot be generalized to the broader pediatric population.”</td>
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*FB = fiberoptic bronchoscopy; ACCP = American College of Chest Physicians; RCT = randomized controlled trial; GER = gastroesophageal reflux; GERD = gastroesophageal reflux disease.
Evaluation of the Causes of Racial Disparity in Surgical Treatment of Early-Stage Lung Cancer

To the Editor:

The idea reported in the article by McCann et al (November 2005)1 that black patients decline surgical treatment for stage 1 and 2 cancers more frequently than their white counterparts requires careful scrutiny. This study should be assessed as an exploration of the ability of physicians to communicate their therapeutic objectives to black patients. Consequently, dynamic variables such as clarity of message, body language, and emphasis could not be captured by retrospective case record analysis. Furthermore, the authors neglected to evaluate the role of physician factors in their observations. This omission is surprising considering that many investigators have shown that physicians asymmetrically employ established standards when caring for black patients.2 McCann et al3 noted that all black patients who were offered surgery by black physicians accepted the procedure. However, this observation was not pursued further. Intriguingly, the authors also noted that elderly black patients declined surgery at an even greater frequency. It would have been interesting if the authors had pursued the insight that we should focus efforts on physician-patient communication as a way to improve surgical rates. 

He points out that a more complete evaluation of the physician-patient interactions would allow for a better understanding of why black patients declined surgical interventions for lung cancer. I agree with him. In the “Discussion” section of our article, we discussed the fact that prior research2 has shown that black patients seeing white physicians rated their physician’s decision-making style as less participatory. Since the publication of our article, research4 has shown that black patients with lung cancer have less trust in their physicians after the visit despite equivalent trust before the visit. As Dr. Dube points out, a better understanding of the dynamics of physician-patient communication and how they effect the development of trust will be a key factor in improving surgical rates.

Dr. Dube would have us further scrutinize physician demographic data to assess how it impacted decision making. As pointed out in the article,1 we only had three black patients offered surgery by black physicians. All three accepted. Given the small numbers, I am not sure how we could have pursued this further. He also wonders why we did not further evaluate the role of physician age, gender, or ethnicity of the advising physician. This line of thought would be in concordance with the notion that physician-patient differences are a causal factor in diverse cases of disparities in care.

The dynamic complexity of the sociocultural universe of modern metropolitan ethnic populations requires more complex communication skills than are required for situations in which the physician and patient are socioculturally more congruent. Uncovering the etiologies of racial disparities calls for innovative research in communication, including visual and audio record analysis as well as physician interviews, to explore the clinical logic behind discrepant care. These approaches would lead to practical solutions toward an important health-care delivery problem.

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I thank Dr. Dube for his interesting comments about our article.1 He points out that a more complete evaluation of the physician-patient interactions would allow for a better understanding of why black patients declined surgical interventions for lung cancer. I agree with him. In the “Discussion” section of our article, we discussed the fact that prior research2 has shown that black patients seeing white physicians rated their physician’s decision-making style as less participatory. Since the publication of our article, research3 has shown that black patients with lung cancer have less trust in their physicians after the visit despite equivalent trust before the visit. As Dr. Dube points out, a better understanding of the dynamics of physician-patient communication and how they effect the development of trust will be a key factor in improving surgical rates.

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