Reviewer's report

Title: Factors affecting the quality of life after total knee arthroplasties: a prospective study

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Reviewer: Montserrat Núñez

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Major Compulsory Revisions

Papakostidou et al. prospectively analysed the quality of life in a cohort of patients with osteoarthritis undergoing total knee arthroplasty using validated measures.

The study is of interest as not many prospective studies have analysed the quality of life in this type of patient. Likewise, the correlations analysed between the different measures are interesting and deserve closer analysis in the article.

However, I have some major reservations.

1) The written English of the article is poor and does not meet the standards of the journal. Apart from various obvious mistakes “Would infection” (Table 1), “radiographic sings” (page 11), the phrasing is clumsy, with the wrong verbal tense used, often making interpretation difficult. The article should be revised by a native English speaking medical translator in order to make it more comprehensible, without ambiguities, and with a better style which could probably reduce the article length.

2) The authors first state that patients were included if they “were speaking Greek” but later state “For patients unable to read Greek, the questions were read out by the interviewer”. This is confusing – are we talking about immigrants or illiterate subjects? Please clarify. Were any patients excluded for these reasons?

3) The authors state that patients with osteoarthritis were studied, but later state that 4% of patients had “posttraumatic arthritis”. What is the rationale for this?

4) The authors do not state exactly how many patients were included in the final analysis. I see that two patients died “in the early postoperative period” Were these patients included in the baseline or later analyses?

5) The authors state that the global mean age was 69.17 years. However, later they state that 86% of patients were discharged directly at (sic) home, and had a mean age of 65.6 years, and that the remaining 14% “were transferred to rehabilitation centers” and had a mean age of 68.9 years. These figures seem contradictory. Please clarify.

6) The authors state that patients were classified according to urban (>10,000) or
rural (< 2,000) status. They do not state anything about the patients coming from areas with 2-10,000 residents. Semi-urban or semi-rural? Please clarify. Also, it would be of interest to know how many patients came from each hospital and whether there were any differences between them and what the catchment areas are for each hospital. Likewise, I believe that Larissa is a fairly large city and therefore it would be of interest to know how the rural/urban status was determined. By municipality?

7) The authors state that 44.5% of patients were depressed preoperatively according to the CES-D10. Later they state that “depression was not detected at (sic) any of the examined groups during the post-operative follow-up intervals”. Later, in the Discussion, they state that “7.35% of patients remained in a depressed mood at 12 months. Please clarify these apparent contradictions.

8) The authors state in the Discussion that patients with BMI>40g/m2 were not included in the study. However this is not stated as an exclusion criterion. What was the rational for this, and how many patients were excluded for this reason?

9) The authors state that one investigator administered the questionnaires and evaluated parameters including range of motion. However, this investigator does not appear to have any medical or other qualifications according to the Title page. Please clarify what training this investigator has or received.

10) There are various ambiguous statements. For example, in the description of the KSS in the Methods section, the authors state “In addition, independent assessments of depression and pain were conducted at each evaluation to determine what psychological benefits patients are likely to experience after the surgical intervention”. What does this mean exactly? Likewise, the authors state that 86% of patients were discharge directly at (sic) home, the majority of whom (93%) received physical therapy services at home from (the ) physical therapist of their choice (12 sessions of physiotherapy in 6 weeks, starting the day after discharge.” Apart from the fact that the 86% and 93% figures are reported in Table 2 without distinction, what does this sentence mean? Were patients able to choose their own physio? Who provided physiotherapy? Equally, the authors should describe what they mean exactly by “rehabilitation centers” for readers accustomed to other types of healthcare models.

11) In fact, Table 2 provides little information and could be eliminated, with the results included in the text.

12) The references seem a little old and could be updated quite easily.

13) The authors should state the minimum clinical difference used to estimate the power of the study and therefore the sample size needed.

14) The authors should analyse the effect size of the improvements in QoL.

15) The authors state a hypothesis in the Discussion. If indeed this is the basic hypothesis underlying the study, it should be stated in the Discussion.

16) The description of the surgical characteristics does not seem to correlate with the variables described in the Methods. Some of this information may be superfluous.
17) WOMAC data should be normalized to a 0-100 scale if possible. If not, the tables should contain the values of the dimensions as a foot note.

18) Abbreviations in the tables should be described.

19) Figure 2 is not clear.

20) In table 5 the authors state 1.5 months. In the rest of the article, 6 weeks. Please unify.

**Level of interest:** An article of limited interest

**Quality of written English:** Not suitable for publication unless extensively edited

**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