

## CORRESPONDENCE

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*In Reply:*—Dr. Partridge suggests that one solution to the plagiarism issue that I discussed in an earlier editorial is to avoid "... the ubiquitous habit of senior authors taking credit for the work product of their junior colleagues..." He also suggests that coauthorship is "exactly the equivalent of plagiarism." I suppose that, at face value, there is some validity to such a statement if most cases of shared authorship did actually represent such an egregious "stealing" of credit. In my opinion, however, this is an overstatement of reality and ignores the usually positive nature of collaborative efforts. The multiauthored publication is the norm today, not because of desire on the part of senior authors to engage in the wholesale theft of the efforts of their junior colleagues, but because science is, in fact, a collaborative activity, as are the overwhelming majority of the articles that are the product of that activity. Research training is now, and always has been, a kind of apprenticeship, with younger workers learning to work and write in a progressive fashion from more experienced practitioners. If a younger individual authors an article, does Dr. Partridge believe that he or she should receive no input from his or her advisors? I would agree that such input need not always result in authorship for the advisor. If the advisor does nothing more than fund the laboratory or provide general, but distant, supervision, I would agree that authorship is not warranted. Dr. Partridge is correct in suggesting that such "token" authorship (perhaps undertaken to add credence to the article or to gain credit for the senior author) is wrong. However, what if that advisor provides guidance for the author, plays a central role in the ideas that form the article, and personally "edits" or even writes key portions of the article? In my opinion, authorship under such circumstances is totally warranted and appropriate. I also believe that this represents the overwhelming number of articles, including editorials that we publish.

Can plagiarism occur under such circumstances? Absolutely. Is it realistically possible for any author to "know" the origin of every

sentence penned by his coauthors and, hence, detect such events before they appear in print? Absolutely not; to do so would require an inhuman knowledge of the millions of published articles that exist in the literature, any one of which could be the source of copied material. Does such "ignorance" absolve the senior authors of the unethical actions of his coauthor? No, the senior author must accept responsibility in the same manner that a senior military officer retains responsibility for the actions of his subordinates. In the case in point, it is clear that the senior author did indeed accept such responsibility and suffered the humiliation of being publicly identified as participating in the publication of plagiarized material.

The solution to plagiarism is *not* to dissolve the unquestionably beneficial relationship between junior and senior, either in terms of the work they do or the articles they publish. The solution involves a combination of educating junior and senior authors about the ethics of their chosen profession, vigilance on the part of authors and editors, and a willingness to publicly disclose such events when they occur. The published letters from Drs. Bhardwaj and Kirsch (published at the insistence of their institution as well as ANESTHESIOLOGY) and my accompanying comments represent the last of these actions—an action that will hopefully contribute to a better understanding of the reasons that "ethical rules" exist.

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## New Temperature Monitoring Guidelines: An Observation and Caveat

*To the Editor:*—I would like to offer an observation and to suggest a caveat, if I may, to the "where to monitor" guidelines,<sup>1</sup> especially as related to skin temperature.

I recently noted a strikingly rapid increase in indicated temperature, early in an anesthetic, derived from a probe located over an axillary artery beneath an upper arm blood pressure cuff. In verification, I measured a normal nasopharyngeal temperature and coincidentally noted that the cutaneous temperature, compared with the nasopharyngeal

temperature, increased to a positive difference of 1.5°C over 5 min as the temperature of the upper body forced air warming blanket increased toward the set temperature of 43°C. Remarkably, the initial response (0.1°C) to the warming blanket temperature change seemed, in retrospect, to begin within less than 1 min. When the blanket set temperature was subsequently reduced to 38°C, the positive difference very rapidly decreased to +0.2°C and then, over the next 20 min, to -0.5°C. I have noted a similar, but smaller, positive difference effect

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from a cutaneous probe on an arm receiving warmed fluids at a rapid rate.

The increasing use of the laryngeal mask airway makes the esophageal site less attractive for core temperature measurement; skin temperature is an easy alternative. Rectal sites will never be frequently used outside of cardiac surgery, accuracy notwithstanding. Given the concurrent increasing application of forced air warming blankets and effective fluid warmers, I would like to suggest an alternative. An esophageal temperature probe, placed inside the finger of a disposable glove to minimize trauma, or a well-lubricated small bore esophageal stethoscope/temperature probe combination, inserted into the nasopharynx, are just as easy and provide a much more reliable measure of core temperature. Furthermore, in my experience, nasopharyngeal temperature is rarely spuriously overelevated or depressed, whereas skin temperature seems to be trustworthy only so long as it behaves as predicted.

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## References

1. Sessler DI: A proposal for new temperature monitoring and thermal management guidelines. *ANESTHESIOLOGY* 1998; 89:1298-9
2. Patel N, Smith CE, Pinchak AC, Hagen JF: Comparison of esophageal, tympanic, and forehead skin temperature in adult patients. *J Clin Anesth* 1996; 8:462-8
3. Marsh ML, Sessler DI: Failure of intraoperative liquid-crystal temperature monitoring. *Anesth Analg* 1996; 82:1102-4

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## Tension Pneumothorax and Apnea Tests

*To the Editor:*—Bar-Joseph *et al.*<sup>1</sup> are to be commended for their report of tension pneumothorax during apnea tests for brain death. We are aware of no previous reports of this complication. We believe that our case of "thoracic inflation"<sup>2</sup> was a result of tension pneumothorax; however, we did not prove that diagnosis by chest radiography or needle thoracotomy. Perhaps because of this omission, our recommendation for using a small-diameter cannula to prevent barotrauma has not found its way into published practice guidelines.<sup>3</sup>

Bar-Joseph *et al.* recommended an oxygen flow rate of no higher than 6 l/min. We used a rate of 15 l/min in our study<sup>2</sup> and in more than 400 subsequent adult apnea tests with no additional occurrences of barotrauma. This rate does not cause CO<sub>2</sub> washout, and, in the absence of cannula wedging, it does not seem to cause tension pneumothorax. However, based on available data, this rate may be no more effective than 6 l/min.

We share the suspicion of Bar-Joseph *et al.* that tension pneumothorax is not rare in apnea tests. The lack of reports is probably due to an understandable reluctance to publish bad results. We strongly recommend that practice guidelines for apnea tests in brain death be revised to include the technique reported by Bar-Joseph *et al.*

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## References

1. Bar-Joseph G, Bar-Lavie Y, Zonis Z: Tension pneumothorax during apnea testing for the determination of brain death. *ANESTHESIOLOGY* 1998; 89:1250-1
2. Marks SJ, Zisfein J: Apneic oxygenation during apnea tests for brain death: A controlled trial. *Arch Neurol* 1990; 47:1066-8
3. Practice parameters for determining brain death in adults (summary statement): Report of the quality standards subcommittee of the American Academy of Neurology. *Neurology* 1995; 45:1012-4

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