Author’s response to reviews

Title: The Effects of Knee Arthroplasty on Walking Speed: A Meta-analysis

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Response letter

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The Effects of Knee Arthroplasty on Walking Speed: A Meta-analysis

Reviewer 1
Dear dr Ravikumar Pydisetty, Thank you for your report. We asked a native speaker to read the text carefully, and made some changes in grammar and language according to his suggestions. You wrote that the paper may be interesting to “those with closely related research interests”. Because of the comments of Reviewer 2, we have added a paragraph to the introduction (the second paragraph in the present version), and some remarks on “Clinical relevance” in the Discussion (just before Limitations), and trust that the text will be interesting to a wider audience now. In view of consistency, we made some minor changes in the rest of the text, and in one Figure. We are convinced that the paper is better now. Thank you again, Onno G. Meijer, on behalf of all authors.

Reviewer 2
Dear dr Nahum Rosenberg, Thank you for your evaluation. You wrote that the paper was “of limited interest” only, and we see your point. In the old version, we focused on epidemiological procedures only, without explicitly stating why our study would be important to the clinic, or which results were surprising. We now have added two paragraphs, which emphasize why the paper is of interest. We first give the text of these paragraphs, and then respond to your concerns in more detail.

The second paragraph of the introduction now reads as:

Over the last years, walking speed has received considerable attention in the literature. In elderly subjects, a decrease in comfortable walking speed may be a sign of co-morbidity [1], or even impending death [2-5]. In knee osteoarthritis, decreased walking speed is associated with joint space narrowing [6], increased concentrations of inflammation mediators [7], and pain [8]. After arthroplasty, walking speed is expected to increase [9], but in a longitudinal study, pain reduction did not lead to increased walking speed in knee osteoarthritis patients with new co-morbid conditions [10]. Hence, walking speed may not only be used as a simple instrument to monitor post-operative recovery, but also as a screening tool for co-morbidity.
And in the Discussion, just before Limitations, we have added:

**Clinical relevance**
The present study is, to the best of our knowledge, the first meta-analysis of the effects of knee arthroplasty on walking speed.

Patients tend to have overoptimistic expectations of functional recovery after arthroplasty, and to underestimate recovery time [55]. Moreover, it is not clear when exactly surgeons decide to recommend joint replacement [56], and provider-patient agreement on the expected benefits and risks of knee replacement is often low [57]. The present meta-analysis reveals that, on average, walking speed will increase considerably after knee arthroplasty, but this may take several months to occur. Moreover, in the long term, walking speed may decline again, which could be a sign of increasing co-morbidity [3].

The physiology of comfortable walking speed remains largely unknown. Just as in breathing rate, it is easy to change walking speed at will, but, again like breathing rate, it is prone to fall back to its own intrinsic parameters. Walking speed is a fair predictor of, e.g., co-morbidity, atherosclerosis, inflammatory status, cognitive impairment, hospitalization, and even mortality [4], and matches the predictive value of extensive clinical evaluation [4]. Walking speed is certainly not specific, but it is easy to measure, provided this is always done with the exact same methodology [14], including, for instance, the amount of clutter in the walkway [58].

Traditionally, UKA was used for older, inactive patients with medial knee osteoarthritis [59]. To date, however, the boundaries have become blurred, and there is considerable overlap in indications for UKA or TKA [60]. In the present meta-regression analysis, UKA led to somewhat better results than TKA, but not significantly so. This is in agreement with the literature [59].

You wrote that:

**Pulling the outcome data following TKA and UKA might be misleading because these surgical procedures have different indications, treat different intrarticular pathologies, in patients in different age groups and have different postsurgical rehabilitation expectations.**

This is now discussed at the end of “Clinical relevance”. For your convenience, we give an adapted version of Figure 2 in this response letter, with black circles referring to UKA, white circles to TKA, and triangles to mixed studies. The Figure suggests that there was no, or no major systematic difference between the effect-sizes after TKA vs. after UKA.
Your second point was:

The results presented do not provide any new information, i.e. it is well known that the expected initial rehab period is six months, therefore the evidence of high variability of outcome in this period is meaningless, additionally it is expected that following the initial rehab period, after 5-6-month, drastic improvement of pain is expected and if the knee pain was the only limiting preop factor the improvement in walking speed is expected. Existence of other limiting factors and their impact are not provided or discussed.

The literature reveals that patients tend to be overly optimistic, that surgeons do not always agree, and that provider-patient agreement is low. Hence, a quantitative analysis of the evidence is timely, as we do in our meta-analysis and meta-regression analysis, which both give new information. This is now discussed in “Clinical relevance”. Note that the correlation between walking speed and pain may be low. Our reference [8] gives -0.20, which implies not more than 4% common variance. A decrease of pain coincides with an increase in walking speed [10], but only when there is no increasing co-morbidity. Of course, a meta-analysis cannot deal with factors that are not, or not sufficiently often, mentioned in the studies analysed. We therefore decided to include some literature on factors codetermining walking speed.
Your third point:
The evolution of the restriction of the distance of walking ability is not provided, although this factor might be of greater importance than the speed of walking, as the reflection of functional outcome following knee arthroplasty. This point has been discussed previously in several reports.

Note that we have no problem at all with measuring distance walked. We now explicitly present the reasons why walking speed is (also) an important variable. Studies that were published in 2011 [2-3] led to considerable discussion [4]. The JAMA paper on walking 400 m in a corridor [2], also mentions walking speed as an important predictor of (co-)morbidity.

You concluded:
This meta analysis does not provide a substantial new information therefore it is not suitable for the publication.

This study does provide substantial new information (see our response to our second point), as is now summarized in “Clinical relevance”, about an important topic.

We may disagree in some points, but your critique stimulated us to adding two important paragraphs, which led to a better paper. Thank you, Onno G. Meijer, on behalf of the authors

Reviewer 3
Dear dr Khalid Syed, Thank you for your positive evaluation. You wrote that the paper may be interesting “in its field”. In view of the comments of Reviewer 2, we have added some paragraphs, the 2nd paragraph of the Introduction, and “Clinical relevance” in the discussion, which will render the paper more interesting to a wider audience. Note that we made some linguistic changes because of the comments of Reviewer 1, and corrected some inconsistencies in the text, and in one Figure. We are convinced that the present version is more accessible now. Thank you again, Onno G. Meijer, on behalf of all authors.