REFERENCES

2 Gonzales-Rothi RJ, Foresman GE, Block AJ. Do patients with sleep apnea die in their sleep? Chest 1988; 94:531-36

To the Editor:

We very much appreciate Dr. Thorpy's comments on the article and editorial which were recently published in Chest on mortality and sleep apnea. Some of his concerns regarding potential short-comings in each of the studies are well taken, but we feel that some issues and criticisms raised by the letter deserve clarification and response.

First of all we agree—as he stated—that a critical methodologic requirement for any survival study is that the population under investigation be clearly defined, which we extensively did in our paper under the subheadings "Type of Study, Patient Population, Patient Categories" in the Materials and Methods section and later under "Vital Statistics" in the Results section.

We also wholeheartedly agree that for a population study to be methodologically appropriate, patients should be seen consecutively. The patients in our study were all seen consecutively. While we would also agree that it is generally desirable to follow a group of patients in any population study for as long as possible, we obviously could not "choose" how long our patients were followed; this was, afterall, a retrospective study. It should be noted that the follow-up time of five to ten years that you suggest as "appropriate" is perhaps somewhat arbitrary. As you know, the "appropriate" length of follow-up of mortality from any potentially lethal condition can be based on a "cause-effect" hypothesis of what is likely to cause death from that condition. There are several well done population studies, for instance, that look at one year mortality from sudden death after myocardial infarction. Is one to say that a one year follow-up is not "appropriate" for such population studies? For that matter, does anyone know how long it takes to die from sleep apnea syndrome once it is diagnosed? If one hypothesizes that apnea patients might die in their sleep as a result of hypoxemia, arrhythmias, and episodic pulmonary hypertension (as has been implied by many authors on this subject), it is not altogether outlandish to suspect that increased mortality might be seen with shorter durations of follow-up.

Dr. Thorpy's statement that we do not provide mean follow-up time for the patients in our paper is unwarranted. We refer to Table 1 on page 533, where mean and range of follow-up for each group of patients are quite clearly stated. This information is also included verbatim in the body of the text in the Results section. Along the same lines, he also comments that "the mention of five and eight years of follow-up by the Gainesville group is misleading." We could not recall having ever made such a statement, and even after thoroughly re-reading our paper, we frankly could not find specific mention of "five or eight years of follow-up" anywhere. We did quite outrightly state (both in the abstract and in the first sentence of the Methods section) that our study spanned an eight year period between July 1978 and June 1986, and it is possible that these statements could have been either misread or, alternatively, misinterpreted.

Dr. Thorpy expressed concern that by having excluded 14 patients from our study because their records were unsuitable, we might have biased the findings in our study. We did not include the records of those patients in our study for various reasons: 1) many did not meet criteria of symptoms clinically suspect of sleep apnea; 2) in some patients a sleep study had been insisted upon by the patient or by other physicians (not necessarily from our group) to evaluate abnormal breathing "spells" or undue sleepiness in patients which turned out not to have sleep apnea (ie, narcolepsy, seizures, pseudoseizures, "choking spells", Cheyne-Stokes respirations, mental retardation, etc); 3) some were pediatric patients; and 4) some were coded by ICDMA diagnoses under "Pickwickian syndrome" but never had confirmatory sleep studies. For these reasons we felt justified in not including these records as suitable for review for our study.

We feel confident that in our study we found no statistically significant differences in mortality between the group of apnea patients and control subjects. The mean ages of the two groups to begin with were not statistically different, and in this sense it could be said that the two groups being compared were similar. The fact that the mean age of patients who subsequently died from each respective group might have differed should not be taken to mean that the original groups being compared were therefore dissimilar, or that the comparisons were therefore invalid or misleading. Predictably, in observational population studies, death is a variable beyond the control of the investigator, as is the age of dying. The differences in age of dying between control subjects and apnea patients is an interesting point that Dr. Thorpy raises and one which merits pursuit in future studies with larger numbers of patients.

Dr. Thorpy also commented that we failed to discuss "the interesting fact" that the "treated" apnea patients had a higher mortality than the untreated group of apnea patients in our study. We did not feel this point merited discussion as a 10 percent ("treated") apnea vs 9 percent (untreated) mortality could hardly be interpreted as a significant difference in death rate between two groups.

Dr. Thorpy also stated that "the clinician must not be discouraged from aggressive therapeutic intervention" as the Gainesville group implied. We quote directly from the conclusion in our article: "Whereas patients with sleep apnea syndrome may exhibit disturbingly severe arterial desaturation and associated dysrhythmias, the clinical impression extracted from this series would suggest that nocturnal sudden death is not as imminent in these patients as our clinical intuition might have previously led us to believe. Therefore, the urgency, timing, and indications for aggressive therapeutic interventions in sleep apnea syndrome, particularly those calling for surgically emergent approaches need to be revisited." We would submit that this statement is clear, that it is well-supported by our data, and that when not taken either out of context or misinterpreted is not likely to be either misleading or discouraging to the treating clinician.

In conclusion, we should perhaps make clear that our personal clinical intuition and bias has always been that patients with sleep apnea syndrome are at increased risk for dying. The intent of our study, however, was to begin to probe that hypothesis with facts and not with suppositions or self-fulfilling prophesies. Our study has shortcomings which we readily acknowledged in both the article and in our subsequent editorial published in Chest. We make no apologies for our findings or conclusions. We contend that both our study and the study by He and co-workers are valid in that they have raised important questions which merit further definition. That these studies are not without shortcomings should neither invalidate them nor open them to undeserved criticisms. In fact, at the risk of "muddying the waters", we are encouraged by the
interest and emotional response they seem to be generating. It is our hope that colleagues in the field of sleep research will be motivated by the challenge to generate a larger, more inclusive data base from which the hypothesis on the natural history of this disorder can be securely tested and validated.

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Vigorous Cleaning of Inspirease Reservoir Devices

To the Editor:

The Inspirease Reservoir Device (Figure 1a), developed to maximize benefit from the metered dose inhaler, has become a very popular and useful adjunct for aerosol therapy.1 The design of this reservoir device 1) does not require coordination on the patient’s part between firing the metered dose inhaler and inhaling, 2) offers the patient visual assurance that he is inhaling the aerosol as the bag collapses, 3) does not allow aerosol leakage to occur during rebreathing since the system has no opening to the outside, and 4) provides an audio cue when individuals inhale at inspiratory flow rates too fast for maximal deposition of aerosol in the lower respiratory tract. The plastic mouthpiece (Figure 1b) is equipped with two delicate horizontal plastic reeds that provide the audio cue.

We would like to bring to the attention of the readers of Chest potential problems associated with the use of this device. During a one-month period of time, five patients came into our outpatient Pulmonary Clinic complaining that their Inspirease devices no longer provided an audio cue for rapid inhalation. Upon examining these devices, we noted the plastic reeds were either bent and/or broken (Fig 1c). Several of the reeds were completely missing. One of the reeds fell off in clinic after being barely touched with the point of a pencil. The patients who used these devices all related a history of vigorous, aggressive cleaning and drying of their devices with hot, strong running water or paper towels. While none of our patients, to our knowledge, sustained any adverse occurrence from the missing or broken reeds, the breakage of the reeds in the mouthpiece could place other patients at risk for two potential problems. First, the thin piece of plastic that breaks off could be inhaled; second, patients may be unaware that the audio cue is no longer functioning and may be improperly using the device.

Since the Inspirease Reservoir Device is an important adjunct in the treatment of patients requiring inhaled bronchodilator therapy, it is important to impress upon patients the importance of gently cleaning and air drying the device and to remind patients to visually inspect their devices for breakage on a regular basis.

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REFERENCES

Severe Reactive Airways Disease Induced by Propafenone

To the Editor:

Since the advent of propafenone as an antiarrhythmic drug, severe reactive airways disease has been a complication sporadically recognized as a side effect.1 After recent experience in a patient with severe spastic airways reaction in acute intoxication with propafenone, we read with great interest the article of Hill et al1 evaluating the asthmogenicity of propafenone.

The effect of propafenone may be attributed to a structural and functional resemblance to propranolol, a beta-adrenergic receptor antagonist.1 In acute intoxication, bronchodilator agents can be used to prevent airways reactivity. In subjects with mild intermittent asthma or chronic obstruction of airways, this drug should be used with caution at doses always less than to 450 mg/day.

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