***To the Editor:***

Stein et al. report on the cost effectiveness of using computed tomography (CT) for minor head injury compared with several other management strategies.[1] They conclude that the selective use of CT in patients with minor head injury according to the Canadian CT Head Rule (CCHR)[2] is the most cost-effective management strategy. However, we think that their analysis is based on several debatable assumptions that may undermine the validity, and consequently, the conclusions of their article.

First, Stein et al. claim that CT is only 98% sensitive for the detection of lesions requiring neurosurgical intervention. This figure seems to be based on two studies, referenced in their article.[3,4] The first article indeed reports three cases of neurosurgical intervention with an initial negative CT. However, in all three cases neurosurgical intervention consisted of placement of an intracranial pressure monitor only, and recovery in all three cases was good. As Stein et al. specifically state in the Methods section of their article, only intracranial hematomas requiring evacuation were considered surgical lesions, which implies that classifying these three cases as false-negative CT results for neurosurgical lesions is not valid. The second article concerns a retrospective study of patients with minor head injury, in 92% of whom CT was performed. In the Discussion section of this article, the authors mention that 6 of 10 patients requiring neurosurgical intervention had negative findings on early CT, whereas there is no mention of these patients in the Results section and details concerning the type of neurosurgical intervention were not reported. These data can therefore neither be verified nor interpreted. The idea, however, that an early CT may be false-negative for neurosurgical lesions is well recognized, and there are numerous reports on these so-called delayed hematomas. However, the incidence has been shown to be extremely low (<0.02%),[5] indicating that the sensitivity of CT for detection of neurosurgical lesions approaches 100% and CT may therefore be used safely to triage minor head injury patients for clinical observation. Interestingly, Stein et al. state precisely this in their discussion, which, in our opinion, contradicts their assumption of a 98% sensitivity of CT for identifying patients requiring neurosurgical intervention.

Second, Stein et al. state that admitting all patients for 24-hour clinical observation has no advantage over discharging patients without further screening, whereas observation in the emergency department for 6 hours apparently does have a benefit over each of these strategies in terms of clinical outcome. This seems very contradictory, as it is difficult to understand that 6 hours of clinical observation does, but 24 hours of clinical observation does not have a positive impact on clinical outcome. Moreover, a recent report by Af Geierstam et al.[6] indicates that clinical observation performs just as well as early CT scanning in terms of clinical outcome in minor head injury patients, which also is in contrast with the assumption that clinical observation is no better than discharge without further screening.

Finally, the authors conclude that the selective use of CT and performing CT scanning in all patients yield the same number of quality adjusted life years and are therefore equally effective. This is hardly surprising, given that Stein et al. assume that the selective use of CT is just as sensitive for identifying patients requiring neurosurgical intervention as when all minor head injury patients are scanned. This assumption, however, is not valid, since external validation studies of the CCHR have shown that the 95% confidence interval of its sensitivity for identifying patients requiring neurosurgical intervention is wide, ranging from 63% to 100%.[7,8] The authors state that, were selective CT to miss 1% of surgical lesions, this strategy would become slightly less effective than a policy of scanning all patients, but the latter strategy would still be very costly. However, given the wide 95% confidence interval of the CCHR sensitivity, theoretically far more than 1% of patients requiring neurosurgery could be missed with the selective CT strategy. This still leaves the key question unanswered: how many patients can we afford to miss using a selective CT strategy? Can we afford to miss any at all or are the additional costs of scanning all patients with minor head injury justified compared with the gains in survival and quality-of-life?

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Head Rule and the New Orleans Criteria for CT scanning in patients with minor head injury. *JAMA*. 2005;294:1519–1525.

***The Authors' Reply:***

We would like to thank Dr. Smits and colleagues for their thoughtful observations regarding our report. Their interpretation of our findings leads us to believe that we were not sufficiently clear in reporting our results.

First, we observed no difference in our point estimate for the clinical effectiveness of scanning all patients with minor traumatic brain injury (TBI) and for selective use of computed tomography (CT; determined by application of the Canadian CT Head Rule [CCHR]). We reached this conclusion because to date there have been no published reports of missed surgical lesions (including point estimates for sensitivity of 1.0 in both articles cited by Dr. Smits and colleagues). Second, we observed no significant difference in the cost of any of the six strategies that we evaluated. Third, because of the extra benefit of the two scanning strategies and because there was no significant difference in cost, we concluded that the liberal use of CT scanning—by which we meant either scanning all patients or selective use of CT—in TBI was justified. We reiterated this conclusion both in the last sentence of our abstract and the concluding sentence of our article.

Our reading of Dr Smits' and colleagues' letter is that even had we been clear about our findings, they would still be concerned that we did not find the "Scan All" strategy to be cost effective compared with "Selective Scanning". Yes, they are correct that there is more uncertainty about the clinical effectiveness of CCHR than there is about the scan all strategy. Yes, despite the point estimate of 1.0 for sensitivity of the former rule, its use in only a small number of individuals with surgical lesions leads to a lower confidence limit in the published literature that is substantially less than 1.0. (In our own more recent unpublished review of almost 8,000 patients with minor TBI, we have found a point estimate for the sensitivity of CCHR of 99% with a lower confidence limit of 94%). But lack of certainty about one of the options doesn't translate into certainty that the other option is cost effective. It leads to the conclusion that we in fact drew: we cannot be certain that the two strategies differ in their cost effectiveness.

One could, of course, use models like ours to directly address the question Dr. Smits and colleagues pose: How many patients can we afford to miss? For example, if we knew with certainty that the sensitivity of the CCHR were 94% but recognized all of the remaining uncertainties in our model, the resulting confidence interval for the cost-effectiveness ratio for the Scan All strategy compared with Selective Scanning would continue to be undefined. In other words, even with this lower estimate of effectiveness for Selective Scanning, there is no willingness to pay for which we could be confident that Scan All is cost effective.

Our advocacy for liberal use of scans for mild TBI has been well known since the early 1990s,[1] and we would be happy to be able to report that we can be confident that the Scan All strategy represents good value compared with all other strategies. As we indicated in our article, we believe the current evidence base allows us to conclude that both liberal use strategies (i.e. Scan All or Selective Scanning with either the CCHR or other published scanning algorithms) are good value compared with the other four strategies that we reviewed. However, we do not think the current uncertainties in the evidence base allow us to differentiate confidently between the cost effectiveness of the two liberal use strategies.

Again, we thank Dr. Smits and colleagues both for their letter and for the opportunity they have provided us to clarify our results.

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***To the Editor:***

I read with interest the description of high-frequency oscillatory ventilation (HFOV) in trauma patients by Kao et al.[1] In the discussion of intraoperative use of HFOV for surgical and trauma patients, the authors comment that "more evidence is needed to confirm [HFOV's] safety".[1]

We have used HFOV in the operating room during excision and closure of burn wounds in a total of 45 patients with adult respiratory distress syndrome (ARDS) to date, and have reported our experience with this technique in the first 33 of these cases.[2,3] As reported,[2,3] HFOV was safely employed in the operating room, with no significant deterioration in the $PaO_2/FiO_2$ ratio or the oxygenation index, and with no respiratory or cardiovascular complications. About one-third of these operative procedures on HFOV were performed with the patient positioned prone.

This is not to say that continuation of HFOV from the burn intensive care unit to the operating room is a simple matter! In fact, this approach requires careful coordination between the burn surgeon, anesthesiologist, and respiratory therapist. Nevertheless, we believe that continuation of HFOV in the operating room is important for the maintenance of lung protection, and to avoid the lung derecruitment that can occur with temporary conversion back to conventional mechanical ventilation for a surgical procedure.

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***To the Editor:***

In 1996, a pilot study with respect to preventable prehospital trauma deaths in a Greek health care region was published in the *Journal of Trauma*.[1]

An editorial comment signed by A. Brent Eastman accompanied this publication. It was clear, constructive, and encouraged further research with respect to trauma-related mortality in our health care region. Did the editorial comment influence the research projects of the authors?

The medical profession in this country, as in many others, inspired by Hippocrates, can find convincing evidence from the international community that auditing our functions as doctors is an essential professional duty.

Under the auspices of the Acropolis and the ancient Greek philosophy and democracy, acceptance of the human rights of our patients with respect to optimal medical care is self explanatory and nowadays an essential demand of the society.

During the last 10 years, some changes have taken place in Greece starting from education, e.g., a wide participation of physicians and medical technicians in the Advanced Trauma Life Support for Doctors[2] and other related courses, as well as legislation for safer driving on the roads.

With respect to trauma-related research, large scale and substantial data have been published recently by this surgical unit,[3,4] as the editorial comment recommended 10 years ago. However, lack of funds threatens continuation and limits the expansion of trauma research projects.

Most importantly, transformations are slow and the impact of education and research on trauma-related mortality has not yet been measured. Mature concepts such as the function of trauma centers have not yet been designed, as the policy makers are not yet convinced of their value, and so funding for the upgrading of the trauma care system is restricted. For the time being, the medical profession should be the pioneers of evidence-based clinical practice and for achieving functional integration within the existing trauma care system.

Ten years ago the editors and the anonymous reviewers did not simply accept or reject an article but encouraged the authors to conduct further research.[1] They elicited positive research findings, did a constructive criticism of the article, and became participants in the multidisciplinary effort to reduce trauma-related mortality. Did the editors and reviewers become participants in the multidisciplinary effort to reduce trauma-related mortality?

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***To the Editor:***

With interest, we have read the article of De Pedro et al.[1] concerning the use of laparoscopic forceps for removal of broken intramedullary locking screws. We can confirm the value of this method and would like to draw the attention of the authors and the readers to a previous description of this technique in the *Journal of Trauma*.[2] We agree with De Pedro et al. that this complication of fatigue failure of nails and locking screws more frequently occurs in small diameter (unreamed) tibia nails. It is for this reason that we advise the use of the 5-mm instead of the 10-mm laparoscopic forceps for this extraordinary application, anticipating the narrow working canal in the tibia.

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***To the Editor:***

In the November 13, 2006 issue of *The Journal of Trauma* (publish ahead of print), Dr. Breyer and colleagues (*Endovascular Management of Trauma Related Renal Artery Thrombosis*) described the use of a stent to treat a traumatic right renal artery injury. The authors suggested that stent failure occurred because systemic anticoagulation could not be administered after stent insertion.

Figure 3 in the article demonstrated that the stent was located primarily within the uninjured main left renal artery with the distal stent edge located within the lower pole branch. There was restored flow to the midportion and lower pole branches. However, a large dissection extending throughout the midportion branch (arrows) was present. The cause of thrombosis was likely to be related to the persistent

traumatic dissection, rather than withholding anticoagulation.

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***To the Editor:***

We have read with great interest the article by Kurimoto et al.[1] The authors designed a study to evaluate the results of blind subxyphoid pericardiotomy (BSP) for cardiac tamponade because of acute hemopericardium and compare them with the results of percutaneous catheter drainage (PCD). The study populations included cases with cardiopulmonary arrest (CPA) on arrival (CPAOA) and CPA (or near CPA) after arrival (CPAAA) in the emergency department. There are some points worthy of discussion.

The patients' characteristics in Table 1 do not have the *p* value for comparison between the two groups, especially in the CPAOA/CPAAA column. The BSP group has more CPAAA patients (6 of 16 patients, 37.5%) compared with the PCD group (8 of 67 patients, 11.9%). The authors also did not detail the hemodynamic status of the 10 survivors before treatment. Aortic dissection is the major cause of cardiac tamponade in this study (54 of 67 patients in the PCD group and 11 of 16 patients in the BSP group). Isselbacher et al.[2] identified all patients presenting with acute aortic dissection and documented cardiac tamponade. The early mortality among those patients presenting hypotensive or normotensive was 43%; the mortality among those presenting with CPA was 100%. The survival rate is much higher in patients with vital signs before treatment. Bayegan et al.[3] found that patients with acute type A aortic dissection and signs of preoperative cardiac tamponade without palpable pulses are at high risk, up to 16-fold increased risk, of preoperative death, even when aggressive treatment with pericardiocentesis or surgical drainage is initiated. Therefore, the outcome in the group with more "near CPA" patients may be better than that with more "already dead" patients. Patients in the BSP group had higher survival rates than did those in the PCD group, and this may not be a result of the relieving methods of cardiac tamponade, but because of the proportion of CPAOA/CPAAA.

Furthermore, chest trauma with cardiac tamponade generally have a variable mortality rate, which ranges from 8% to 100%.[4–7] Perchinsky et al.[7] found that nonsurvivors in patients with blunt cardiac rupture tend to have more associated injuries, as indicated by higher Injury Severity Scores. The authors included the trauma patients to analyze the results, but they didn't describe the mechanisms of traumatic cardiac tamponade such as blunt or penetrating injury and whether other associated injuries coexisted. The interpretations of outcome and results are complicated and could easily be misleading. The effectiveness and ineffectiveness of percutaneous drainage is not defined clearly in this article. In our experience, the aspirates by the catheter drainage are often mixed with blood and clot that make the drainage intermittent and not smooth.

I was also curious about how the authors knew all of the causes of acute hemopericardium in these patients with cardiac tamponade. How many patients did undergo median sternotomy in the emergency department or operating room? How many patients did undergo autopsy after death?

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***The Authors' Reply:***

We appreciate the comments and questions of Wang et al. regarding our article on blunt subxiphoid pericardiotomy (BSP). We would also like to thank the many physicians who showed great interest in our article in various correspondences. Most questions we received were related to three points, regarding the details about the 10 survivors and the cardiopulmonary arrest after arrival (CPAAA) patients, and validation of the comparison between BSP and percutaneous catheter drainage (PCD). In our series, six patients successfully resuscitated by PCD survived, after which there was emergency operation for aortic dissection in four, for penetrating cardiac injury in one, and conservative treatment for blunt cardiac injury associated with severe brain injury in one. Also, four patients successfully resuscitated by BSP survived, after which emergency operations for aortic dissection were performed in two, pen-

etrating cardiac injury in one, and conservative treatment for cardiac rupture secondary to acute myocardial infarction (AMI) in one. Conservative treatments were selected considering severe brain injury and considerable postresuscitation encephalopathy. Two patients with cardiopulmonary arrest on arrival (CPAOA) because of AMI or aortic dissection were among those 10 survivors. There were 14 CPAAA patients with injuries consisting of aortic dissection in 9, penetrating cardiac injury in 3, blunt cardiac injury in 1, and cardiac rupture as a result of AMI in 1. Eight of these were initially resuscitated by PCD and six by BSP. It is obvious that CPAAA patients are more likely to survive than CPAOA patients. Therefore, we agree that there were some biases as Wang et al. pointed out, such as statistical significance in the number of CPAAA patients ($p = 0.023$), when comparing the two groups.

Our study design is not appropriate to determine an optimal method to relieve cardiac tamponade secondary to hemopericardium. However, what we wanted to emphasize in our article is that our modified subxiphoid pericardiotomy performed in a blind fashion was safe, quick, and effective, even in emergency patients with hemopericardium. At minimum, we propose that BSP must be considered for patients in which PCD fails to relieve cardiac tamponade, probably because of clotting in the pericardial sac. The effectiveness of BSP and PCD against cardiac tamponade was confirmed by cardiac echography. Diagnosis of hemopericardium was confirmed by BSP or PCD in 82 patients, except for 1 patient in whom PCD was not able to relieve cardiac tamponade and who died in the emergency room.

Lastly, we want to emphasize that there is still a chance to resuscitate even CPAOA patients with cardiac tamponade as a result of hemopericardium, as in the two patients described above. Although not in the present series of BSP, we previously reported one successfully treated patient with CPAOA as a result of aortic dissection who suffered cardiac arrest for 45 minutes.[1] It is our belief that once this BSP technique has been used, its usefulness in a critical situation becomes clear.

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