To the Editor:

We read with interest the abstract by Krommer et al entitled, "In-Field, Extremity Amputations: Prevalence and Procedure in Emergency Services," [*Prehospital and Disaster Medicine* 1992;7 (Suppl. 1):33s]. The authors concluded that training related to in-field extremity amputation should be emphasized by emergency medical services (EMS) agencies and reinforced through continuing education. Ninety-six percent of those responding to the authors' survey stated this training was not available through their EMS agency.

We wish to report that a field amputation training program has been available in California since 1991. We developed a training course which teaches physicians to perform amputations and fasciotomies under austere conditions (*Ann Emerg Med* 1992;21:613). Although this laboratory is part of the overall Medical Disaster Response (MDR) training course, which targets victims in a major earthquake, the techniques used are applicable to any field amputation. The training includes didactic sessions which discuss indications for amputations, anesthesia, fluid management, and treatment and prevention of rhabdomyolysis. A laboratory session also is offered, where participants perform amputations and fasciotomies on fresh, human cadaver material under direct supervision. In addition, we have received a grant from the American College of Emergency Physicians to produce a video demonstrating this training. To date, we have trained more than 150 physicians.

We are concerned that the survey results showed that in 11% of cases, EMT-Paramedics were considered responsible for performing amputations. In large, metropolitan EMS systems, it is our opinion that such a procedure should be performed by a physician with prior experience and training. It is not possible to justify amputation as part of the paramedic scope of practice under these conditions. Instead, it would be better to require "Go" team support at the local trauma center. Under austere disaster conditions or in rural situations where a trained physician is not immediately available, other rules may apply.

If we consider that only a handful of in-field amputations occur in EMS systems throughout the country, it appears more cost-effective for community physicians to obtain this specialized education and training through an already established system (like the MDR project), than to require each EMS agency to develop a completely new program.

To the best of our knowledge, no other training program of this type currently exists. We welcome comments from others who may be aware of other such programs.

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To the Editor:

I read with interest the article entitled, "An Analysis of Invasive Airway Management in a Suburban Emergency Medical Services System," by Krisanda et al in the April–June 1992 *Prehospital and Disaster Medicine.*

The authors are to be congratulated for a carefully researched and most interesting paper.

I would like to express a couple of concerns from my point of view as Medical Director of an ambulance service. Firstly, as an anaesthesiologist, I am painfully aware of the degree of hypoxia that can accompany an unsuccessful intubation, especially with repeated attempts. And this can happen even under the near ideal circumstances of an operating room intubation. I believe there is a potential for taking patients, who were in fact breathing successfully, and rendering them hypoxic. Your authors stated that even experienced providers only were successful on the first attempt in 57% of cases.

My other concern is that the article does not answer or even ask the one most important question relating to this procedure, "Did the patients benefit?" An assumption is made that intubation inevitably benefited all these patients. It would be most interesting to compare such a group of patients with a group in which their airways were managed by competent, well-trained ambulance officers, but without intubation.

I had one unfortunate experience of watching a very comprehensively trained paramedic in one United States system follow his protocol for a patient with COPD to the full. The patient, who when they started was somewhat distressed but unable to speak, initially was given suxamethonium, then diazepam, intubated, and ventilated, causing some bleeding in the process and damaging a tooth. In my own system, the patient would have been given one to two liters of oxygen and transported in comfort to a hospital. I am quite sure the patient would have done at least as well.

Many assumptions have been made about paramedic procedures, which have gained a momentum all their own. Perhaps, we should look more often at whether we actually are improving outcome significantly by these aggressive and not entirely innocuous procedures, as in the article by Lavery et al in the same issue.

I am not by any means saying that no patients need to be intubated, but I believe it to be an invasive and potentially harmful procedure, for which one must have good grounds.

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To the Editor:

Some years ago, there was a change in the nomenclature as well as the prehospital treatment recommended for a patient at risk of a spinal injury: neutral treatment or neutral positioning was recommended to replace the practice of applying cervical traction. The implementation of this change was gradual; the information has been disseminated to the field provider slowly (over several years), mainly through EMS texts, educators, and conference speakers. The result has been a positive evolution in patient care.

In the interest of providing better patient care, it may be time for another change in terminology. I believe that it may be worthwhile to replace the term "spinal immobilization" with the more accurate term "spinal motion restriction" or "spinal motion restriction procedures." Immobilization, as defined by *Webster's Dictionary*, is to render incapable of movement. In prehospital care, we cannot provide immobilization as the word is defined. Review of the literature corroborates this fact. Studies show that even the halo device, perceived by many to be the standard of "immobilization," allows a significant amount of movement.

As we continue to work for improvements in all aspects of patient care, it is important that the terminology used accurately reflects the skill or procedure. By suggesting that correctly applied spinal precautions provide immobilization, we give EMS providers a false sense of security. This may be part of the reason we continue to see potentially dangerous treatments such as patients wearing only a cervical collar, patients walked to ambulances, or some other variation of inadequate spinal care. One only has to turn on a news broadcast in virtually any city in the United States to see inadequate spinal care at an accident scene. Although these types of incidents are on the decline, it clearly shows that that there are many who still do not realize that extrication collars, for example, do not provide immobilization. It seems reasonable to believe that moving away from the term spinal immobilization could help to ensure that EMS personnel recognize that patients at risk of a spinal cord injury require and deserve a system approach to limiting motion and the potential for further injury. If motion restriction is stressed in initial and continuing education classes, hopefully, we will see an improvement in the quality of care delivered to the patient. Certainly, I understand that simply by changing terminology, we will not eliminate all future problems, but I do believe it is a positive starting point.

I would encourage those who agree not only to incorporate spinal motion restriction into their vocabulary, but urge their medical directors to support its use on a system-wide basis. Perhaps a concerted effort on the part of medical directors, educators, and providers will hasten acceptance of the concept of motion restriction and further the evolution of patient care.

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To the Editor:

We write in hopes of answering the question, "Does MAST Make a Difference," that was posed at the Winter Meeting of the National Association of EMS Physicians, held in Naples, Florida, in January 1993. We comment on Dr. Mattox's meritorious paper and, in doing so, focus on his reported subset of 127 patients where MAST appears to have reduced morbidity by 9% in those patients in profound hemorrhagic shock.

This aspect of the data usually is not commented on by those who cite Dr. Mattox's paper to justify removing MAST from ambulance and paramedic units nationwide.

MAST in Profound Hemorrhagic Shock

In 1976, Dr. Eugene Nagel clearly showed that the MAST was capable of raising the blood pressure in patients in deep hemorrhagic shock.[1] Since then, 700 researchers have produced more than 250 papers in trying to define the use, mechanism, efficiency, and limits of MAST. Most recently, papers from San Francisco[2] and Houston[3] claim to have put the final nail in the coffin in the MAST dispute by presenting data that are meant to show that MAST does not in any way positively affect survival. In fact, Mattox claims that his study of 784 patients shows an increased mortality rate of trauma patients treated with MAST (31% vs 25%), compared to his NO-MAST matched control group. Dr. Mattox hoped his study would "forever close the book on...MAST." Likewise, Mackersie, Christensen, and Lewis at San Francisco General studied 161 trauma patients treated with MAST and found that it was "clear that there was no overall improvement in survival or clinical status in MAST treated patients when compared to conventionally treated patients."

Interestingly, if one looks at Table 3 of the cited study from San Francisco General,[3] their claim that "there appears to be no advantage to using MAST," does not bear out.

Table 3—Group 3 (profound shock, but not moribund)

Intervention	No. Patients	No. Death	Mortality Rate (%)
MAST	9	4/9	44
No-MAST	6	4/6	66

In this significant subset of their series (those in deep hemorrhagic shock, but not moribund), there actually is a 22% increase in survival in the group who had the benefit of MAST. Granted there only were 15 patients in this arm of their study, but this certainly does not support their conclusion that "there appears to be no advantage to using MAST." In fact, this study is entirely consistent with the original data presented in 1976 by Nagel: 12 patients in deep shock, all showed a positive blood pressure response with 80% survival.

Ultimately, Mattox's most famous, most labor-intensive, and most meticulous paper was needed to clarify the situation further.[3] Mattox, starting with 9-1-1 patients in a prospective manner, clearly identifies a subset of 127 patients in Table 4 who had prehospital blood pressures of less than 50 systolic, who, when treated with MAST, had a mortality of 62%, compared to a mortality in their No-MAST controls of 71%.

Table—Blood Pressure < 50 mmHg

Intervention	No. Patients	No. Deaths	Mortality Rate (%)
MAST	55	34	62
No-MAST	72	51	71

Again, this larger study also seems to be in agreement with the original studies by Nagel's group that showed that the benefit of MAST was with patients in profound shock.[4] Mattox does comment that this beneficial effect was not seen in those patients with major vascular injuries. Hence, this 9% increase in survivability in profound shock was in those patients with blunt abdominal trauma.

In conclusion, victims of blunt abdominal trauma in hemorrhagic shock, in the prehospital environment, still should receive the benefit of MAST.

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https://doi.org/10.1017/S1049023X00052109 Published online by Cambridge University Press