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. 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 that 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, 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, 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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References

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) diagnosis 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.
Furthermore, the study accepted any previously reported diagnosis of MH in the patient or family member as susceptibility for MH. The data of the study itself suggest that this is not valid, in that the results cast doubt on any reported family or patient history of MH. The combined number of incorrectly coded MH diagnoses categorized as “Fever unrelated to MH” or “Other [non-MH]” was greater than the number of incidents that the study found correctly coded for MH. Pinyavat et al. stated that they “did not consider personal and/or family history cases as coded in error,” but because only medical records for the index hospitalization of each patient were reviewed, it is impossible to determine whether the patient and/or family histories were actual incidents of MH. A true accepted standard exists to determine MH susceptibility, through caffeine–halothane contracture testing, as referenced in the study. There may be a number of patients who receive the MH International Classification of Diseases code without having an actual personal or family history due to relying on previously reported incidents of MH instead of the results of accepted standard testing. Thus, we feel that the published conclusion that 47% of the International Classification of Diseases–coded diagnoses referred to MH susceptibility is misleading because of the high rate of incidents with miscoded diagnoses and that the actual susceptibility could be less.

Competing Interests
The authors declare no competing interests.

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Reference

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In Reply:
We thank Campion et al. for their interest in our study examining the accuracy of malignant hyperthermia (MH) diagnoses in hospital discharge records1 and appreciate the opportunity to answer their questions. First, they ask why we chose the expert panel approach over the Delphi method for the medical record review. As described in our article, we used two expert panelists to independently review each medical record with a standard data abstraction form and the MH Clinical Grading Scale (CGS). When there was discrepancy between the two panelists, a third expert on site was consulted to reconcile the difference. Consensus on final CGS scores was reached in all cases without difficulty. The Delphi method is a consensus-building technique involving multiple rounds of polling a panel of experts, who usually remain anonymous to each other. The Delphi method was neither appropriate nor practical for use in our study, which was performed in six medical centers in compliance with privacy rules and the requirements of institutional review boards.

Second, Campion et al. wondered why we did not provide CGS scores. As a qualitative measure, the CGS is designed for ranking the likelihood that an adverse anesthetic event represents an incident of MH.2 The expert panelists’ assessment of the MH diagnoses (shown in table 1 of our article1) was based on the individual CGS scores along with other clinical characteristics. We did not present CGS scores because the number of MH incident cases (n = 11) included in our study was too small; providing individual characteristics for such a small sample would be problematic according to the conventional statistical reporting standards and personal privacy protection laws.

Third, Campion et al. question our decision to include MH susceptibility based on positive medical history in assessing the accuracy of MH diagnoses. Although we agree that the documentation of MH susceptibility in the medical record may not be entirely accurate, we do not agree that “the data of the study itself suggest that this is not valid.” Our study provides valuable data for assessing and interpreting MH diagnoses ascertained from hospital discharge records. It is noteworthy that diagnoses in hospital discharge records include both incident and prevalent cases. Therefore, it is necessary for us to consider both MH episodes occurring during the hospitalization and preexisting MH susceptibility in assessing the accuracy of MH diagnoses in hospital discharge records. The results of our study indicate that MH diagnosis listed in hospital discharge records has a positive predictive value of approximately 70%, which is similar to the findings regarding diagnoses of cerebrovascular disease and congestive heart failure.3

Finally, Campion et al. suggest that we should have used the caffeine–halothane contracture test, instead of medical record review, as the reference standard. Although we agree that the caffeine–halothane contracture test is widely regarded as the “accepted standard” for determining MH susceptibility, its specificity is less than 80%,4 and performing this test to confirm the diagnosis of MH susceptibility would not have been feasible for our study. We reviewed medical records for six institutions to retrospectively estimate the accuracy of coding for MH in hospital discharge records. The study proposed by Campion et al. could only be performed prospectively. Given the rarity of MH cases, such a study is unlikely to ever be performed.

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