Relevance of the Postoperative Quality Recovery Score to Discharge Readiness

To the Editor:

The study by Royse *et al.*[1] on the postoperative quality recovery score is interesting because it attempts to address the patient's perspective in some manner. Researchers have not paid much attention to the patient's opinion in the development of postoperative assessment tools. This may be important because, for instance, after laparoscopic cholecystectomy, most patients believed that they were not ready for discharge while nursing staff had the opposite view.[2] In a prospective study[3] of 194 outpatients, when patients determined their own discharge readiness, the time they felt discharge ready was significantly shorter than the actual time they were discharged based on nursing assessments. In light of these findings, we suggest that perhaps the patients' input should be considered with a view to improving the relevance of the postoperative quality recovery score. We believe that the patient's opinion is crucial and deserves a greater weighting in the "physiology domain" of the postoperative quality recovery score. In addition, we question the relevance of items that assess functional recovery from actual surgery as being too simplistic. The surgical literature has more sophisticated tools that are surgery specific (*e.g.*, the Constance score for shoulder surgery).[4] Perhaps consideration should be given to underweighting items that pertain to functional recovery from surgery.

Himat Vaghadia, F.R.C.P.C., Pamela Lennox, F.C.A.R.C.S.I., Peter Lee, F.C.A.R.C.S.I., M.D.* *University of British Columbia, Vancouver, British Columbia, Canada. peterjohnlee@hotmail.com

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In Reply:

We thank Vaghadia *et al.* for their correspondence regarding the Postoperative Quality of Recovery Scale (PQRS).[1] They suggested that the use of subjective patient opinion might be more heavily emphasized among the multiple domains assessed in the PQRS. Interestingly, and presumably in support of this position, they quote two articles regarding the value of patient opinion in determining fitness for discharge—with opposing results! In the first of these articles, patients who underwent cholecystectomy would have delayed their discharge relative to the assessment of healthcare professionals[2]; in the second, the outpatients would have been discharged earlier than the health professional assessment.[3] This underlies the problem of excessive reliance on patient opinion and is one of the reasons our group chose to go forward with objective measurements. It is well-known, for example, that patients will not detect the same level of cognitive dysfunction compared with objective neuropsychological tests.[4] This was echoed in our study,[1] in which the patient perspective of cognitive recovery was twice that of the objective measures. One of the difficulties with excessive reliance on patient response in this domain is the influence of other factors on judgment, such as the desire to be discharged and the possibility of patients not having clarity of thought.

We identified that most current recovery assessment tools are based on subjective opinion or subjective recall of past events. This was considered a weakness, and we believe that the use of objective testing over repeated periods is the strength of the PQRS. Another strength is our concept of recovery (*i.e.*, "return to baseline values or better"). All patients undergo testing before surgery, which allows objective assessment of when they return to presurgery levels of function. We did include a subjective assessment domain called the "overall patient perspective." This was included to allow better comparability with other recovery scales in the literature and questions aspects of return to function, satisfaction, and cognition. It only commenced from day 1 onward.

The PQRS is not just another "readiness for discharge tool" nor does it cater for specific surgery end points (*e.g.*, joint function or range of movement). If a particular operation, such as shoulder surgery, is *only* to be assessed for home readiness, then other published scales may offer a better solution, as Vaghadia *et al.* have commented. The strength of the PQRS is the objective assessment of multiple domains of recovery during the early, and late and long-term, periods. In the future, the PQRS may have broader application; at this stage, it is principally designed as a research or audit tool to assess how "what we do" affects the quality of recovery after anesthesia and surgery. It is far more complex than a simple tool to determine home readiness.

Colin F. Royse, M.D., B.S.,* Stanton Newman, D.Phil., David J. Wilkinson, M.B.B.S., F.R.C.A., on behalf of the PQRS Advisory Board. *University of Melbourne, Carlton, Australia. colin.royse@heartweb.com

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Intraoperative Ketamine and Chronic Opioid Use: Less Pain, More Morphine?

To the Editor:

The recent paper by Loftus *et al.*,[1] "Intraoperative ketamine reduces perioperative opiate consumption in opiate-dependent patients with chronic back pain undergoing back surgery," was of interest to me because I occasionally use the technique described. This group of patients is very complex and I congratulate the authors for undertaking this study.

There are four points that warrant clarification. First, the primary outcome of the study was based on data derived from a review of medical records (*i.e.*, mean [SD] 48-h postoperative oral morphine equivalent consumption of 500 [300] mg). However, the placebo group consumed only 309 (341) mg of this substance. Can the authors comment on this large difference and its possible relevance?

Second, the term "morphine equivalents" requires further explication. This terminology was confusing because it was applied to oral and intravenous formulations. Which formulation was used was not always immediately clear. For example, in their table 1,[1] were "median preoperative morphine equivalents" delivered orally or intravenously? The text implies that these equivalents are intravenous morphine. If this supposition is correct, then it appears to me that both groups of patients may be consuming more morphine equivalents at 6-week follow-up (data presented as mean [SD]) than they did preoperatively (data presented as median [interquartile range]). In fact, the placebo group appears to have much higher rates of morphine consumption at 6 weeks compared with their own preoperative consumption levels and the treatment group's consumption at 6-week follow-up. There is a possibility that the treatment group's 6-week follow-up consumption has also increased from the preoperative period, which is concerning. Can the authors clarify and comment on these points?

Third, the treatment group had more spinal levels operated on than did the control group. In fact, this difference reached statistical significance. However, this feature of the study was not addressed by the authors. Do the authors believe this difference was clinically significant?

Finally, with regard to these observations, specifically with respect to possible increases in morphine consumption among both groups at 6-week follow-up and the chronic nature of back pain, I believe that a more extended follow-up period is warranted. Do the authors plan to do this?

Richard D. Seigne, M.B.B.S., F.R.C.A., Christchurch Hospital, Christchurch, New Zealand. richard.seigne@cdhb.govt.nz

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In Reply:

The management of acute postsurgical pain in opiate-dependent patients is one of the most difficult clinical challenges in perioperative medicine. Although ketamine has been shown to be of value in some surgical settings for opiate-naïve patients, there is little information available pertaining to its use in opiate-dependent patients who need to undergo major and painful surgery.

Therefore, we designed our recent study[1] to determine the role, if any, of an easily implemented intraoperative ketamine protocol on postoperative recovery parameters. The study, if positive, was designed to be the beginning of an ongoing effort to define optimal treatment for surgical patients who suffer from chronic preoperative pain. As such, we appreciate the opportunity to clarify the issues raised by Dr. Seigne.

Dr. Seigne expressed concern and asked for clarification regarding the fact that the amount of opiate used in the 48-h postoperative period differed between the population reviewed in order to power the study and the actual study control group. The standard deviations for both groups are quite large. Thus, there was no statistically significant difference. Further, one would expect the groups to be different, given the intrinsic variability in the surgical population of interest.

Dr. Seigne was also concerned by the fact that the study patients, in both the treatment and placebo groups, appear to be using more pain medications at 6 weeks as compared to baseline, and that preoperative morphine use is presented as median (interquartile range) whereas postoperative use is reported as mean (SD). Preoperative morphine use is reported as median (interquartile range) because of the skewed nature of the data; this measure is a better reflection of the population. This was not the case for postoperative morphine use, however, making mean (SD) the more appropriate presentation. Results are reported in intravenous morphine equiva-