a number of reasons. It is unlikely that any of the trials included in the review would employ therapy with the combination of LABAs and corticosteroids (CS). However, even if they had, one would imagine the distribution would be similar across the groups in a randomized controlled trial, and their effect would be of limited concern. The trials in the review are older, and these drugs are infrequently used, even today. Furthermore, Dr. Jalba’s claim of benefit from LABAs only relates to the treatment of patients with chronic asthma, since there is no evidence that adding LABAs to therapy for patients with acute asthma is beneficial.

When trials using similar designs (ie, randomized controlled trials), comparisons (ie, inhaled CS vs CS), populations (ie, acute asthma), and outcomes (ie, relapse) exist, pooling is justified. In our review,1 where there was no significant clinical or statistical heterogeneity, pooling was clearly appropriate. If the results of pooling demonstrate heterogeneity, then limited exploration is warranted, and pooling may not be appropriate. Conversely, searching for subgroup differences has been shown to generate erroneous results and should be considered far more carefully than Dr. Jalba would suggest.3 Precisely how would Dr. Jalba suggest that the current search failed in studies such as this. I offer a quantitative analysis to show that their conclusion is not just “partly invalidated” by suboptimal compliance but, rather, is completely invalidated. It is critical to distinguish efficacy (ie, the effect in the laboratory under ideal conditions) from effectiveness (ie, the effect in everyday life)5 when comparing surgical and nonsurgical treatments for sleep apnea. Effectiveness is far more relevant than efficacy to clinical practice. A perfect laboratory value with the device on (ie, efficacy measure) is of no help to the patient who does not wear the device outside the laboratory. Treatment adherence is not an issue with surgical therapy. Thus, it makes sense to compare only the effectiveness of surgical and nonsurgical treatments. It is a fallacy to compare the efficacy. Rather than exclude subjects who failed to receive or use their randomly assigned treatment, the authors should have counted those subjects as treatment failures in an assessment of effectiveness, as is required in a true intention-to-treat analysis.3

Including these treatment failures reduces the 4-year apnea index success rate of the dental appliance from 81% (32 patients) to 54% (48 patients), and that of the UPPP from 53% (40 patients) to 49% (43 patients). The difference between treatment groups is no longer statistically significant (p = 0.68 [Fisher exact test]). Likewise, both the 4-year apnea-hypopnea index success rate and the normalization rate are no longer statistically different (p = 0.20 and p = 0.28, respectively). The results are similar even if we exclude the subjects who dropped out due to unrelated medical problems (p = 0.67, p = 0.13, and p = 0.27, respectively). Furthermore, the sleep study values obtained while patients wore the dental appliance in the laboratory should be corrected for the actual usage in everyday life in order to measure treatment effectiveness.6

The fact that patient satisfaction (which is an inherent measure of effectiveness rather than a measure of efficacy) with UPPP was as high as with the dental appliance (satisfied subjects: UPPP, 30 of 46 subjects; dental appliance, 27 of 46 subjects)5 suggests that these treatments have similar effectiveness. In fact, the authors have reported previously3 that quality-of-life contentment (another inherent measure of effectiveness) improved significantly more for patients in the UPPP group than for those in the appliance group.

The suboptimal effectiveness of each individual treatment for

### Sleep Apnea Devices and Sleep Apnea Surgery Should Be Compared on Effectiveness, Not Efficacy

To the Editor:

Walker-Engström et al stated in their article in CHEST (March 2002)1 that treatment with a dental appliance “showed a significantly higher success and normalization rate” and thus had “superior effectiveness” than uvulopalatopharyngoplasty (UPPP) for the treatment of obstructive sleep apnea. Although this study had many strengths, such as randomization, long follow-up, and blinded sleep study interpretation, the conclusion depended on a key methodological fallacy that is common in studies such as this. I offer a quantitative analysis to show that their conclusion is not just “partly invalidated” by suboptimal compliance but, rather, is completely invalidated.

### REFERENCES


sleep apnea (including continuous positive airway pressure) suggests the need for complementary and collaborative treatment regimens.

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To the Editor:

We thank Dr. Weaver for his comments on our article that was published in CHEST (March 2002).1

No treatment is effective if patients are not compliant. This is an inherent problem when comparing the effects of nonsurgical treatments to those of surgical treatments. Patients can stop taking drugs or, in this case, can stop using a dental appliance. Once operated on, however, they cannot withdraw from treatment. Even in the best-designed, best-managed, short-term clinical trials, there will be patients who withdraw.2

In a clinical trial in which treatment extends over an unusually long period (4 years in our study), the risk of withdrawals from treatment, as expected, increases quite a lot, even though the results of a clinical trial often differ from the those of clinical practice because of the careful monitoring of patients in the trial. Therefore, it might be more appropriate to use the term efficacy instead of effectiveness in clinical trials. The careful description of the number of withdrawals and the exact reasons for them are important issues in interpreting the results in a clinical trial. Such an analysis was performed in our study. That compliance is an important response variable is recognized; they must be measured.

To the Editor:

The study by Shorr and colleagues (July 2002)1 was a thought-provoking analysis of patients awaiting orthotopic lung transplantation (OLT) for sarcoidosis in the United States. The authors noted that patients with sarcoidosis appeared to be less likely to undergo OLT than patients with idiopathic pulmonary fibrosis. The fact that listed patients with sarcoidosis were more likely to be black and female raises concerns about race or gender bias in organ allocation, but there is another more plausible explanation for their observation. Lung donors are more likely to be men,2 and thus there is potentially a “size bias,” which also adversely affects patients with cystic fibrosis awaiting OLT. We routinely down-size donor lungs into CF recipients of small stature to increase their opportunity for undergoing transplantation.3

The recommendation of Shorr et al that patients with sarcoidosis be awarded a “waiting time credit,” as is done for patients with idiopathic pulmonary fibrosis, fails to address the fundamental issue of who should be offered a lung; the patient who is able to wait the longest or the patient who is most at risk of death on the list? In this respect, the current United Network for Organ Sharing lung distribution algorithm is fundamentally flawed.

The Lung Allocation Subcommittee of the United Network for Organ Sharing Thoracic Organ Committee is developing a distribution algorithm that considers the risk of dying on the waiting list coupled with the chance of posttransplant survival rather than using waiting time as the principal determinant of lung allocation.4,5 Patients with sarcoidosis and other less common lung diseases have posed some challenges in our analyses because of the relatively small numbers compared to patients with other diagnoses. Nevertheless, we found that

Orthotopic Lung Transplant for Sarcoidosis

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