

To the Editor:

Since becoming a member of the World Association for Disaster and Emergency Medicine, I have had frequent opportunity to appreciate the quality of *Prehospital and Disaster Medicine*.

Three papers published in July–September 1994 and January–March 1995 attracted my attention, and I wish to share the following comments:

1. In the July–September 1994 issue, the paper on “On-Site Physicians and Scene Time” represents state-of-the-art usage of statistics. The hypothesis of the caption was, “The use of on-site ALS by physicians is associated with a significant increase in scene time.” On page 179/52, it reads “The purpose of this study was to describe and identify factors associated with variations in scene time for trauma patients treated by physicians at the site.” In the conclusion, the text restates the obvious fact that, “the use of physician-provided ALS is associated with significant increases in scene time.”

Up to this point, it is nice to see that this analysis correlates the “commonsense” of the “greater the number of activities performed on site, the longer time needed.” Still, in the conclusion, there is a great leap to: “In view of the lack of benefits associated with these interventions . . . study . . . provides further support for the general implementation of the ‘scoop and run’ approach.”

This called for a careful rereading of the paper.

The study group is defined (page 185/58) as, “all of the trauma victims with at least moderate injury” and “for whom a physician was present at the scene.” (So, patients were not divided into various groups according to their ISS to check if scene time is correlated to ISS.)

But on the same page we can read, “In the entire study cohort, the presence of a physician was associated with a statistically significant increase in mean scene time when compared with only EMTs being at the scene.”

What are we really comparing?

Comments from Tables 4, 5, and 6 are relevant, but, my question in relation to the conclusions is, if no actions are implemented on scene, and the victim is “scoop and run” to the hospital, is the victim going to be sent straight to the operating theater?

I believe that he/she will be assessed in the accident and emergency department and will receive necessary attention (IV line, intubation, medication, etc.) similar to what the patient would have received on scene in a prehospital-care approach. Then, and only then, the victim can be taken to the operating theater.

So, is the “scoop-and-run” approach really providing earliest necessary care, specifically with often overwhelmed accident and emergency departments? It also would have been extremely useful to know about any mortality rate from this study group, and then to have a statistical comparison with a similar group (with similar ISS and hospital definitive care level) not benefiting from prehospital ALS.

Instead of this, the conclusion is based on a controversial “upper limit for scene time” set by the author at 20 minutes, and “odds of dying.”

It is surprising to read in the same article, “There is no argument that increased prehospital delay is associated with worse outcome in severely injured patients,” (page 179/52) and “The significant impact of long prehospital time in causing excess mortality” (page 188/61). This study demonstrates only that providing ALS requires more time.

In my opinion, the conclusion of this article is inadequate and excessive.

2. It was extremely interesting to discover in the January–March 1995 issue, a paper on “Transport Time for Trauma Patients” concluded that “no prehospital transport time beyond which time transport to the closest hospital would have increased mortality was identified, because no prehospital time <90 minutes exerted a significant adverse effect upon survival.”

Even more interesting was the comment that “survival was associated with longer times,” which appeared to be surprising to the authors.

Here, it would have been interesting to know why prehospital transport time was different among the two groups (alive/dead).

As indicated in the conclusion, it is possible that more sophisticated prehospital care, inducing longer prehospital transport time, might increase survival.

The Sampalis and Petri studies seem relatively complementary; I hope that these two authors will develop joint studies on the “prehospital-time” problem.

3. The third article linked to this problem was in the July–September 1994 issue as “Evolution of Rescue Systems: A Comparison Between Cologne and Cleveland.”

The main difference between the two EMS systems was, “Germany preferred to concentrate on the first, prehospital step of rescue chain, whereas the United States improved the second step in the emergency department of the hospital.”

Table 2 shows that the German prehospital system is slightly more expensive than the U.S. system. Here, it would have been interesting to have a comparison between hospital-care costs in both systems.

Figure 3 shows that there is no significant difference in survival rates for each trauma score group between the two systems. Once again, the conclusion is frustrating: “From the medical viewpoint, based on current outcome measures, there is no indication to change the rescue systems . . .”

This is pragmatically true.

But through this study, are we authorized to think that “sophisticated prehospital care plus good hospital emergency care” have the same results as “good prehospital care plus sophisticated hospital emergency care”?

What about “scoop and run plus good hospital emergency care”? And which is the most cost-effective system?

In conclusion, from my European perspective, these three articles together highlight some points of the dilemma “stay and play” versus “scoop and run.” Of course, this is only a “nonepidemiological” impression, but . . .

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To The Editor:

I must commend Carol J. Shanaberger, Esq., EMT-P, for a well-researched and thoughtful paper on base-station legal liability. One of the problems of journal articles is the lag time between submission and publication. Thus, I feel the readers should be made aware of a fairly recent case that further defines the rela-

tionship between the emergency medical technician (EMT) and the base station.

In *Riffe v Vereb Ambulance Serv. Inc.*, 650 A.2d 1076 (Pa. Super. Ct. 1994) as reported in *Health Law Week* on March 24, 1995, 4 HLawWk 207, the Pennsylvania Superior Court supported a lower court decision dismissing a hospital from a case where the EMT gave 44 times the recommended dose of lidocaine. The plaintiffs settled with the ambulance company and the paramedics, but proceeded against the hospital. The trial judge dismissed the hospital, finding that the ambulance company and paramedic were ostensible agents for the hospital, and once the agents are released, the claim against the hospital is extinguished.

On appeal, the plaintiffs claimed their theory of negligence was not agency but straight negligence. The appeals court disagreed, stating the responsibility of hiring, training overseeing, and certification of paramedics was a responsibility of the emergency medical services and

not the hospital. Thus, with no theory of liability remaining, the appeals court affirmed the trial court ruling.

The rulings in this case are a double-edged sword for base stations and base-station physicians. While confirming the distant relationship between the base station and the EMT with regard to negligence in training, hiring, and supervision, the implication remains that the ambulance company and EMT still may be regarded as agents of the base-station hospital, thus imputing vicarious liability upon the latter. The appeals court did not have to rule on the vicarious liability issue since plaintiffs appealed on the negligence issue alone.

Stay tuned as more cases undoubtedly are going to appear to further define the roles and liabilities of all parties involved in prehospital care.

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