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In Reply:

We thank Dr. Khan *et al.* for providing a useful synopsis of recent relevant British and European literature, highlighting the work since 2007 of the National Hip Fracture Database in the United Kingdom. They reinforce the importance of prompt surgery and multidisciplinary team care for managing these complex, frail, elderly patients. We regret that the likelihood for useful data collection from the world's greatest per capita consumer of health care remains limited. There are no sizeable databases in the United States that focus on these patients. However, we are pleased that the American Academy of Orthopedic Surgeons has recently published an easy-to-read guideline on the management of hip fractures in the elderly that has been endorsed by eight U.S. Specialist Societies.¹ Interestingly, sections on perioperative pain management and anesthesia, seemingly completed without formal input from anesthesiology and associated specialist societies, provide a robust conclusion that “strong evidence supports similar outcomes for general or spinal anesthesia for patients undergoing hip fracture.”² We compliment our British colleagues on their diligence and multidisciplinary cooperation while concurring that this major, developed world, healthcare problem presents abundant opportunities for further research.

Competing Interests

The authors declare no competing interests.

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In Reply:

We thank Dr. Khan *et al.* for their comments regarding our recent Clinical Concepts and Commentary of the perioperative management of elderly patients with hip fracture¹ and for giving us the opportunity to extend the discussion.

Dr. Khan *et al.* are correct when indicating that mortality rates may be lower in countries such as Northern European countries, which have been at the forefront of care improvement for elderly patients with hip fracture and orthogeriatric concept.^{2,3} However, we do not agree with Dr. Khan *et al.* who consider that a randomized trials concerning early

surgery would be unethical. First, the facts are that, in most countries including ours, and in contrast to what is thought and/or recommended more than 50% of these patients are still operated later than 48 h. Second, such a trial will not compare early and late surgery (which we agree would be considered as unethical) but rather accelerated surgery *versus* standard of care, whatever is standard of care in the participating institutions, as suggested by the HIP ATTACK (HIP Fracture Accelerated Surgical Treatment And Care track) investigators.⁴ Third, we think that a randomized trial is necessary to definitely convince emergency physicians, anesthesiologists, and orthopedic surgeons that hip fracture is an urgent procedure in elderly patients, just because we must recognize that this is not yet the case everywhere.

Concerning perioperative score, we clearly need more efficient scores than the American Society of Anesthesiologist score, particularly when considering this aged, frailty, and comorbid population. The Nottingham Hip Fracture Score may work better than the American Society of Anesthesiologist score, but its discrimination is not very high (area under the receiving operating characteristic curve: 0.76) and its calibration not so good,⁵ and we think that Nottingham Hip Fracture Score may not appropriately assess all dimensions of the “preoperative characteristics” of these elderly patients, which include of course comorbidities but also frailty and previous walking capacity. Moreover, it should be pointed that other variables may play an important prognostic role such as the delay for surgery, perioperative hemodynamic stability, and the occurrence of perioperative complications, potentially limiting the efficiency of a preoperative score.

Concerning benzodiazepine withdrawal, we think that Dr. Khan *et al.* made a link with perioperative sedation, which was clearly not our intention. We only want to indicate that, particularly in France which is probably a good candidate for the World championship of psychotropic drug use in the general population,⁶ physicians must carefully ask the patient for possible chronic benzodiazepine administration (sometimes for years) because it may be responsible for withdrawal in the postoperative period with potential deleterious consequences (delirium, epileptic crisis with fall, *etc.*).

We fully agree with the suggestion by Dr. Khan *et al.* to promote close international collaboration to develop active research on this topic because our population is aging and hip fracture is frequent and has devastating consequences in elderly patients. More clinical research is clearly required for this major health problem.

Competing Interests

The authors declare no competing interests.

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Questions Regarding the Diagnosis of Malignant Hyperthermia

To the Editor:

Although we read with interest the recent article regarding the accuracy of malignant hyperthermia (MH) diagnoses in hospital discharge records,¹ we were concerned by aspects of the study’s methodology and data analysis. Two clinicians out of a panel of five assessed each coded MH diagnosis, and categorization was based on the agreement of two clinicians; in the event of a disagreement, a third clinician categorized the case to create a majority. It is unclear both why this method was used instead of the consensus-driven Delphi approach, given that expert opinion was the accepted standard, and why a κ statistic was not provided to demonstrate the strength of agreement between the two raters. In addition, although the study used the MH Clinical Grading Scale to standardize the diagnosis, no data on the calculated Clinical Grading Scale scores of the patients in the study were provided. These data would have added transparency and validity to the results of this study.