reported vomiting or aspiration. Moreover, there were no cases of traumatic complications reported in the patients treated with nebulized lidocaine.

There were limitations to this study. The study was not blinded, but the participants for the "experimental" group were selected for inclusion by the paramedics. It is possible that the group of patients who received nebulized lidocaine is a different population from the "control" group. It is possible that they selected a group of "easy-to-intubate" patients, thereby falsely elevating the success rate in the "experimental" group. However, it is not likely that this phenomenon occurred. A weekly call review was conducted by the medical director, during which all cases involving the use of nebulized lidocaine were discussed retrospectively. During these sessions, the paramedics indicated the most common reasons that they "chose" not to use the treatment protocol were: 1) "too close to the hospital;" and 2) the patient had previously had "easy" intubation by paramedics and "didn't need it."

Conclusion

When nebulized lidocaine is used as an adjunct to endotracheal intubation in the prehospital setting, it is associated with significantly higher intubation success rates by paramedics. The technique is taught to paramedics readily, and uses drugs and equipment already available to them. This study indicates that nebulized lidocaine can be used safely and efficaciously in the critically ill or injured patients in the prehospital setting.

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To the Editor:

The following is in response to the critique of our article on Firefighter/Paramedic (FF/PM) Burnout by the EMS Journal Club in the *Research Review* section of the June issue of the *Journal of Emergency Medical Services (JEMS)*. We offer the following clarifications and rebuttals in response to the criticisms offered by the "Facts" (Heramba), "Truth" (Rick), "Real Truth" (Lawrence), and the "Ultimate Truth" (Jack) team from the Department of Emergency Medicine at East Carolina University School of Medicine in Greenville, North Carolina.

In response to "The 'Facts' According to Heramba" . . .

The "Facts" cited by Heramba pretty well summarize our research. Heramba states, "There is no doubt in my mind that certain common personality traits exist among those who choose to be rescue/health care workers. Whether the ones chosen by the authors are good representations of these I do not know." We, of course, think they are, though, we also agree there may be others.

In response to "The 'Truth' According to Rick"...

Rick was stunned that our article implied, "that paramedics are more authoritarian than police officers." Our article did not imply, it factually compared the research available concerning the authoritarian scores of *one* sample of police officers to *one* sample of FF/PMs.

In response to our pointing out that other studies have shown that highly authoritarian individuals are less comfortable in highly autocratic organizations, Rick states: "Many EMS organizations are pretty autocratic. It seems like a setup for a no-win situation." In our discussion, we suggest that this autocratic style of management perhaps is part of the problem, should be studied further, and should be changed if found to be a contributor to practitioner burnout.

Rick has made a good point in his statement that what we "fail to do is talk at all about the interactions between the 'authoritarian' FF/PMs and medical 'control.'" We chose not to address medical control. However, we agree this may be an important factor related to authoritarianism for this population and deserves attention in future studies.

In response to "The 'Real Truth' According to Lawrence" . .

Lawrence's first point is that in research, "a correlation does not necessarily mean 'cause and effect,' and lack of correlation does not rule out 'cause and effect.'" We couldn't agree more. However, we

claimed no such "cause and effect" relationship.

Lawrence then goes on to point out that the burnout scores obtained in our research were low, and therefore insignificant. We do not agree. The burnout scores obtained in our research closely matched the distribution of scores using the same instrument with other samples in which burnout was found to be a significant problem. Interpreting the scores as ratio-level data is not appropriate. A burnout score of 60 cannot be accurately interpreted as twice the burnout of a score of 30. In a similar vein, a low score cannot be interpreted as though the middle point of all possible scores represents "normal" burnout, nor assume further that a low score, in reality, means low burnout. A high score is not necessary in order for burnout to be a significant problem in real life. The primary value of the scores was in permitting us to test the probability of there being a relationship between burnout and the other factors included in our study, not as a meaningful descriptor of burnout for this population.

Finally, Lawrence concludes by hypothesizing that caring, frustration, and altruism may be contributors to burnout. Perhaps these variables should be researched for correlation with burnout in FF/PMs. Lawrence also mentions the frustration of going on emotionally difficult emergency calls, but that FF/PMs should not stop caring. Again we couldn't agree more.

In response to "The 'Ultimate Truth' According to Jack"...

Jack begins his critical review of this research by stating: "there are many difficulties with this paper, the first of which is the title; it's both verbose and misleading. Since firefighters and paramedics in many systems perform two entirely different jobs, they should not have been lumped together." We are aware that sometimes the jobs of firefighting and paramedic work are distinctly different. However, the subjects were referred to accurately as FF/PMs. Our title precisely described both the study parameters and the subjects studied.

In reference to Jack's being dismayed at our erroneous reference to his co-authored article, we note that Jack fails to point out how he sees that we misquoted his article. Furthermore, Jack's article only was mentioned as one of many others dealing with the general subject of "psychological stressors" that may affect firefighters and paramedics, and potentially impact their respective burnout.

Jack's next criticism was that, "The small numbers in this study do not support the author's conclusions." A sample size of 91 is not huge, and at this point, these findings certainly cannot be generalized to the entire population of FF/PMs. That's what replication studies are for. However, the sample size is certainly large enough, and the study design rigorous enough to be published as "original" research. Moreover, it is worth noting that the sample was 98% of the population of FF/PMs in the fire service. Incidentally, the sample size of Jack's co-authored article referenced above was 79!

Finally, Jack states that we, "wander off on diverse tangents, editorialize about stress management training and counsel on issues of control, as well as discuss the effects of burnout on personnel." We do make several suggestions in our discussion section of the article which are appropriate and expected for applied research. All of the suggestions related to the findings of the study. Obviously, these suggestions need to be tested to determine their value. Jack fails to make himself clear as to why he feels that this research falls short of the mark as original research. Obviously, other authorities in the field believed that it met original research criteria as this article was published in a refereed journal.

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