Reviewer’s report

Title: Determining Utility Values in Patients with Anterior Cruciate Ligament Tears using Clinical Scoring Systems

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Reviewer: Stefano Lupparelli

Reviewer’s report:

The authors have addressed an important topic, which is determining utility values in patients with an anterior cruciate ligament tear. Incorporating such information into decision-making would doubtless enhance our understanding of the impact an ACL tear may have on patients.

The following observations would improve the quality of their submitted article if the authors could consider a major compulsory revision.

BACKGROUND

1. The authors correctly report that a reasonable outcome following an ACL tear can be expected either by conservative or surgical treatment. This is indeed the case so much so that a distinction between “copers” and “non copers” has been made in the literature. However, it should be also clarified to the readers that the indications for surgery depends on a number of variables including patients’ level of activity and their willingness to modify it, response to rehabilitation, instability episodes despite activity modifications and rehabilitation. In other words, assigning patients with an ACL tear to either a conservative or surgical intervention is not based on an equivalent spectrum of symptoms and physical findings. An important point is also represented by the role a surgeon may play in indicating surgery versus a conservative treatment.

2. Information about the impact of an ACL tear on quality of life as assessed by the SF-36 is indeed reported in the literature. Admittedly, no information regarding utilities is available. However, assessing patients’ utilities is a slightly different concept also using different assessment tools and the readers would benefit from a concise explanation about what utilities are.

3. The authors succinctly detail the IKDC, Lysholm and Tegner score in this section. My suggestion is that the information relevant to these scoring systems might be more suitably placed in the “Methods” section.

4. The authors should clearly state both the design of the study and the null and test hypothesis of the study they have conducted.

METHODS

1. The authors use the term “survey” in the “Methods” section even if they have
employed the term study (admittedly without specifying the type of design) in the “Background” section. This should be corrected as already suggested. To my best knowledge, surveys are less likely to be associated with hypothesis testing. Conversely, it would seem that the underlying aim of the study might be to find out whether the IKDC, Lysholm and Tegner scoring systems correlate with patients’ utilities and whether these can discriminate the scenarios proposed by the authors.

2. My understanding of the study methodology is that four scenarios (or vignettes) characterizing different level of activities of patients with an ACL tear were created according to the method validated by Gottlob. Then a study sample of orthopaedic surgeons (used as proxies in lieu of real patients) were asked to fill in the IKDC, Lysholm and Tegner scores as well as the utility scores. The authors finally performed their correlation analyses, which is the main focus of the study. If my understanding is right, it would then be useful to give further information in this section about the number of orthopaedic surgeons constituting the study sample, whether they work in the same institution, whether they share the same indications for surgery or conservative treatment. Also, the authors state that “the opinions of the orthopaedic surgeons who had treated more than 100 ACL patients…”. It would be important to report whether these 100 ACL patients are treated monthly or annually, for example. Finally, the authors should report what is the percentage of surgeons treating more than 100 ACL patients and 300 patients within the study sample. It would also be interesting to know what criterion they used to weigh as double and triple the opinion of surgeons respectively operating more than 100 and 300 ACL patients.

3. As previously stated, a succinct but informative description of the scoring systems, including the Health Utility Index, used in the study should be incorporated in this section.

4. One important issue is whether the orthopaedic surgeons used as proxies regarded patients as described in the scenarios as patients capable of living with their impairment or whether they completed the scoring systems having a potential surgical candidates in mind. Not differentiating between the two types of patients risks introducing a selection bias: surgical candidates and non-surgical ones could be characterized by different utilities. In other words, using, for example, physiotherapists as proxies could yield totally different results.

5. It would seem that the authors used the same age (35 year-old) to set the four hypothetical patient scenarios. This would represent a major limitation to their analysis as it has been demonstrated that quality of life as measured by the SF-36 in ACL patients is affected by the patients’ age, gender and associated comorbidities. Could this apply to utilities as well? We actually do not know but, should this be the case, the findings of their studies could not be extrapolated to patients who are younger or older than 35 years of age.

6. I am not entirely sure that scoring data could be regarded as continuous values regardless of their distribution. I would have personally used non-parametric testing but I would recommend a statistician consultation.
RESULTS

1. The authors might wish to organize their “results” section in a way that avoids overlapping information. Specifically, the results of correlation of utility values are reported under the subparagraph “Utility values” and then again under the subparagraph “Correlations of knee scores and utility values”.

2. Two sets of graphs relevant to Figure 1 and Figure 2 were supplied with the manuscript, which do not seem to differ aside from the layout. The authors might wish to supply only one set of graphs to avoid redundancy.

DISCUSSION

1. There would seem to be some redundancy at the beginning of this section as the authors have repeated some of the findings reported in the “Results” section. There is little discussion of the clinical meaningfulness regarding their observations, for example what they refer to as “difference in gain” of utility values among different vignettes.

2. The potential (selection) bias and confounders (age, gender, comorbidity) should be better highlighted in this section.

In conclusion, I have found two major limitations:

1. The lack of a study sample including actual patients risks introducing an uncontrollable selection bias that could flaw the study

2. The four hypothetical scenarios are limited to a fixed age that is not truly representative of the wide spectrum of patients encountered in real practice

Hence, I would strongly encourage the authors to perform a compulsory major revision including a cohort of patients that could be contrasted with the findings obtained by the authors using orthopaedic surgeons

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests: I declare that I have no competing interests