Author's response to reviews

Title: Knee complaints seen in general practice: athletes versus non-athletes

Authors:

Marienke van Middelkoop (m.vanmiddelkoop@erasusmc.nl)
Robbart LI van Linschoten (r.linschoten@erasusmc.nl)
Marjolein Berger (m.berger@erasusmc.nl)
Bart W Koes (b.koes@erasusmc.nl)
Sita MA Bierma-Zeinstra (s.bierma-zeinstra@erasusmc.nl)

Version: 2 Date: 18 January 2008

Author's response to reviews: see over
Dear editor,

We thank you and the reviewers for the constructive comments on our manuscript “Knee complaints seen in general practice: active sport participants versus non-sport participants”

We will discuss the remarks of the reviewers below.

Reviewer's report 1
Title: Knee complaints seen in general practice: athletes versus non-athletes
Version: 1 Date: 17 December 2007
Reviewer: Elaine Thomas
Reviewer's report:

Major compulsory revisions
Introduction
1. The authors do not present a convincing argument for the need for this study. Although they state in a number of places that it is of interest to study the differences between athletes and non-athletes, they do not state why this is interesting. For example, would it be beneficial to treat the two groups separately if differences were found between diagnosis, prognosis or GPs initial policy of treatment?

Reply 1:
We agree with the reviewer and added the following text to the introduction, page 3: “These differences could have implications for applied treatment strategies of these knee complaints, i.e. it might be beneficial to treat the athletes different than the non-athletes because of a different diagnosis. Further, it is of interest to explore differences between athletes and non-athletes regarding the GP’s initial treatment, medical consumption and prognosis of the two groups. If the medical consumption appears to be the only difference between athletes and non-athletes we will need to reflect on the implications of such difference.”

Discussion
2. This section is a rewording of the results section. There is no discussion of the implications of the research and only one reference to other work (which relates to the dropouts rather than the main study findings). There is some mention of limitations but this could be expanded upon.

Reply 2:
We agree with the reviewer that there is less discussion about the implications of the research findings. Therefore we added the following text: Page 9, second paragraph: “This implies that there is no specific knee complaint that can be associated with sport participation.”
Page 9, third paragraph: “Therefore, future research should not only focus on traumatic knee injuries in athletes, but also on non-traumatic injuries.”
Page 10, second paragraph: “In this context it must be mentioned that there can be an overlap between the different treatment strategies, i.e. one patient could receive more than one advice and/or treatment at their first consultation at the GP. However, athletes were more often advised to ‘go easy on the knee’ than the non-athletes; this is probable related to the physical activity level of the athletes or to the type of knee complaints.”
Page 11, first sentence: “The role of the GP in this relationship remains unknown, i.e. it is unknown if the GP is aware of the physical activity level of the individual patient at consultation.”
Page 11, last sentence first paragraph: “Therefore, this study does not give any indications for the GP to inform athletes different than non-athletes regarding the prognosis of their knee complaints.”
Page 11, second paragraph: “Finally, we did not find any substantial differences in the diagnosis and prognosis of the knee complaints between athletes and non-athletes but we did find a
difference in medical consumption between the athletes and non-athletes. Apparently athletes do prefer a more active strategy compared to non-athletes. However, the exact reason for this higher medical consumption remains unknown.”

Conclusions

3. This section in particular needs to reflect on the implications for practice. The results suggest that there is no need for policy for GPs to treat athletes and non-athletes differently as they are very similar. Is this what the author would conclude?

Reply 3:
We agree with the reviewer and changed the text of the conclusion: “The results of this study indicate that there are no major differences in diagnosis and prognosis of knee complaints between athletes and non-athletes presented to the GP. This implies that there are no indications for different treatment strategies applied in both groups. Though, athletes are more often advised to ‘go easy on the knee’ and to rest than the non-athletes. However, this advice might be related to the physical activity level of the patients. Further, there is a trend towards increased medical consumption among athletes while the functional disability scores are higher and the pain scores are lower than among the non-athletes.”

Figures: Figure 1

4. The percentages presented in the last two boxes, labeled total recovery, are calculated including those lost to follow-up in the denominator. Hence, you present a total recovery of 54.9% for the athletes which I assume is calculated as 231/421. However, 35 people were lost to follow-up, so should the total recovery percentage not be 231 / (421-35) = 59.8%? The authors need to clarify whether they have assumed that those lost to follow-up did not recover, in which case, they need to justify this and explain whether similar assumptions have been made in the other analyses in this paper, or they need to amend these results accordingly.

Reply 4:
We agree with the reviewer that we made a mistake in the calculation of the percentage recovered athletes and non-athletes. This mistake was only made in the calculation of the percentages and has therefore no influence on the results of the analyses made in this article. For the analyses on the course and prognosis we analyzed only the subjects with follow-up data. The numbers are changed in the text and removed from figure 1 (see reply 40 reviewer one).

Minor essential revisions

Title

5. I would suggest that the term athlete is not appropriate for this study. This term is usually used to relate to professional sports people, rather than those who partake in sport in their leisure time. It seems that the authors are interested in differences between those who are physically active and those who are not. A term such as physically active or regular exercisers would be more appropriate than athlete.

Reply 5:
We agree with the reviewer and changed the title of the article in: “Knee complaints in general practice: active sport-participants versus non-sport participants”. Further, we changed the text in the methods section: “From this total cohort population we extracted patients who were active sport participants, defined as athletes (n=421) or non-sport participants, defined as non-athletes (n=388).”

Abstract

6. The conclusions are the results presented in a slightly different form. The authors should use the results to draw conclusions about the implications of their study for practice and/or future research.

Reply 6:
We agree with the author and made some adaptations in the abstract, page 2, results and conclusion sections: “The results showed that acute distortions of the knee were significantly more diagnosed in athletes than in non-athletes (p=0.04). Further, more athletes were advised by their GP to ‘go easy on the knee’ than the non-athletes (p<0.01), but no differences were found in number of referrals and medication prescribed by the GP. The medical consumption was significantly higher among athletes; however, no significant differences were found between the two groups for recovery at one-year follow-up. Conclusions: There are no major differences in the diagnosis and prognosis of knee complaints between athletes and non-athletes presented to the GP. This implies that there are no indications for different treatment strategies applied in both groups. However, athletes are more often advised to ‘go easy on the knee’ and to rest than non-athletes. Further, there is a trend towards increased medical consumption among athletes while functional disability and pain are lower than among the non-athletes.”

Methods
Study Design
7. How did GPs define knee consultations? Was this using a code, free text or just their feeling about the consultation? Also, were GP consultations taking the same format that they would usually take?

Reply 7:
As already mentioned in the text, the GPs noted the working diagnoses of the knee disorders according to the International Classification of Primary Care. This means that all knee complaints were coded in the same format as GPs usually do at a consultation. Further, the consultations were taken in the same format as they usually take. To clarify this we added the following text to the methods section, study design: “The consultations were taken in the same format as they usually take.”

8. In general, were GPs aware of whether a patient was an athlete or not, either by the definition used in this study or by some other definition?

Reply 8:
In the methods we described that the data used in this study were extracted from a cohort study with 1068 subjects. Unfortunately, this cohort study was not primary designed to distinguish athletes and non-athletes. As mentioned in the methods, we defined athletes and non-athletes following sport activities mentioned in the questionnaire. So the GP was not involved in the definition of an athlete. Therefore we do not know if the GP is aware of whether the patient is an athlete or non-athlete. To clarify this we added the following text to the discussion section, page 10: “The role of the GP in this relationship remains unknown, i.e. it is unknown if the GP is aware of the physical activity level of the individual patient at consultation.”

9. Line 17: I do not understand the word anamnesis. Please could the author use a term which is more widely used?

Reply 9:
We agree with the reviewer and changed anamnesis in ‘history taking’.

Outcome measures
10. Line 5 The authors refer to the WOMAC functional disability score as being a global score. Perhaps the description would be better without the term global

Reply 10:
We agree with the reviewer and removed the term global.

Potential confounders
11. Everything discussed in this section is also mentioned in the Statistical Analysis section. This section could therefore be removed.
**Reply 11:**
We agree with the reviewer and removed this section.

**Statistical Analyses**
12. This section could be written more concisely, which would be easier for the reader to follow. I suggest something of the form: Logistic regression analyses were used to test the association between athletic status and i) the type of knee complaint, ii) initial policy of the GP, iii) medical consumption, iv) patient satisfaction with treatment received, v) recovery at one-year follow-up, vi) discomfort during employment, vii) discomfort during daily activities. All of these analyses were adjusted for age, gender and BMI. In addition, models ii, iii, iv, vi and vii were adjusted for trauma and baseline severity (measured by the WOMAC). Models vi and vii were also adjusted for the appropriate baseline discomfort score. Linear regression was used to test the association between athletic status and the pain and function, as measured by the WOMAC. These analyses were adjusted for age, gender, BMI, trauma, baseline severity and baseline pain level. However, from the current explanation, I am not clear as to what adjustments were made in the pain and function analyses. This needs to be clearer.

**Reply 12:**
We agree with the reviewer and changed the text in the statistical analyses section: “Descriptive statistics were used to characterize demographic information, and chi-square and t-tests were applied to test the baseline differences for age, gender, BMI, WOMAC score and pain. Logistic regression analyses were used to test the association between athletic status and i) the type of knee complaint, ii) initial policy of the GP, iii) medical consumption, iv) patient satisfaction with treatment received, v) recovery at one-year follow-up, and, vi) discomfort during employment and daily activities.

All of these analyses were adjusted for age, gender and BMI. In addition, models ii, iii, iv, v and vi were adjusted for trauma and baseline severity (measured by the WOMAC). Model vi was also adjusted for the appropriate baseline discomfort score. Linear regression was used to test the association between athletic status and pain and function, as measured by the WOMAC. These analyses were adjusted for the potential confounders age, gender, BMI, trauma and baseline severity (WOMAC). The analyses for pain and function (WOMAC) were also adjusted for appropriate baseline pain and function scores, respectively.”

13. In the final paragraph, the authors need to add that the results of the linear regression analyses are presented as, for example, adjusted means. They cannot be presented as odds ratios, as this paragraph currently suggests, as it is not possible to obtain odds ratios from linear regression.

**Reply 13:**
That is correct. However, we only present the p-values of the linear regression analyses and therefore it is not necessary to add this sentence to this paragraph. However, we changed the sentence in the last paragraph into: “The results of the logistic regression analyses are presented as odds ratios (ORs), with 95% confidence intervals (CI).”

**Results**
14. The use of the ± symbol in relation to standard deviations is inappropriate and an outdated presentation style. This should be changed. For example, instead of showing the mean age of the study population as 45.3 ± 16.9 years, this should be shown as 45.3 (16.9) years, where it is clarified previously that values will be shown as mean (SD).

**Reply 14:**
We agree with the reviewer and changed the presentation of the means and standard definitions in the results section.
15. The use of p-values in the text is not particularly helpful to the reader. P-values are to a large degree dependent on sample size and so do not give an idea of the magnitude of the relationship. It is more informative to show odds ratios. Although the odds ratios are already shown in the Tables, it would be useful to present them, either with or without their corresponding confidence intervals, in the text.

Reply 15:
We agree with the author that a p-value does not give an idea of the magnitude of the relationship. However, most p-values mentioned in the text are accompanied by means or frequencies of the comparison groups. So to our opinion odds ratios in the text will not clarify the magnitude of the relationships. However, the editor can decide whether these odds ratio’s and confidence intervals should be mentioned in the results section or not.

Study Population
16. Page 7, line 3. This sentence reads as though the two groups presented are the dropouts and non-dropouts, when in fact the authors are referring to the athletes and non-athletes. This needs to be made clearer.

Reply 16:
Apparently it is clear that we do indeed refer to dropouts and non-dropouts and not to athletes and non-athletes in the first paragraph of this section. This is a comparison of dropouts and non-dropouts before the comparison of athletes and non-athletes starts.

17. Page 7, line 8. WOMAC is written as Womac. This needs to be changed to WOMAC.

Reply 17:
Womac has been changed into WOMAC.

18. Differences between study completers and dropouts are more usually presented as percentage differences, for categorical data, and mean differences, for numerical data. The use of odds ratios here is not particularly helpful as it suggests that the odds of dropping out in those with less pain are higher than the odds of dropping out in those with more pain. What the reader really wants to know is the whether the same proportion of those with and without pain dropped out.

Reply 18:
We agree with the reviewer that the odds ratios are not helpful in this section. Therefore we removed the odds ratios and confidence intervals and inserted a mean difference for the pain score. Text changed: “The pain score at baseline of the dropouts was significantly lower compared to the pain score of the non-dropouts (mean difference 0.69)”

Course and prognosis
19. On page 5, the authors set up the term clinically relevant improvement and define it as those who report total recovery or major improvement on the experienced recovery variable. However, in later parts of the manuscript they use other terminology, e.g. in the Statistical analyses section they use recovery, in the Course and prognosis section and Figure 1 they use total recovery. This is confusing as I am not sure whether they mean the defined clinically relevant improvement or actually only those reporting total recovery. Consistency in terminology is needed.

Reply 19:
We agree with the reviewer that this is a bit of confusing. Therefore we added the following text into the paragraph on page 5. “The categories 'total recovery' and 'major improvement' represent a clinically relevant improvement and are in this study defined as being recovered.”
20. P-values are not 0.000, statistically they are <0.001. This needs to be corrected.

Reply 20:
We agree and this is changed in the table.

21. All figures should be given to the same number of decimal places and % signs are not needed within the body of the Table as they are given in the header. For example, in the Athletes/Gender cell, the values should read 58.0, not 58%.

Reply 21:
We agree with the reviewer and changed the values in the table.

22. The type of knee complaints seems to be rather confusingly categorised. People can clearly fall into two or more of the displayed groups. For example, they could have bilateral acute distortion caused by trauma. I suggest that this part of the Table is split into sections (Cause: trauma; Description of problem: Bilateral, recurrent; Diagnosis: general knee complain to chronic meniscus fracture). Alternatively, the first three rows could be removed, as they do not really represent working diagnoses. If these concepts remain in the Table, further explanation is needed of the term recurrent, for example in respect of osteoarthritis.

Reply 22:
We agree that this table could be more clarified by splitting it into 2 sections. Therefore we applied some changes in the table.

Table 2
23. The % sign could be put in the first row of the Table i.e. Athletes (%), Non-athletes (%), rather than next to every value in the Table. This would make it easier to read.

Reply 23:
We think that this will not make it easier to read the table. However, the editor can decide different on this subject.

24. Is there any overlap between treatment strategies here? For example, can patients be advised to reduce weight and given medication? This should be mentioned.

Reply 24:
Yes, it is possible that there is an overlap in treatment strategies. The GP can give one patient more than one advice and/or treatment. To clarify this we added the following sentence “In this context it must be mentioned that there can be an overlap between the different treatment strategies, i.e. one patient could receive more than one advice and/or treatment at their first consultation at the GP” to the Discussion section, page 10.

25. It is not clear what is meant by the term Save the knee. Is this taken from Dutch guidelines, or is this a term chosen by the authors? This needs to be clearer. I am also unsure as to whether compresses belong in the same category as the other three treatments under Save the knee.

Reply 25:
We agree with the reviewer and changed ‘save the knee’ in passive strategy.

26. The column of p-values is not needed because the confidence intervals describe the significance of the association.

Reply 26:
It is indeed possible to remove the p-value column, however to our opinion it can give the readers a quick overview of the results. So we argue not to remove the p-value column.
27. Footnote. Womac needs to be re-written as WOMAC.

Reply 27:
Text has been changed.

Table 3
28. The header for this Table says the medical consumption is over the one-year follow-up. This needs to be made clear in the Statistical Analysis section as medical consumption is compared as part of the initial policy as well as data over the 12-month follow-up period.

Reply 28:
The medical consumption is indeed expressed over one-year follow-up. To make this more clear we added the following sentence to the Methods section, page 5: “The medical consumption of the patients, expressed in frequency of visits, was calculated over the 12 months follow-up period.”

29. The % sign could be put in the first row of the Table i.e. Athletes (%), Non-athletes (%), rather than next to every value in the Table. This would make it easier to read.

Reply 29:
See reply 23

30. It might be better to present the types of therapists or specialists as physiotherapist, orthopaedic surgeon, other, because of the small numbers.

Reply 30:
It is indeed possible to present the table in a sorter form. However, because table 3 isn’t a large table and the small numbers do give us more insight in the type of therapist and specialist visited by the patients we prefer not to change this table.

31. As in Table 2, p-values are not needed.

Reply 31:
See reply 26

Figures
Figure 1
32. The final two boxes of this flow chart are labeled total recovery. This does not fit with the definition given on Page 5. See Comment 20.

Reply 32:
See reply 20. However, we removed that boxes from figure one (see reply 40 on reviewer one).

Figure 2
33. This Figure shows pain and function scores at three-month intervals throughout the one-year follow-up. This needs to be clearer in the text, which suggests that Figure 2 only shows the results at one year.

Reply 33:
We agree with the reviewer that the presentation of figure 2 might not be completely clear. Therefore we changed the text, results section, course and prognosis: “Figure 2 shows the unadjusted mean pain and WOMAC scores at three-month intervals throughout one-year follow-up.”

34. Could the authors make it clearer what is shown in this Figure? Is it the adjusted means from the linear regression analysis or is it unadjusted means?
Reply 34:
See reply 33.

Discretionary revisions

Methods

Outcome Measures

35. The description of the WOMAC function scale is a little confusing as the authors say that the scale measures functional disability of the knee and then say the score ranges from 0 (poor) to 100 (excellent). This reads as though disability is excellent and I think the word disability should be changed to ability.

Reply 35:
To our knowledge is the description of the WOMAC function scale we used a widely accepted definition. So we decided not to change this description.

36. This section details some measures that were collected in the study, but not used in the analyses presented in this paper. It would be simpler not to mention these measures (e.g. satisfaction with GPs policy at one-year follow-up).

Reply 36:
Satisfaction of GPs policy is described in the results section, GPs initial policy and medical consumption, last paragraph. So the description of these measures in the methods section will not be removed.

Study Population

37. In line 6 of this section, it should read, satisfaction with the GPs

Reply 37:
Text has been changed.

38. In line 7, the sentence would make more sense if the word registered were changed to recorded. GPs initial policy and medical consumption

Reply 38:
To our opinion the sentence would not make more sense if the word registered is changed and because of this reason we decided not to change this sentence.

39. Page 8, line 14-15. Should this read, Most patients visited a physiotherapist (30%) or an orthopaedic surgeon (19%), rather than and an orthopaedic surgeon?

Reply 39:
Text has been changed.

Figures

Figure 1

40. If those lost to follow-up have been removed from the analysis, it may be more appropriate to add in a box to show the total number eligible for analysis. The authors may want to consider removing the results, i.e. the percentages recovered, from this as it is not clear that this is the primary outcome and that it deserves a more prominent position in the paper than other results.

Reply 40:
We agree that is more appropriate to add a box with the number of eligible patients and to remove the box with recovered patients. Therefore we adjusted figure 1.

What next?: Unable to decide on acceptance or rejection until the authors have
responded to the major compulsory revisions

**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, and I have assessed the statistics in my report.

**Declaration of competing interests:**
I declare that I have no competing interests
Reviewer’s report 2
Title: Knee complaints seen in general practice: athletes versus non-athletes
Version: 1 Date: 17 December 2007
Reviewer: Stephen Brealey
Reviewer’s report:
General
This study recognizes that complaints of the lower extremities are a common problem and so is designed to investigate differences in knee complaints between athletes and non-athletes presenting in general practice.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)
None.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. The study questions are well defined and clearly presented. The study design is appropriate and adequately described and appears to be conducted on quite a large scale and thus has adequate generalisability. There seems to be an adequate definition and distinction between athletes and non-athletes. I am not sure the WOMAC was the best instrument of choice as it is designed as an osteoarthritis index when the anticipated injuries are those of the anterior cruciate ligament and meniscus lesions. Is it possible to include a reference as to how the WOMAC has been validated in sports injury patients?

Reply 1:
The WOMAC osteoarthritis index was indeed developed for patients with hip or knee osteoarthritis. Since the WOMAC index focuses on daily activities, research has been carried out to assess the validity of the WOMAC index in other patient groups. Unpublished data of our research group showed that the WOMAC content and construct validity is adequate and that there is good responsiveness in adolescents and young adults with knee complaints in general practice. Further, it is known that the KOOS based on the WOMAC, is widely used to evaluate subjective outcome in anterior cruciate ligament (ACL) reconstructed patients. Further, the Lysholm score was also included in the questionnaires in this study. The Lysholm score was developed to determine the functional status of patients with ACL injuries of the knee, but is also validated for patellofemoral pain syndrome and meniscal injuries and various other chondral lesions of both traumatic and degenerative nature. However, we did not find any differences between the WOMAC and Lysholm scores in this study. Because of this reason we made the arbitrary decision only to include the WOMAC score in this study.

2. Analyses are controlled for confounders. However, why was logistic regression used to examine difference in type of knee complaints, and why was this analysis adjusted for age, gender, and BMI? It seems to me, particularly as you report the results of this analysis in Table 1, that you are only presenting these differences as baseline characteristics. Age, for example, might influence type of knee complaint between the two groups (such as osteoarthritis), but why adjust for it?

Reply 2:
Because of the baseline differences in age, gender and BMI we decided to adjust all the analyses for these variables, including type of knee complaints. It is indeed true that these variables might influence type of knee complaints. This is exactly the reason why we adjusted the analyses. If we would not adjust the analyses we would find a significant difference for osteoarthritis between athletes and non-athletes, however this difference is not attributed to being an athlete or not but is attributed to the difference in age. By adjusting the analyses, the p-values give a more representative value for the differences between athletes and non-athletes, attributed to being an athlete or not.
3. I also don’t understand why in a regression model you would have type of knee complaint predicting whether a patient is an athlete or not. I would have thought chi-square tests of differences between the two groups for each knee complaint would be sufficient.

Reply 3:
As described in reply 2 we adjusted all analyses for the baseline differences in age, gender and BMI. By means of the Chi-square test we can not adjust the analyses for potential confounders and because of this reason we applied logistic regression analyses.

4. The Statistical analyses section could also be improved for making clearer what is the dependent variable for each analyses. Why was linear regression analyses used to test pain and function (WOMAC) at one-year follow-up?

Reply 4:
We do not think it is necessary to explain why a linear regression analysis is applied in combination with continuous variables instead of a logistic regression analysis for dichotomous variables. However, we improved the statistical analyses section as already mentioned by reviewer one, reply 12.

5. Table 1 should define BMI as kg/m2.

Reply 5:
Text has been changed.

6. For Table 3, why was the analysis adjusted for revisiting the GP? This was not specified in the Statistical analyses section.

Reply 6:
The analysis as it is represented in table 3 was not adjusted for revisiting the GP. However, as described in the discussion, in the Dutch healthcare system patients generally have to visit their GP before being referred to a therapist or specialist. Consequently, we found a strong relationship between revisiting the GP and medical consumption (p<0.001). Therefore, we repeated the analysis for medical consumption (therapist or specialist) with adjustment for revisiting the GP.

This is an additional analysis and is therefore not described in the statistical analyses section of the methods.

7. Does Figure 2 include 95% confidence intervals as specified in the title? May be the font size of the mean point estimates can be reduced so that the confidence intervals can be seen.

Reply 7:
Yes, figure 2 does include 95% confidence intervals as specified in the title. It makes no difference if we reduce the font size of the mean estimates because the 95%-CI s are very small. Further, if we reduce the size of the mean estimates they will not be well visible.

8. The discussion and conclusions are adequately supported by the data, however I would like to see the use of the word significant used in the conclusions to more accurately describe the findings. For example, the conclusions should read Athletes are (significantly) more often advised to go easy on the knee and to rest than non-athletes. Moreover, is it more accurate to state that Athletes are (significantly) more often advised to save the knee than non-athletes? The analyses reported appear to have been undertaken for the policy Save the knee and not for individual elements within it.

Reply 8:
The conclusion section will be rewritten, as already mentioned at reply 2 and 3 from reviewer one.
9. The title does not convey what was found but adequately describes the study.

Reply 9:
That is true and we decided to describe the study in the title rather than to give a description of the results of the study.

10. The abstract could be improved as described above with the appropriate use of describing what findings were significant.

Reply 10:
We agree with the reviewer and made some adaptations in the text of the abstract, results section: “The results showed that acute distortions of the knee were significantly more diagnosed in athletes than in non-athletes (p=0.04). Further, more athletes were advised by their GP to ‘go easy on the knee’ than the non-athletes (p<0.01), but no differences were found in number of referrals and medication prescribed by the GP. The medical consumption was significantly higher among athletes; however, no significant differences were found between the two groups for recovery at one-year follow-up.”

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
None.

What next?: Accept after minor essential revisions
Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
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.
Reviewer’s report
Title: Knee complaints seen in general practice: athletes versus non-athletes
Version: 1 Date: 5 December 2007
Reviewer: Olivier Bruyere

Reviewer’s report:
The objective of this study was to investigate the differences in type of knee complaints, medical consumption and outcomes between athletes and non-athletes. This is a well-designed, interesting study.

Major Compulsory Revisions
- Abstract: I believe that the most interesting results of this study are not highlighted. As a matter of fact, the authors focus mainly on the significant results. However, from my point of view, the fact that there are no significant differences of active strategy, medication or referrals to care givers between the two groups is also very important and interesting. The same remark is also true for functional disability or the medical consumption at one year. I would suggest to reduce the part of the conclusion section and to add more results.
- Abstract: the fact that occasional athletes were excluded from this study must be added.

Reply:
We agree with the reviewer and the abstract has been rewritten: see reply 6 reviewer one.

- Discussion: I am surprised that there are no other studies having compared knee complaints (or symptoms) between athletes and non-athletes. It could be interesting to compare the results obtained by the authors to results obtained in other studies.

Reply:
It would certainly be interesting to compare our results with the results of other studies comparing athletes and non-athletes with knee complaints. Unfortunately, we could not find any comparable literature. Therefore we could not compare our data with other studies and discuss this in the discussion.

What next?: Accept after minor essential revisions
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests

We modified our manuscript based on points raised by the reviewers (highlighted with yellow in the manuscript).
With the above mentioned additions and revisions we fully complied with the comments of the reviewers. We hope that you find our manuscript in this revised form suitable for publication.

We look forward to hear from you.

Sincerely yours
On behalf of the co-authors,

Marienke van Middelkoop