can be designed for modification at the local level to ensure compliance with local law. I have no doubt that solutions to the complicated medical and legal problems associated with the prehospital resuscitation decision-making process can be found. I invite members of the organization to participate in the process of developing such guidelines to ensure that the rights and interests of all parties in the system are respected and protected.

References

1. American Heart Association: Standards and guidelines for cardiopulmonary re suscitation and emergency cardiac care. *JAMA* 1986;255:2841.
2. *Ibid.* 2980-2981.
3. *Ibid.* 2980.
4. *Ibid.* 2980-2981.

Richard A. Lazar
Portland, Oregon

To The Editor:

It was flattering to be the subject of such an extensive commentary by Dr. Moles (Vol 5:271-272). Close reading of this "critical review" reveals three distinct types of comments. We are pleased to respond to each of them.

I. Issues that were explicitly noted and discussed in the original article in Vol 5:

A. "No rationale or criteria are given for the selection of the pacemaker electrode combination in each subject..."

However, page 146, paragraph 1 notes explicitly that "the particular pacer electrode combination selected for each subject was determined by a previous TCP study in which moderate to severe discomfort was experienced at capture threshold."

B. "Unblinded exposure in the N_2O trial...introduces a placebo-type bias; this error could and should be quantified in a comparative trial blinded by use of cylinder medical air delivered through an identical system."

Again, the issue is overtly noted. Page 146, paragraph 3 states, "...pilot trials demonstrated that subjects invariably distinguished the nitrous oxide gas from a control gas. As a result, these trials were unblinded." Then once more, on page 147, paragraph 6, we note, "...limitations to the present study...the study was unblinded due to the ability of subjects to distinguish the nitrous oxide gas from the 'control' gas."

C. Dr. Moles states, (page 272, Paragraph 1) "Prior exposure to N_2O, providing previous knowledge of effect, unequivocally (italics ours) compounds this error with a second conditioned bias favoring N_2O...."

But page 148, paragraph 1, specifically deals with this question. "...each subject had participated in a previous TCP (not nitrous oxide) study and was familiar with the technique of TCP....Previous (TCP) experience should not have introduced a consistent bias favoring the nitrous oxide or room air trial." Actually, very few of the subjects had previously experienced nitrous oxide and how this would affect their pain perception is far from "unequivocal," it is extremely speculative.

D. Dr. Moles takes us to task for "omitting comment on capture verification in the N_2O trials." Careful reading of page 146, paragraph 2, however would have revealed "Electrocardiographic (EKG) documentation of capture was required for all trials."

II. A second category of comments may be grouped under the rubric "comments that are factually wrong." Space limitations preclude an inclusive listing, but we note a few.

A. "The range given for the PVAS (Pain visual analog scale) extends only to 8, which seems paradoxical."

There is nothing paradoxical about it. The upper range of responses was indeed 8.0. The maximum possible response was 10.0 representing very severe pain. Clearly, no subject considered his pain to be "very severe" even if he elected to have his pacing discontinued. This is neither surprising or paradoxical.

B. "The last sentence of the methods section seems far from exact!"

Really? The sentence in question states, "Where appropriate, preferences for the respective trials were compared with the Chi-Square or Fisher's Exact Test." As frequent readers of medical journals already know, this is a commonly used phraseology when one of two similar statistical tests is more appropriate than the other due to cell frequency. As always, Fisher's Exact Test was utilized for analysis when cell size was low, Chi-Square in the other cases.

C. None of the statistical criticisms appear valid. The assertion (page 272, paragraph 6) states,"The pacer time trial reports means of 22.4 and 23.8 seconds....These data are not normally distributed and the t-test is invalid."

There are two errors in this statement. First, the distribution, while obviously not a perfect normal distribution, is, in fact, not markedly skewed. Further, the t-test utilized is quite robust to violation of normality assumption when n=18, as it did here.

III. The third type of comments deal with question and definitions that were not quite clear to Dr. Moles. We are happy to clarify them, although it seems likely that they would have been a source of ambiguity to most readers. The capture threshold was expressed as 103±37 ma—this does, indeed, refer to the entire range (not the standard deviation) of the responses. We did not report whether the 15 (out of 18) subjects expressing a preference for nitrous oxide was statistically significant as this type of two-tailed exact binomial test is almost trivial and likely to be misleading. For what it is worth, the value is indeed significant at p .0075. "Premature termination" means that the subject asked us to stop TCP due to discomfort. "Prolongation time" refers to how much longer a subject could be paced with nitrous than without. We are not surprised that the other peer reviewers had no difficulty understanding these concepts, even without explicit definitions. And yes, the consent from explicitly mentioned in our article was (like the study itself) approved by the University of Pittsburgh IRB.

Finally we would just note the remarkably eclectic concatenation of "confounding variables" that we are berated for not specifically excluding: "psychotropics; fasting; food; and alcohol intake; exercise; circadian endocrine/endorphin variations, e.g., menstruation and endomorphin variations...". We will allow PDM's readership to reach their own conclusion regarding the criticality of such factors in a TCP study. And, we hope that our comments will similarly allow them to evaluate the overall validity of Dr. Moles' critique.

Michael B. Heller, MD
Associate Professor of Medicine
University of Pittsburgh
Pittsburgh, Pennsylvania

To The Editor:

These comments are in reference to the recent article by Schwartz et al on the "Role of the Physician in a Helicopter Emergency Medical Service," (Vol 5,1) and the follow-up correspondence from Morgan (Vol 5,2). Dr. Morgan seems convinced that patients might be treated by non-physicians for serious or minor illnesses, particularly in a helicopter EMS.

It is well-established in American medicine that physician assistants and nurse practitioners are able to provide basic medical care. Indeed, such properly licensed and supervised individuals are authorized legally to administer such care, in both the hospital and outpatient settings. Additionally, the medical profession has decided that specially trained paramedics are the appropriate health care providers for the great bulk of EMS patients, when associated with physician consultation for medical control and treatment protocol development.

Clearly, there are some complicated EMS cases which might necessitate the intervention of a physician during flight, but as Schwartz and his colleagues so nicely show in their paper, these cases remain a distinct minority. The dispatch of physicians on helicopters for every EMS call would take physicians away for areas of greater need, e.g., busy emergency departments with high acuity patient loads. It is with this reasoning in mind that prehospital medical care has evolved to its present form, with EMT-I, EMT-D, and EMT-P staff providing care for patients outside the hospital. As with so many other issues, more is not necessarily better; so it is with the presence of physicians on the great majority of helicopter EMS flights.

Steven J. Rottman, Jr., MD
Medical Director
UCLA Center for Prehospital Care
Los Angeles, California