

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 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 Francisco² and Houston³ 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, Mackerse, 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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