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

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References

2 Last JM. A dictionary of epidemiology. New York, NY: Oxford University Press, 1995; 52

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 well-documented in our article and is also included in the conclusion section of the article.

Two of the response variables presented in our article are success rate and normalization rate. In his reappraisal of our article, Dr. Weaver assumed that all the patients who withdrew from treatment had nonnormal somnographic values after 4 years, and thus, he in turn placed them in the unsuccessful and nonnormalized group. Thus, Dr. Weaver introduced an extreme assumption into his calculations. For several reasons, which are carefully described in the article, none of the patients who withdrew from treatment was examined after 4 years with somnography. These patients could have attained normalization using thesomnography measurement if they had attended the 4-year follow-up. The results of an examination for an individual patient cannot be predicted; they must be measured.

We think our study has a sound basis for the conclusion that the use of a dental appliance with regular follow-ups can be recommended for long-term treatment.

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References


Orthotopic Lung Transplant for Sarcoidosis

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...
sarcoidosis patients with pulmonary hypertension had a higher risk of death on the wait list than those with normal pulmonary artery pressures. Sarcoidosis patients with normal pulmonary artery pressures have an unpredictable natural history, which is a vexing problem for lung transplant surgeons trying to decide whether to perform a transplant in someone who is stable or to use the organ for someone who is more ill.

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REFERENCES


To the Editor:

We appreciate the comments of Dr. Egan regarding our study1 of outcomes for patients with sarcoidosis awaiting lung transplantation. He raises two issues with our analysis.

First, he suggests that size bias might explain some of the differences we noted in waiting times. Because more lung donors are male but most sarcoidosis patients listed for lung transplant are female, there may be too few organs that would be appropriate for smaller, female patients. This certainly is a possibility. Thus, we have subsequently conducted a multivariate analysis to control for this potential source of confounding. In so doing, we find that gender does not correlate with probability of undergoing lung transplantation in patients with either sarcoidosis or idiopathic pulmonary fibrosis. Nonetheless, race remains a significant predictor of transplantation. These results underscore our concerns about the potential for racial bias in the allocation of lungs for transplantation and make size bias a less likely explanation for our observations.

Second, we only meant our data to suggest that perhaps the waiting time credit for patients with idiopathic pulmonary fibrosis be re-evaluated. The issue of the most appropriate allocation system for scarce organs is a complicated, multifaceted question, with significant social and ethical implications. Rather, we intended for our findings to spur discussion over this topic in the transplant community. Any algorithm for the distribution of lungs for transplantation cannot be based purely on numerical probabilities of benefit—such models cannot capture many factors, particularly when the disease studied is rare. Similarly, scoring systems that include severity of illness to the exclusion of prior waiting time may be open to gaming and abuse, as noted with the older allocation system for liver transplants.

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The opinions expressed herein are not to be construed as official or as reflecting the policies of either the Department of the Army or the Department of Defense.

REFERENCES


Lung Cancer Screening

Contumacy vs Mendacity

To the Editor:

Integrity is the currency of scientific discourse; mendacity and attempts to discredit findings by impeaching their author are its counterparts. Contumacy (obstinate resistance to authority) is irrelevant in this context, for, since 1632, when Galileo was incarcerated by the Inquisition for publishing his Dialogo sopra i due massimi sistemi del mondo, tolemaico e copernicano, appeals to authority to resolve scientific disputes have been held in small regard.

The author’s editorial1 intentionally misrepresents the content of my article2 in order to repudiate it, stating that I “postulated” that 33% of lung cancers were overdiagnosed, that 33% were nonaggressive, and that death occurs when the FEV1 falls to 1 L. I preceded figures provided for overdiagnosis (cancers that prove nonlethal due to either tissue overinterpretation or death from a comorbidity), clinical behavior, and lethal FEV1 values in my model with the statement: “Consider the following hypothetical scenario.” My intent was to provide a conceptual model that might resolve the seeming paradox of higher mortality despite increased survival in screenees. I posited that it could be accounted for if the increased long-term mortality consequent to loss of pulmonary reserve in persons with overdiagnosed lung cancers were not offset by a reduction in mortality afforded screenees with nonaggressive (those that might be cured if identified early) lung cancers. The author’s misrepresentation is not attributable to a misreading or oversight. He raised this issue when he reviewed the article, and I furnished (unnecessary, since the article was unambiguous) clarification. In an ad hominem argument, he further implies that, because of my prior association with the Kaiser Permanente system, my conclusions were motivated by an effort to spare the health plan the expense of membership screening. I am not currently associated with that organization, I was not influenced by the association, and I received no monetary or other consideration for authoring the

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