To the Editor:

In the article, "Paramedic Field Instructors: An Approach to Training the Newest Paramedics While Maintaining the Interest of the Most Successful Senior Paramedics," published in *Prehospital and Disaster Medicine*, April–June 1995, Krochmal et al have attempted to address two major problems all emergency medical services (EMS) systems face: 1) effective integration and supervision of the clinically inexperienced advanced life support provider; and 2) motivating the senior paramedic to remain enthusiastic and committed to competent (and compassionate) clinical-care delivery.

The paramedic field instructor program crafted by the New Haven EMS system may be unique because it has used a single approach to handling several different system management needs. The idea of taking senior, experienced paramedics and finding a way to motivate them to share their experiences with others is not a novel intent. However, their success may be. Under the system they describe, everyone wins—the paramedic, the student, the new intern, the senior paramedic, even the patient!

After reviewing the article, I was left with several questions. They include:

1. Was the selection criteria used to identify the 30 paramedic field instructors adequate in providing the system with the quality they desired? For example, was there a difference between those selected who had previous teaching experience compared to those without? The initial four-hour training period seems, on the surface, to be quite short to prepare the inexperienced instructor/preceptor adequately.
2. The idea of presenting challenging continuing medical education (CME) to the new as well as the "old" paramedic is a constant challenge. Was the monthly three-hour CME in which the paramedic field instructors participated in addition to or in lieu of the normal monthly session that they already were expected to attend? If it was in lieu of that normally required, were there any problems encountered with meeting national registry and/or state recertification requirements, given that it appears that many of the items discussed were not necessarily of a purely clinical nature?
3. There is little reason to doubt that the presence of the motivated group of paramedic field instructors can lead to improvements in training and field performance. However, what objective data were found to validate the "real" benefit of the new program? In turn, can these data now be used to justify these individuals receiving a higher salary for their work?

This paper also describes the instructional and the evaluational role played by the paramedic field instructor. However, it failed to describe the system criteria used to assure performance quality among the paramedic field-instructors. Did each paramedic field instructor become responsible for only one student or intern, or several? For the system to receive an ultimate benefit from a paramedic field-instructor program, each performance must be of a similar qualitative nature. Was there satisfactory inter-reliability found among the 30 personnel? If not, what were the problems that were seen?

In summary, the paper presents one suburban area's attempt to implement an innovative idea to meet several needs. Their experience would seem to verify what many systems have known for some time. Some of our best instructors are not found just in the classroom. Despite the important information shared by their report, there are a number of other questions that must be answered before the full impact of a program like this can best be understood and emulated.

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To the Editor:

Schmidt et al reported the successful resuscitation of a child with severe hypothermia after cardiac arrest of 88 minutes in 1986 in the January–March 1995 issue of *Prehospital and Disaster Medicine*.[1] Although the patient was asystole at the time of admission to the emergency department, he was warmed by external warming and warmed inspiratory air during prolonged mechanical cardiopulmonary resuscitation. This 9-year-old report, which was first published in 1988, is still most remarkable with its very good outcome.[2]

Recommendations for the treatment of severe hypothermia have changed,[3] and different management of severely hypothermic patients with cardiac arrest is used in our emergency department. We have treated 26 severely hypothermic patients with body core temperatures below 30° C in our emergency department since 1992. Four patients suffered circulatory arrest (one EMD, three patients ventricular fibrillation). One patient (28.7° C) stabilized after defibrillation and was warmed by warmed infusion, warmed inspiratory air, and bair hugger. Three patients (23.9 to 24.6° C) were warmed by cardiopulmonary bypass (CPB) (Biomedicus, Medtronic pump with a heparin-coated system). Percutaneous vascular femoral-femoral cannulation required 17 minutes to 35 minutes. CPB-time was 90 minutes to 205 minutes. All four patients were long-term survivors without neurological deficit. The use of a mechanical chest compression system, "Thumper," facilitates prebypass management (i.e., the cannulation of vascular access), but cannot replace cardiopulmonary bypass.

Although successful reanimations of asystolic patients with accidental hypothermia by other methods have been reported, the best possibility to restore spontaneous circulation in asystolic hypothermic patients is active rewarming by CPB.[4,5] Hospitals that have the possibility of treating hypothermic victims should possess the facilities for CPB, so that extracorpal life support can be provided without delay in life-threatening circulatory failure in hypothermic patients.[6,7]

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To The Editor:

Performance of chest compressions during prehospital transport is an underinvestigated issue. The recent publications by Stone and Thomas on resuscitation in ambulances and helicopters are, therefore, of great importance, and I know of only one report from another author on this subject.[1–4]

Please allow me some constructive criticism and some questions that possibly could be answered by Stone and Thomas in the Forum section of *Prehospital and Disaster Medicine.*

Their study on chest compressions in ambulances does not mention the type of ambulance used, the speed of the moving ambulance, and the success of chest compressions in a standing ambulance. It showed that chest compressions are difficult to perform in a moving ambulance, but does not answer the question of whether the problems are related to the movement, the ambulance design, or both.[1]

An influence of ambulance size and design is quite possible because the same authors showed differences between two types of helicopters.[2] If the ambulance design is the main problem, which could be shown by similar low rates of correct compressions in a standing and a moving ambulance, better ambulances would be an adequate solution. A pressure-sensing device, which was used successfully for two minutes in the "cramped quarters of the BO-105," seems a suboptimal solution because of the high physical demands to the operator.[3]

An influence of speed was shown by Greenslade who reported greater difficulties when driving over 30 mph, but this report is only qualitative and does not mention the type of ambulance used.[4] If the ambulance movement is the main problem, transport in a helicopter, preferably in a MBB BK-117 or something similar, would be a solution.[2] Obviously this is not always possible. A lower speed is another solution that also reduces the risks to the operator who stands in an ambulance driven with warning lights and siren. However, a lower speed prolongs transport, and this could be detrimental for the patient even if it is associated with better quality of chest compressions.

So pneumatic devices are probably the best solution to the problem because they might enable a better quality of chest compressions, allow the operator to be seated, and free the operator for other tasks. Further studies on this subject are needed.

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To the Editor:

The fact that mask ventilation with more than 20 mbar risks gastric insufflation has been known for more than 30 years, but often is forgotten. The publications by Weiler et al and Devitt et al are important because they remind us of a common and dangerous complication that also occurs with the laryngeal mask.[1–3] Weiler et al propose limitation of pressure to 20 mbar during mask ventilation and a reduction in tidal volumes during cardipulmonary resuscitation.[1] We agree to this and want to add some aspects.

There is at least one manufacturer that implements 20 mbar pressure-release valves (that can be switched to 60 mbar for intubated patients) in both automated and manual ventilators (Medumat–®: and Combigag®: Weinmann, Kronsaalasweg, D-22502-Hamburg, Germany).[4–6] These devices are far from perfect, but they are able to prevent gastric insufflation. Their main disadvantage is the lack of a loud audible control of the pressure-release valve as realized in 1959 by Lucas.[7]

Recently, we tested 10 manual ventilators.[8] We did not measure pressures but found that the Weinmann Combibag® limited tidal volumes to 1,100 ml on a Laerdal Recording Resusci® Anne. Use of ventilation bags without pressure-release valves resulted in tidal volumes up to 1,500 ml. It should be noted, however, that 20% of the ventilations with the Combibag® were below 500 ml, and the device got a bad handling assessment. Both problems might be overcome by training and the above-mentioned implementations of an audible control of the pressure-release valve.

Another interesting device in our test was the prototype bellows ventilator Cardiovent® (Kendall, Raffineriestr., D-93333-Neustadt, Germany). The 40-mbar pressure-release valve of the prototype does not prevent gastric insufflation, but the tidal volume can be adjusted in 200-ml steps. It allows controlled tidal volumes of about 500 ml with mask ventilation, as proposed by Weiler et al, and tidal volumes of 800–1,200 ml after intubation with the same ventilator.

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