Comments on the article by Jinha et al. ''A task- specific validation of homogeneous non-linear optimisation approaches'

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Letter to the Editor

Dear Editor

We read with great interest the article by Jinha et al. ‘‘A task-specific validation of homogeneous non-linear optimisation approaches’’ (2009, vol. 259, pp. 695–700). The article studies the important issue of muscle recruitment using nonlinear convex optimisation and concludes: ‘‘that convex homogenous nonlinear optimisation approaches cannot predict individual muscle forces properly, as force-sharing among synergistic muscles obtained experimentally are not just scaled versions of joint loading, not even in a first approximation’’. Although the experimental data used appear to be well collected, we believe that the mathematical treatment and study setup contains several errors and limitations that have significant impact on the article's findings.

(1) From Eqs. (5) to (6), the authors use that $\phi(\kappa h) = \kappa\phi(h)$, which is only true for linear functions.

(2) Using Eqs. (4) to (6), it is stated that, given two different resultant joint moment vectors, which are scaled versions of each other, the resulting muscle forces for the two load cases are also scaled versions of each other with the same scaling ratio as the resultant joint moment vectors. This result is stated to hold for all convex homogeneous objective functions and musculoskeletal models. However, this is only generally true in the absence of upper bounds on the muscle recruitment, i.e. if muscle recruitment beyond 100% of a muscle's strength is accepted. While the upper bounds on muscle forces may be omitted for submaximal tasks, such as walking, they become important in the analysis of maximal tasks, where the distribution of forces changes as soon a muscle reaches 100% activation (Rasmussen et al., 2001).

(3) The article compares two situations in different gait cycles, which happen to have joint moments vectors over the knee and ankle joints that are scaled versions of each other, and use these to show that experimental muscle forces scale non-linearly, seemingly contradicting the mathematical behaviour of the convex optimisation problem. However, this argument presumes similarity of the moment arm vector, m_i, in the two cases. Therefore, since muscle moment arms change significantly with posture (Klein et al., 1996; Delp et al., 1999; Lee and Piazza, 2008), the article's argument only holds if the postures in the two cases also happen to be the same. It seems very unlikely that this should be the case.

(4) The experiment compares situations of scaled moment vectors of the knee and ankle only. However, the situation of equilibrium in the distal part of the hind limb is significantly influenced by the hip joint moment due to bi-articular muscles crossing the hip and knee joints and possibly also by the equilibrium of other joints proximal to the hip. The article's arguments are only valid for a system in which the knee and ankle joints are muscularly isolated from the rest of the body. For practical reasons, it is common practice to model parts of the musculoskeletal system in isolation from the rest of the body, and when doing so, it is critical not to exclude mechanical elements that significantly influence the force equilibrium. For studies of the lower extremity, it remains debatable how much to include, but exclusion of the hip joint from the model will likely introduce significant inaccuracy. Our experience with human models is that the hip joint and a part of the trunk must be included to provide origin points for the iliopsoas muscle fascicles.

The principal conclusion of the article is that muscle recruitment based on convex criteria does not seem to be mathematically able to reproduce experimental data. However, given the study limitations mentioned above, two other possible explanations for the findings of the article for this submaximal task could be: (1) the relevant joint moment vectors in the compared situations are in fact not collinear. As the authors only monitor the ankle and knee joint moments, it seems unlikely that, if the remaining joint moments of the cat were included in the analysis, they would also be collinear. (2) There may be a significant change in muscle moment arms between the compared situations, which is not controlled in the study or captured by the model. Due to these uncontrolled variables, it is not possible to determine whether the mismatch between the modelled and measured muscle forces is due to the muscle recruitment criterion or the excluded elements in the model.

To improve the study, we recommend to: (1) extend the analysis to also include the joint moments of the hip and include trunk origin points for muscles crossing the hip due to their importance for the equilibrium at the hip. (2) Quantify the variations in muscle moment arms and ensure that their variations are insignificant.

Since the authors have access to valuable measured muscle force data for model validation purposes, which are not available in the majority of studies, we recommend that the authors build a 3D musculoskeletal model of the cat hind limb that can take into account the effect of changing muscle moment arms and the effects of bi-articular muscles crossing the hip, knee and ankle. With this model, the commonly used convex muscle recruitment criteria can be examined for the different functional trials of the cat and the predicted forces evaluated quantitatively. Because all musculoskeletal models include uncertainties, the confidence in the results could be increased by computing the muscle forces as a function of uncertainties in the model parameters to obtain a range of possible computed muscle forces. If the measured muscle forces fall outside this range, it is likely that the applied muscle recruitment criterion is not correct. Otherwise the mismatch between the nominal musculoskeletal model and the measured muscle forces can be due to model uncertainties.

Muscle recruitment is indeed a challenging field of science requiring complex models and devoid of a gold standard because it is practically infeasible to measure muscle forces in vivo.

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Numerous papers on the subject (van Bolhuis and Gielen, 1999; Happee and Van Der Helm, 1995; Praagman et al., 2006) have found reasonable but definitely less than perfect matches between model predictions and experimental data, and the entire field contains many open questions. In view of this situation, the work of Jinha et al. discussed in this letter is most welcome. The interested reader may refer to a recent review by Erdemir et al. (2007) of different approaches and to an even more recent result speaking in favour of a minimum fatigue criterion (Ackermann and van den Bogert, 2010).

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