

## Response to Drs. Woodson and Steward

Barbara A. Phillips, M.D.

*Pulmonary Division, University of Kentucky Medical Center, Lexington, KY*

*J Clin Sleep Med 2005;1(4):437*

To the editor:

I thank Drs Woodson and Steward for their letter addressing my “con” editorial about surgical treatment of sleep apnea.<sup>1</sup> In this paper, I commented on 2 of their studies without including the references, and I apologize for that. They have both made substantial contributions to the literature about surgical treatment of sleep apnea, and failure to include the citations was an omission that I regret.

Drs Woodson and Steward have several concerns about my interpretation of their reports.<sup>2,3</sup> They take issue with my statement that their 2003 report about temperature-controlled radiofrequency tissue ablation<sup>2</sup> failed to include “the actual data about sleep architecture, AHI, or oxygen saturation.” Baseline characteristics of the patients, including AHI and lowest SaO<sub>2</sub>, are indeed reported (Table 1, page 852), although information about sleep quality or structure is not. However, the table of treatment effects (Table 5, page 855) does not include raw data, but rather something called “Change,” which is the posttreatment mean value minus the baseline mean value for the group for a variety of measures. This is not raw data, and it is not complete data. The reader who wants to make a comparison of AHI’s pre and post treatment must compare Tables 1 and 5, and also must take into account that there are different numbers of patients in most columns compared with the baseline data reported in Table 1. For example, with regard to the AHI, baseline in Table 1 is 21.3+ 11 events/hour for 29 patients; the change reported in Table 5 for TCRFA is -4.5 +13.8 for 24 patients, and no data is given for CPAP. I guess this means that 24 of the 29 patients who had TCRFA had a mean fall in AHI from 21.3 to 16.8 events/hour. No data about sleep quality or structure are given in this (or any) table. And we are left wondering what happened to 17% of their initial surgical group. They did not, as I incorrectly stated, record esophageal pressure.

I did misinterpret the reaction time data presented in Tables 1 and 3 of the extended follow-up study<sup>3</sup> of this population. There are 3 different reaction time outcomes reported in Tables 1 and 3: slowest, median and fastest. Slowest reaction time is reported as a

reciprocal, while median and fastest are not. The rationale for the different treatments of these raw data is, in fact, given in the paper referenced by the authors. There were no important changes in any of the reaction times after surgery. I was wrong, and I apologize for this.

Finally, they take issue with my omission of the fall in Epworth Sleepiness Scale (ESS) by a mean of 3.4 points after radiofrequency treatment. As I stated in my editorial, the ESS is quite subjective, and correlates poorly with objectively measured sleepiness.<sup>4</sup> In post-surgical patients, the ESS does not correlate with Respiratory Disturbance Index (RDI) or oxygen desaturation.<sup>5</sup> Further, many patients with significant sleep-disordered breathing have normal scores on this test.<sup>6</sup>

In my editorial, I noted that Woodson, Steward et al, “deserve credit for undertaking an extremely uncommon task in this study; inclusion of a placebo (sham surgery) group.” In fact, they deserve credit in general. Their work is difficult, and they continue to approach it with diligence. I thank them for their response, for their understanding that this was, indeed, a “debate” (thus subject to skewed interpretation), and for their willingness to continue to collaborate to improve outcomes for our patients.

### REFERENCES

1. Phillips B. Upper airway surgery does not have a major role in the treatment of sleep apnea. *J Clin Sleep Med.* 2005;1:241-5.
2. Woodson BT, Stewart DL, Weaver EM, Javaheri S. A randomized trial of temperature controlled radiofrequency, CPAP, and placebo for the treatment of mild obstructive sleep apnea syndrome. *Otolaryngol Head Neck Surg.* 2003;128:848-61.
3. Steward DL, Weaver EM, Woodson, BT. Multilevel temperature controlled radiofrequency for obstructive sleep apnea: extended follow-up. *Otolaryngol Head Neck Surg.* 2005;132:630-35.
4. Sangal RB, Mitler MM, Sangal JM. Subjective sleepiness ratings (Epworth Sleepiness Scale) do not reflect the same parameter of sleepiness as objective sleepiness (maintenance of wakefulness test) in patients with narcolepsy. *Clin Neurophysiol.* 1999;110:2131-5.
5. Giudici S, Farmer W, Dollinger A, Andrada T, Torrington K, Rajagopal K. Lack of predictive value of the Epworth Sleepiness Scale in patients after uvulopalatopharyngoplasty. *Ann Otol Rhinol Laryngol.* 2000;109:646-9.
6. Walsleben JA, Kapur VK, Newman AB. Sleep and reported daytime sleepiness in normal subjects: the Sleep Heart Health Study. *Sleep.* 2004;27:293-8.

### Disclosure Statement

Dr. Phillips has indicated no financial conflict of interest.

Address correspondence to: Barbara A. Phillips, M.D., MN 614, Pulmonary Division UKMC, 800 Rose Street, Lexington, KY 40536; Tel: (859) 226-7006; E-mail: bphil95@aol.com