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 belief in objectivity, biochemical measures could not be considered the “gold standard,” nor were they perfectly measured in 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.

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REFERENCES


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. 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
College of Emergency Physicians announced that they are jointly working on documents regarding the application of family-centered care as it pertains to the death of a child in the emergency department. A recent review of peer-reviewed manuscripts shows a clear trend in favor of FWR, especially by families, followed by nurses and experienced physicians. This review pointed out the limitations in the published literature. Many FWR articles did not test a hypothesis, and broad conclusions were made from weak data. Clearly, more work is needed before FWR can be advocated wholesale.

In the mean time, increased discussion and study may enhance the opportunity for and acceptance of controlled FWR in many settings. When done correctly with appropriate staff liaisons, the practice of FWR can be very rewarding. Many parents have thanked my colleagues and me for allowing them to be present during their child’s precious last moments. They were present at the beginning of the child’s life. They should be allowed to be present at the end!

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To the Editor:

We thank Dr. Creamer for his interest in our article, and we acknowledge his expertise in pediatric intensive care and family-witnessed resuscitation (FWR) for pediatric patients. We agree with Dr. Creamer that pediatric intensive care, by its very nature, requires a more family centered approach, in which there is continuous interaction between health professionals and parents functioning as guardians and decision makers.

In our original article, we stated that the results of opinions toward the pediatric patient should be interpreted with caution, as only 20 of the 494 physicians surveyed were trained in pediatric specialties. We warned that our pediatric resuscitation data might not reflect the opinions of the larger community of pediatric intensivists. In fact, Dr. Creamer’s published data show that he and many other pediatricians have embraced FWR with great success and satisfaction.

Our survey data did not show a statistically significant difference of opinion toward FWR of children when pediatric-trained health-care professionals were compared to adult health-care professionals (26.1% vs 14.2%, respectively; p = 0.138); however, the subgroup of 12 primary pediatricians was more likely to favor FWR for children compared to adult-trained health-care professionals (41.7% vs 14.2%, respectively; p = 0.022). While subgroup analysis supports Dr. Creamer’s position, we believe that the number of pediatricians we surveyed was too small to draw definitive conclusions.

In summary, we agree with Dr. Creamer that this area requires more research and discussion before FWR can be "advocated wholesale." In the interim, hospital leaders should consider each patient and family situation individually and should ensure that physicians and nurses are trained to support FWR in appropriate circumstances.

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Clarifying Cardiac Decortication Procedure

To the Editor:

I read with interest the article by Byrne et al in the December 2002 issue. I have two questions regarding management of this difficult patient cohort.

First, in the cases in which the parietal pericardium was excised on the left side, was Gortex (WL Gore; Sunnyvale, CA) or other material used to close the pericardium? Might replacing the pericardium, which is usually not recommended, prevent this complication or at least lengthen the interval until it occurred?

Second, it appeared that the patients who had positive histology results or histology demonstrating tumor recurrence on reoperation had a much poorer survival rate. If there was tumor present on the frozen section, should the cardiac decortication be aborted?

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REFERENCE

1 Byrne JG, Karavas A, Colson Y, et al. Cardiac decortication (epicardectomy) for occult constrictive cardiac physiology after left extrapleural pneumonectomy. Chest 2002; 122:2256–2259