

# The Cost-effectiveness of Anterior Cruciate Ligament Reconstruction in Competitive Athletes: Letter to the Editor

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## Dear Editor:

We have, with great interest, read the report from Stewart et al<sup>4</sup> regarding the cost-effectiveness of anterior cruciate ligament (ACL) reconstruction in competitive athletes. We congratulate the authors for a clear description of their methodology and the selection of included studies but have concerns regarding the potential for the nonrepresentative selection of nonreconstructed patients in the study.

Firstly, in the 2-year report of the KANON trial,<sup>1</sup> a randomized clinical trial (RCT) of rehabilitation plus early ACL reconstruction or rehabilitation plus optional delayed ACL reconstruction after an acute ACL injury, Table 3 provides data on return to preinjury levels of sports for *both* arms. In the study by Stewart et al,<sup>4</sup> however, only data for the early ACL reconstruction arm were included. We cannot find any information on why only 50% of the results from the only available RCTs on treatment were excluded from the analysis.

Although not clearly stated in the article, we speculate that the exclusion of results from the rehabilitation plus optional delayed ACL reconstruction arm could be because 23 of 59 patients (39%) underwent delayed ACL reconstruction over the 2-year follow-up period. However, results according to the treatment received are available in the supplementary Appendix, which can be accessed from *The New England Journal of Medicine* homepage. From the Appendix, it is evident that 14 of 36 (39%) participants remaining in the rehabilitation alone group returned to their preinjury level of sports.

Secondly, we note that the 3-year follow-up results from a prospective cohort of 100 ACL-injured active patients treated nonsurgically were not included.<sup>2</sup> In that study from Kostogiannis et al,<sup>2</sup> 40 of the 67 (60%) nonreconstructed patients returned to their preinjury activity level or higher after 3 years, and an additional 8 patients (12%) reported a 1-step decrease in their Tegner activity level (ie, return to sports at almost the same level).

Thirdly, we find it interesting that a 10-year report on a matched-pair analysis of high-level athletes was not discussed.<sup>3</sup> In that study, there was no difference between the highest achieved postinjury activity level of the athletes treated nonsurgically (median Tegner score of 7 [range, 4-10]) or with ACL reconstruction (median Tegner score of 8 [range, 3-10]).

In summary, we are concerned that selection bias may have impacted the validity of the conclusion from the

Stewart et al<sup>4</sup> report because (1) the nonsurgical sample for comparison was only 5% of the ACL-reconstructed sample (n = 147 vs n = 2719, respectively) and (2) the mean rate of return to preinjury levels of sports of the included non-ACL-reconstructed sample was 17.1%, whereas the respective mean rate was 50% in the studies that, for unclear reason(s), were not fully included.<sup>1,2</sup>

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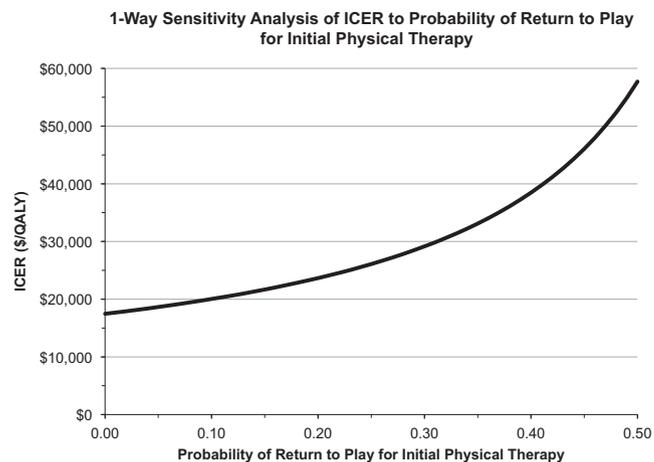
# The Cost-effectiveness of Anterior Cruciate Ligament Reconstruction in Competitive Athletes: Response

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## Authors' Response:

We appreciate the concerns brought up in the letter to the editor regarding our article on the cost-effectiveness of anterior cruciate ligament (ACL) reconstruction in competitive athletes.<sup>4</sup> The authors of the letter appear concerned that the return-to-play rate used for the nonoperative group in our study is low and thus impacts the validity of our conclusions. Because the values obtained from the literature and utilized in the model can be debated, we ran a sensitivity analysis, which showed that the incremental cost-effectiveness ratio (ICER) is relatively insensitive to the probability of returning to play.

Regarding the study by Frobell et al,<sup>1</sup> it would be misleading to state that 14 of 36 patients who were treated



**Figure 1.** Sensitivity analysis of ICER versus probability of return to play for initial physical therapy.

nonoperatively returned to the same level of play when in fact 23 patients in the initial rehabilitation group of 59 patients required ACL reconstruction. Instead, one could argue that the return-to-play rate for this group was actually 14 of 59 (23.7%) using a nonoperative treatment strategy. This value falls within our sensitivity analysis parameters.

In the study by Kostogiannis et al,<sup>2</sup> patients were advised to modify their activity level and avoid contact sports, particularly soccer, basketball, and handball. The Tegner scale does not fully differentiate between contact and noncontact sports, and thus, although patients may have reached a similar Tegner score, they may have never returned to the same level of play as before the injury. In fact, the authors of this study state, “We can assume that most of the patients indeed followed our advice to avoid contact sports.” Thus, this study was not included in our model.

Finally, the study by Meuffels et al<sup>3</sup> was not included because the authors failed to specify the percentage of athletes who returned to play at the same level.

Our study does not discount the idea that certain athletes may be able to return to the same level of play after nonoperative treatment for an ACL rupture. However, the available literature shows that a much larger proportion of athletes return to the same level of play with ACL reconstruction than with nonoperative treatment. Given

this, our study shows that ACL reconstruction as an initial treatment is a cost-effective strategy for athletes. Although the probability of return to play after nonoperative treatment can be debated, our sensitivity analysis shows that our model is relatively insensitive to this probability. Figure 1 shows a sensitivity analysis of the ICER versus the probability of return to play for nonoperative treatment. This figure demonstrates that ACL reconstruction remains cost-effective as an initial treatment strategy (ICER <\$50,000/quality-adjusted life year) over a wide range of return-to-play probabilities for nonoperative treatment. Thus, our conclusions remain valid despite the concerns presented in the letter to the editor.

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