Dear Dr. habil. Molnár Ildikó,

I would like to sincerely thank you for your careful and constructive evaluation of my revised doctoral dissertation. I greatly appreciate your recognition of the improvements made in the revised version, particularly regarding the clearer theoretical framing, methodological transparency, and more cautious interpretation of the findings.

I also appreciate your remaining comments and questions. They are highly valuable because they help clarify the scientific scope of the dissertation and the appropriate interpretation of the three thesis points. Below, I provide my responses to the main issues raised in the final report.

1. Clarification of the single-subject finite element model

I agree that the finite element model in Study 1 was based on a single subject-specific foot geometry and therefore cannot be interpreted as population-level evidence. However, the purpose of using one subject-specific model was not to represent all pregnant women, but to create a controlled biomechanical simulation platform.

The main objective of this part of the dissertation was to decouple two pregnancy-related mechanical factors: increased gestational loading and reduced passive plantar ligament–fascia stiffness. In real pregnancy, these factors occur simultaneously, and in a multi-subject dataset they would also be mixed with individual differences in foot geometry, arch structure, ligament attachment sites, and plantar soft-tissue morphology. By keeping the same foot geometry and systematically changing only the loading and stiffness parameters, the model allowed me to examine how one foot structure responded to different pregnancy-related mechanical conditions.

Therefore, the finite element results should be understood as a subject-specific sensitivity analysis within a defined loading–stiffness design space. The value of this approach is to identify possible mechanical pathways, for example loading-related transverse arch widening and stiffness-related arch-height reduction, rather than to estimate average effects for the pregnant population. I have revised the dissertation to make this interpretation clearer and to avoid direct generalization beyond the simulated model conditions.

2. Interpretation of IAA and the absence of EMG validation

I also agree that the conclusions related to neuromuscular control should be interpreted carefully because EMG data were not collected in the present study. In the revised dissertation, I have therefore avoided interpreting IAA as direct evidence of muscle activation or neural control.

In this thesis, IAA was used as a model-based biomechanical method. The muscle forces were estimated within the OpenSim musculoskeletal model, and IAA then calculated the joint angular acceleration induced by selected muscle-group forces. Therefore, the IAA results provide a mechanical interpretation of how model-estimated muscle forces may influence joint motion during stance. They do not directly validate real muscle activation patterns.

The value of IAA in this dissertation is that it helps move beyond external gait parameters, such as walking speed and stride length, and provides a model-based interpretation of joint- and muscle-group-specific mechanical effects. Nevertheless, I fully agree that future studies should combine IAA with synchronized EMG measurements. EMG could be collected from key lower-limb muscles, such as the tibialis anterior, gastrocnemius, soleus, quadriceps, hamstrings, and gluteal muscles. This would allow future research to compare muscle activation timing and amplitude with IAA-derived muscle-force-induced joint angular acceleration patterns, thereby strengthening the interpretation of neuromuscular adaptations during pregnancy.

3. Psychological factors and conservative gait adaptation

I agree that psychological factors, such as fear of falling, increased caution, or reduced confidence during walking, may also contribute to the conservative gait pattern observed in late pregnancy. The present dissertation mainly focused on biomechanical and model-based indicators, and therefore cannot fully separate psychological caution from mechanical adaptation.

In future research, this issue could be addressed by combining biomechanical gait analysis with psychological and behavioral measures, such as fear-of-falling questionnaires, balance confidence scales, fall history, and perceived stability ratings. These measures could be analyzed together with walking speed, stride length, double-support time, ground reaction forces, EMG activity, and model-based indicators. Such a design would help determine whether conservative gait patterns are more strongly associated with mechanical constraints, psychological caution, or their interaction.

Therefore, in the present dissertation, I interpret the conservative gait pattern as a biomechanically observed adaptation, while acknowledging that both mechanical and psychological factors may contribute to this behavior.

4. Footwear intervention and fall-prevention interpretation

I agree that Study 3 does not provide direct clinical evidence that negative-heel shoes reduce fall incidence. The intervention was acute and focused on biomechanical indicators during walking. Therefore, the findings should not be interpreted as proof of long-term fall prevention.

The contribution of Study 3 is more specific: it shows that heel-to-toe drop is a modifiable footwear design parameter that can acutely influence late-pregnancy gait biomechanics. In particular, negative-heel shoes affected stance-phase force timing, ankle posture, and COM–COP alignment. The reduced peak backward COM–COP angle suggests a change in anteroposterior stability-related alignment, but this remains an indirect biomechanical indicator rather than direct evidence of reduced falls.

Future studies should examine long-term footwear use, adaptation effects, comfort, adherence, possible compensatory loading at other joints, and actual fall or near-fall outcomes. A longitudinal or randomized intervention study would be needed to determine whether the observed biomechanical changes translate into clinically meaningful reductions in fall risk.

5. Relationship between biomechanical indicators and actual fall risk

The final report correctly notes that the relationship between biomechanical indicators and real fall events remains indirect. I agree with this point. In the present dissertation, walking stability was assessed through biomechanical variables, including gait parameters, joint mechanics, ground reaction forces, and COM–COP-related indicators. These variables are relevant to stability, but they cannot directly confirm future fall events.

A direct relationship between biomechanical indicators and actual fall risk would require a longitudinal clinical design. Pregnant women could be followed from early pregnancy to postpartum, with repeated gait assessments and prospective recording of falls, near-falls, daily activity exposure, footwear use, and psychological factors. Statistical models could then test whether biomechanical indicators, alone or in combination with behavioral and clinical variables, predict real fall risk. This would be an important next step beyond the current dissertation.

Once again, I sincerely thank the reviewer for the detailed and constructive evaluation. The comments have helped me clarify the scientific contribution and the appropriate interpretation of the findings, and they also provide a clear direction for future research.

Sincerely,
Li Xin

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