monitoring; and (3) pH recordings have some considerable limitations with poor reproducibility data. However, from our findings we cannot rule out that asymptomatic GER was more prevalent in patients with sleep apnea compared to patients who only snore. Only 5 of 15 patients with sleep apnea and GER, which were documented by abnormal findings of pH monitoring studies, had symptomatic GER in the study by Penzel and colleagues. It also might be considered that patients with a respiratory condition have a different perception of sensing reflux events compared to healthy subjects. If this is the case, do patients with sleep apnea and asymptomatic GER require separate GER treatment? Furthermore, if therapy with continuous positive airway pressure improves nocturnal GER in the absence of sleep apnea, is GER then necessarily related to apneas? Since these questions are as yet unanswered, new studies are necessary in order to investigate the link between sleep-disordered breathing and GER more thoroughly.

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**Success Rates for Nortriptyline**

To the Editor:

We would like to congratulate da Costa et al (August 2002) on extending the database on nortriptyline for tobacco dependence. However, we would like to comment on several issues regarding their study. First, we would caution against comparing results across studies. Comparing efficacy for given treatments is best done using direct comparative data. In the absence of such data, results from similarly designed studies may be compared as long as limitations of the comparison are noted.

In the present study, abstinence rates appear to be based on patient self-report without biological confirmation, which is the standard for determining efficacy in smoking cessation studies. Additionally, efficacy is reported as the 1-week quit rate at the end of treatment. No data on quit rates at other time points during treatment or on continuous quit rates are presented. The limited quit data reported and the lack of biological confirmation of quitting do not allow for an informed comparison with bupropion studies. In addition, this study employed relatively intensive group therapy administered by psychiatrists. Such therapy would be expected to elevate quit rates as opposed to quit rates in studies that used a less intensive behavioral intervention.

Regarding a separate issue, and in contrast to a statement by the authors, the cardiovascular profile for bupropion is well-established. Bupropion therapy has been evaluated in multiple depression and smoking cessation studies, and has been shown to be associated with minimal cardiovascular risk. In a study of bupropion therapy in smokers with cardiovascular illnesses, the safety profile was similar to that seen with bupropion therapy in a general smoking population. In the present study of nortriptyline therapy, 16% of smokers accepted into the study were excluded due to ECG alterations, which further raises questions about comparability.

In conclusion, it is inappropriate to conclude that success rates obtained with nortriptyline therapy in this study are comparable to those established with bupropion. In fact, when available evidence of efficacy and safety were reviewed by Agency for Health Care Policy and Research, only bupropion and nicotine replacement therapy were recommended as first-line treatments.

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To the Editor:

We would like to thank Drs. Jiménez-Ruiz and Orive for their interesting letter and detailed comments. We agree that direct comparison between the results of different studies should be done cautiously. Our comments in the discussion suggest that the results obtained in our trial are similar to those achieved with bupropion administration in other published series. As to abstinence rates in our study, they were reported as 1 week after treatment, as well as 3-month and 6-month success rates following treatment.

In a meta-analysis of published studies comparing self-reported smoking status with results of biochemical validation, the authors observed high level of sensitivity and specificity for self-reported abstinence. The authors observed that despite their believed objectivity, biochemical measures could not be considered the “gold standard,” nor were they perfect measurement of accuracy. Carbon monoxide and thiocyanate can be elevated even in individuals who do not use tobacco. Biochemical tests also have limited ability to detect very low levels of smoking that would be expected from recent quitters. Only cotinine-plasma may be the biochemical test of choice if adequate resources were available for collection and analyses; however, our study compared success rates of abstinence in placebo and nortriptyline groups, in order to control confusion factors, and showed that nortriptyline significantly increases the smoking cessation rate.

There is no objective reason to expect differences in abstinence rates due to the type of behavioral group therapy. It is rather difficult to compare behavioral approaches as more or less intensive.

About the Agency for Health Care Policy and Research review and guidelines, published in June 2000 and including data from studies found in the medical literature before that particular date, the authors considered nortriptyline as the drug of choice for second-line treatment of tobacco addiction. That decision was not based on its lack of effectiveness. As the authors stated, nortriptyline was “identified as efficacious and may be considered by clinicians.” We think that data from our study, as well as others recently published could add to the evidence of the efficacy of nortriptyline in the present context.

Drs. Jiménez-Ruiz and Orive stated that “in contrast to a statement by the authors, the cardiovascular profile of bupropion is well established.” We included in our discussion the following citation: “there have been no clinical trials establishing the safety of bupropion in patients with cardiovascular disease,” quoted from the same reference cited in their letter. In this study with 36 depressed patients, 5 patients could not complete treatment because of the adverse effects of bupropion. The authors concluded that studies with more subjects are needed to confirm that the cardiovascular profile of bupropion may make this drug a useful agent in patients with preexisting cardiovascular disease. The other, more recent reference cited by Drs. Jiménez-Ruiz and Orive was presented in a medical meeting in September 2002, after our article was published. We agree with their statement that bupropion cardiovascular adverse effects are not considered a major restriction to its use, as further discussed in our article.4

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Family-Witnessed Resuscitation

To the Editor:

In their recent article in CHEST, McClenathan et al (December 2002) did a superb job of highlighting the issues surrounding family-witnessed resuscitation (FWR) from an adult medicine perspective. Although the American Heart Association recently recommended that family presence be strongly encouraged during resuscitative efforts, their survey clearly demonstrated that this practice is not universally embraced. Nevertheless, the practice of FWR is viewed favorably in the field of pediatrics.

The debate sparked by the parentally witnessed resuscitation event that was mentioned in the above article initially inspired the pediatricians involved to survey our own colleagues’ opinions of the practice. We found that pediatricians are more likely to be accepting of and to repeat FWR than their adult medicine counterparts.2 Roughly one third of the pediatricians surveyed would allow a family presence during cardiopulmonary resuscitation, and almost two thirds would repeat the practice. Pediatric inpatient-oriented specialists were far more accepting of family presence (57%) and were willing to repeat it (74%) than their adult counterparts in pulmonology (20%) and critical care medicine (40%).

The realities of pediatric training and practice make us more accepting of family presence. Early and repeated exposures to delivery room resuscitations help to prepare pediatricians for FWR. Deliveries, by their very nature, require at least one parent to be present, with the father usually toting a video camera. Additionally, we constantly interact with parents in their literal role as guardian. Parents are the decision makers, and they watch over their children even during calamity.

The comfort that pediatricians have with a parental presence may explain why family-centered care is embraced in pediatrics. In fact, the American Academy of Pediatrics and the American