I would like to start out by congratulating Richard Mullins on an excellent conference. I think it is going to be very exciting, and I am happy to participate. I would also like to thank Dr. Mullins for the opportunity to discuss Ellen MacKenzie's presentation. You know, Dr. MacKenzie is one of those people who makes all of us want to be better scientists. I think her analysis is right on the mark. I am going to disagree with a few things, but the kind of careful approach that she has gone through is exactly what we need as we go through an evidence-based process.

I looked at this question a little bit differently and asked myself, is this type of analysis, in fact, out of date relative to what we are trying to accomplish. There were certainly studies that demonstrated that preventable death was occurring during the 1960s and in the early 1970s. At the time, system goals were to document timeliness of care and appropriateness of care, and that meant preventable death. There is little question that these studies helped to move the tide toward the development of trauma systems.

One can, in fact, make the argument that preventable death and the demonstration that preventable death has improved, is in fact what the development of trauma systems is all about. The golden hour, the Advanced Trauma Life Support program, trauma centers, and trauma systems exist to deal with this outcome. Interest in cost-effectiveness, decreasing the variability in the care process, functional recovery, and reduction of posttraumatic stress are much newer outcomes that would not exist without having solved the preventable death problem. There is little question that these studies helped to move the tide toward the development of trauma systems.

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There are legitimate concerns with this type of assessment, and Dr. MacKenzie has outlined these nicely. These concerns include the actual method of determination, the panel size and type, the consistency of definitions, the reliability of these definitions, and the form of data expression (percentage vs. actual number). Similarly, any preventable death study needs parallel statistical validation by using any one of the available outcomes such as TRISS, NISS, ASCOT, APS, or ICISS.

Given the problems then, does this remain a reliable measure of trauma system efficacy. The study by MacKenzie et al.1 has determined what preventability studies are not good for. Because of the variability that may assess quality and not preventability, potentially preventable death categories are probably unreliable. Similarly, judging complex aspects of care, such as the effects of fluid resuscitation on respiratory function, or attributing controversial care to preventability such as no deep vein thrombosis prophylaxis and pulmonary embolism, introduce variability in these studies, which can be very difficult to resolve. With this said, in her study when one compared the categories of nonpreventable and possibly preventable to probably preventable and definitely preventable, there was very good intrapanel and interpanel agreement, as high as 95%. One can conclude from this
important work that panel studies judging preventable death are good for determining the obvious. This is, in fact, exactly what they are supposed to determine.

If one makes the hypothesis that trauma systems get patients to the right hospital and deliver the right treatment, then one needs to decide what the essential elements of this treatment are. For non-central nervous system injuries, I would submit that the paradigm that is most important to those developing trauma systems, makes the assumption that after injury a patient is bleeding and that bleeding needs surgical intervention. Similarly, after a perforated viscus the need for intervention is essential. One then evaluates lack of intervention or delayed intervention for this kind of injury and defines a patient who does not have such an intervention as a preventable death. We now have a reasonable metric on which to test this hypothesis. By using this simple paradigm, I would like to review the preventable death studies done to date to evaluate whether, in fact, they have shown an effect of trauma systems.

The first studies are often referred to as the Orange County Studies by West et al.\textsuperscript{2,3} If one looks carefully, in 1974, 11 of 30 deaths were clearly preventable, 9 of which were caused by bleeding and 1 of which was caused by sepsis; 10 of these 30 patients did not have an operation. In 1978, 15 of 21 deaths were clearly preventable and 14 were caused by bleeding; 11 of the 21 had no operation. A reevaluation after system implementation found only 2 of 23 deaths were clearly preventable, and only 1 of 14 patients did not have an indicated operation. Clearly, any study like this has multiple confounding variables, but this would suggest the development of a trauma system and the delivery of patients to trauma centers reduced the incidence of patients who were bleeding and not operated on.

A similar study was done after implementation of the trauma system in San Diego.\textsuperscript{4,5} This study compared 1982 data in which 12 of 90 deaths were considered frankly preventable and the postsystem data in which 3 of 112 deaths were believed to be frankly preventable. Evaluation of the detail of these preventable deaths reveals again that timely surgical management (primarily to control bleeding) were the most frequent problems solved by trauma system implementation.

Importantly, an independent assessment by emergency physicians comparing two autopsy series in San Diego County before and after system development found 17 of 83 traumatic deaths preventable and only 1 of 62 deaths preventable after system implementation.\textsuperscript{6,7} Although many observations could be made from this study, the most important was that surgical intervention and control of bleeding was occurring.

A similar study was reported from Dade County in 1982 by Kreis et al.\textsuperscript{8} In this study, although different in methodology, there were 37 of 52 non-central nervous system preventable deaths believed to be caused by bleeding and delay or no surgery. After system implementation, the incidence of delay or no surgery for bleeding dropped in the trauma center but not in other hospitals still receiving patients. The study reported by Campbell et al.\textsuperscript{9} and Thoburn et al.\textsuperscript{10} compared the effect of system implementation in Hillsboro County, Florida, at two time points. They also found the incidence of delay to surgery for bleeding dropped, suggesting that by using this limited definition of system efficacy remains useful. It is easy to criticize the quality of all of these studies. They are not a substitute for continuous quality improvement or process improvement, error analysis, morbidity assessment, outcomes assessment, or measuring experience and volume performance. Most would agree that defining preventability has become a moving target as trauma systems and the quality of trauma care improves. Nonetheless, we need to keep these important studies in mind as we evolve in our assessment of trauma systems.
The fundamental question is do these studies demonstrate efficacy of trauma systems. If one's definition of a trauma system is prehospital transport, rapid assessment, and surgical intervention with fewer people not bleeding to death, then, I believe, panel studies have demonstrated this repeatedly.

By using this focused definition of preventability, this methodology remains useful. Perhaps the best way to place these studies in perspective is, if the data do not change when a system is put in place, it is not the measurement tool but the system that is need of change. Again, I want to congratulate Dr. MacKenzie on an excellent review of this important area and thank the planners of this meeting for the opportunity to comment.

REFERENCES


Address for reprints: David B. Hoyt, MD, UCSD Medical Center, Division of Trauma, 200 West Arbor Drive, San Diego, CA 92103-8896.