Dear Dr. Bálint Kovács,

I would like to sincerely thank you for your careful evaluation of my doctoral dissertation and for your constructive comments. I appreciate the detailed suggestions, which helped me further clarify the methodological boundaries, terminology, interpretation, and limitations of the dissertation.

I have carefully revised the dissertation according to your comments. The main revisions and clarifications are summarized below.

1. Single-subject finite element model and scope of interpretation

I agree that the finite element study was based on a single subject-specific foot model and therefore should not be interpreted as population-level evidence. However, the rationale for using a single subject-specific model was not to represent the whole pregnant population, but to create a controlled biomechanical platform for mechanism-based analysis.

In this study, the key objective was to decouple the effects of pregnancy-related loading and reduced passive plantar ligament–fascia stiffness. In real pregnancy, these two factors occur simultaneously, and in a multi-subject dataset, the effects would also be mixed with large inter-individual differences in foot geometry, arch structure, ligament attachment locations, and plantar soft-tissue morphology. By using one subject-specific geometry and systematically changing only the prescribed loading and stiffness parameters, the model allowed us to isolate how the same foot structure responded to different pregnancy-related mechanical conditions. Therefore, the single-subject design was appropriate for the mechanistic purpose of this study. It enabled a controlled sensitivity analysis within a defined loading–stiffness design space, rather than an estimate of population prevalence or average pregnancy effects. The findings should be interpreted as showing possible mechanical pathways of pregnancy-related foot deformation, such as loading-related transverse widening and stiffness-related arch-height reduction, rather than universal values for all pregnant women.

I have revised the dissertation to make this scope clearer. The finite element results are now described as subject-specific mechanistic findings, and the conclusions avoid direct generalization to pregnant women as a population. The limitations section also states that the findings are constrained by the individual geometry, material properties, boundary conditions, loading assumptions, and stiffness-scaling assumptions of the model. Future studies should extend this approach to multiple subject-specific models, different foot types, and longitudinal imaging or experimental validation to improve generalizability.

2. Regression analysis in the finite element study

Thank you for the important comment regarding the statistical framework of the finite element study. I agree that the 16 full-factorial simulation conditions were deterministic outputs generated from a single subject-specific model, rather than independent biological observations. Therefore, conventional inferential statistical terminology, such as p-values, confidence intervals, and statistical significance, is not appropriate in the usual population-level sense.

To address this issue, I revised the finite element statistical analysis as a descriptive regression-based sensitivity analysis. The purpose of the regression is now clearly stated as quantifying the direction and relative coefficient pattern of body weight-related loading, passive plantar ligament–fascia stiffness scaling, and their interaction within the simulated W–K design space. I have avoided interpreting the coefficients as population-level effect sizes or inferential statistics. P values and corrected p values are no longer reported for this part of the analysis. Instead, the interpretation focuses on standardized coefficients, relative magnitude, and mechanical direction.

3. Terminology concerning gestational weight gain and added gestational mass

Thank you for the comment regarding the terminology of body mass change during pregnancy. I agree that the term “redistribution of body mass” may be misleading if it suggests that pre-existing maternal mass is reallocated. The intended meaning was that gestational weight gain adds mass to the body, and that this added gestational mass is not evenly distributed across body segments.

I have therefore revised the terminology in the dissertation. “Gestational weight gain” is used to describe the overall increase in maternal body mass during pregnancy. When referring to the spatial location of the added mass, the revised text uses expressions such as “uneven distribution of added gestational mass” or “added gestational mass distributed in the anterior-inferior trunk region.” This distinction makes the mechanical interpretation clearer and avoids equating pregnancy-related weight gain with a simple or uniform increase in body mass.

4. Hormonal changes, joint laxity, and connective tissue behavior

Thank you for noting that hormonal changes begin early in pregnancy and should not be described only as a response to later mechanical loading. I agree. The revised dissertation now treats hormonal and connective-tissue changes as natural physiological processes during pregnancy, rather than as purely load-driven adaptive responses.

I have also added a clearer definition and explanation of joint laxity. In the revised text, ligamentous laxity is not interpreted as a uniform decrease in the stiffness of every ligament or tendon. Instead, it is described as increased passive joint mobility or reduced passive restraint under load, potentially related to endocrine remodeling, tissue hydration, collagen turnover, viscoelastic behavior, and local mechanical conditions.

The discussion has also been revised to address the apparent contradiction between increased mechanical loading and possible increases in tissue compliance. I now explain that these mechanisms may act simultaneously and not necessarily in the same direction. Increased loading may stretch some passive structures and increase their tensile demand, whereas pregnancy-related endocrine and tissue-level changes may reduce the effective passive restraint of some tissues. Therefore, pregnancy-related foot deformation is now interpreted as the result of interaction among external loading, foot geometry, and tissue behavior, rather than as a simple uniform reduction in ligament stiffness.

5. Plantar fascia and passive stiffness assumptions

Thank you for asking where the plantar fascia and passive stiffness changes came from. I agree that direct evidence quantifying pregnancy-specific changes in plantar fascia stiffness is currently limited. Therefore, I have revised the text to avoid implying that the stiffness-scaling values were directly measured pregnancy-specific tissue properties.

In the revised dissertation, the stiffness parameter is presented as a simulation assumption used to examine the mechanical sensitivity of the subject-specific foot model to possible reductions in passive plantar support. The model was not intended to prove the actual in vivo change in plantar fascia compliance during pregnancy. Instead, it was used to test how the modeled foot would respond mechanically if passive plantar ligament–fascia stiffness were reduced within the defined simulation framework.

I also added discussion that real foot passive structures may behave heterogeneously. As bone positions and arch morphology change, some ligament attachment points may move farther apart and increase tensile demand, whereas other structures may become shorter or less loaded. In the case of the plantar fascia, arch flattening may increase tensile stretch, while pregnancy-related tissue changes may counteract this through altered compliance. This uncertainty is now explicitly acknowledged.

6. Comparison between finite element pressure and Pedar pressure

Thank you for pointing out that this comparison should not be called a direct accuracy or measurement-error calculation. I agree that the finite element model and Pedar system do not measure exactly the same physical quantity. The finite element model estimates contact pressure within the modeled foot-ground interaction, whereas Pedar measures plantar pressure at the external contact interface, with soft tissue and insole effects included.

I have therefore revised the wording. The comparison is now described as a reference-condition plantar pressure comparison and a qualitative consistency check of the approximate regional magnitude and distribution pattern, rather than as a direct accuracy assessment. I also clarified that the comparison concerns similar anatomical regions, while acknowledging that the exact locations of the peak values may differ between the model and the experimental measurement.

7. Relationship among spatiotemporal variables, muscle control, and movement outcome

Thank you for noting that spatiotemporal variables, neuromuscular control, muscle force, and movement outcome are interrelated and should not be treated as completely separate mechanisms. I agree with this point.

The revised dissertation now presents these variables as different levels of the same biomechanical system. Spatiotemporal parameters describe the global walking outcome. Joint kinematics, joint moments, and joint work describe the movement and loading pattern at the joint level. Induced acceleration analysis provides a model-based mechanical estimate of how selected muscle-force contributors influence joint angular accelerations within the musculoskeletal model. This structure avoids presenting these variables as independent mechanisms and instead interprets them as complementary descriptions of walking gait.

8. Interpretation of IAA and "muscle contribution"

I agree that the original wording may have overstated what induced acceleration analysis can demonstrate.

In the revised dissertation, I have avoided interpreting IAA as direct evidence of actual muscle activation, neuromuscular regulation, or muscle work. The terminology has been revised to "model-estimated muscle-force-induced joint angular acceleration" or "IAA-derived muscle-force-induced joint angular acceleration." These expressions are intended to clarify that the analysis estimates the angular acceleration induced by selected modeled muscle forces within the musculoskeletal system.

Therefore, the IAA results are interpreted as biomechanical inferences rather than direct physiological measurements. They provide information about the model-estimated mechanical roles of selected muscle groups during stance, but they do not directly measure neural control, EMG activity, muscle force in vivo, or muscle work.

9. Segmental inertia and angular acceleration

Thank you for asking how moment of inertia was considered. In the revised explanation, I clarified that the angular accelerations obtained from IAA are not calculated from muscle force alone. They are generated within the multibody dynamics framework of the musculoskeletal model, which includes segment mass properties, center-of-mass locations, inertial properties, joint geometry, external forces, and dynamic coupling among body segments.

Accordingly, the reported IAA outputs should not be interpreted as isolated muscle work. They represent the model-predicted angular acceleration associated with specific force contributors within the dynamic system. Segmental moment of inertia is therefore included in the model equations of motion and contributes to the resulting acceleration estimates.

10. Walking speed and possible bias

Thank you for pointing out that walking speed may bias the comparisons. I agree that this is an important limitation.

In the gait comparison study, participants walked at their self-selected comfortable speed. This design reflects natural walking behavior and is useful for describing how pregnant women actually choose to walk. However, it also means that differences in joint moments, ground reaction forces, segmental accelerations, and joint work may be partly influenced by walking speed. I have therefore strengthened the discussion and limitation to state that the findings should be interpreted as late-pregnancy walking under self-selected speed conditions, rather than as speed-independent pregnancy effects.

In the footwear study, I also agree that walking speed should ideally be controlled across conditions to isolate the independent effect of footwear geometry. In the revised dissertation, I explicitly state that negative-heel shoes modified gait mechanics under self-selected walking-speed conditions. I also acknowledge that the observed footwear-related differences cannot be attributed solely to heel-toe drop, because walking speed differed among footwear conditions. The conclusion has been revised accordingly, and future speed-controlled studies are recommended.

11. Force, torque, work, and mechanical relevance

Thank you for asking whether the analysis was limited to force or also included torque and work. The revised dissertation clarifies that the gait comparison study included not only ground reaction force and kinematics, but also sagittal-plane joint moments and body-mass-normalized joint work during stance. The discussion now gives more emphasis to joint moments and ankle work before interpreting the IAA-derived acceleration patterns.

Regarding ground reaction force curves, I agree that simply stating that a curve is higher or lower is not sufficient. I have therefore revised the discussion to explain the biomechanical meaning of the time-continuous GRF differences. The footwear-related changes in vertical and anteroposterior GRF are now interpreted in relation to loading acceptance, stance-phase support, body progression, and late toe-off propulsion. I also acknowledge that impulse analysis could provide an additional summary of force-time behavior and may be considered in future work.

12. Discussion structure and interpretation

Thank you for suggesting that the discussion should follow the structure introduced in the thesis. I have reorganized the discussion into three main sections corresponding to the three studies:

1. structural interpretation of the finite element findings;
2. biomechanical interpretation of walking gait during pregnancy;
3. biomechanical effects of negative-heel footwear in late pregnancy.

This revised structure follows the progression of the thesis from internal foot mechanics, to whole-body walking gait adaptation, and finally to footwear intervention. I have also reduced repetition of the results and placed more emphasis on the biomechanical meaning of the findings.

In the finite element discussion, I now explain that literature-informed loading and stiffness parameters were used to simulate possible pregnancy-stage conditions, rather than claiming that these inputs were directly measured in the same subject. The wording has been revised to indicate that, based on the model results, it is possible to speculate how increased loading and reduced passive support could influence foot deformation and load transfer.

I have also added a more explicit discussion of daily-life implications. Reduced arch stiffness may limit the efficiency of force transfer during push-off, which may be related to the lower ankle moment and lower ankle positive work observed in pregnant women. I also discuss that reduced arch height may influence rearfoot motion and whole-body posture, although this is presented as a possible biomechanical implication rather than a directly proven causal relationship.

13. Footwear conclusions and fall-prevention interpretation

Thank you for emphasizing that footwear-related conclusions should be cautious because walking speed was not controlled. I agree.

In the revised dissertation, I no longer claim that negative-heel shoes directly reduce fall risk. Instead, the conclusion states that negative-heel shoes acutely modified stability-related gait indicators under self-selected walking-speed conditions. Specifically, they influenced walking organization, lower-limb posture, force timing, and COM–COP alignment, rather than increasing sagittal-plane ankle or knee moment output.

I have also clarified that these findings provide preliminary biomechanical evidence that heel-toe drop can influence walking mechanics in late pregnancy, but they do not establish whether these changes translate into improved gait stability or reduced fall risk in daily life. Future studies should include speed-controlled designs, larger cohorts, longer-term adaptation, comfort and adherence assessment, and real-world safety outcomes.

14. Publications related to the thesis points

Thank you for the comment regarding the relationship between the thesis points and the related publications. I have revised the thesis-points and publication-related sections to make the connection clearer. The publications are now presented in relation to the specific thesis points they support, including finite element modeling of pregnancy-related foot deformation, pregnancy-related gait alterations, and footwear or plantar-loading biomechanics.

Overall summary

In summary, I have revised the dissertation to make the methodological scope and interpretation more precise. The most important changes include:

- **clarifying the finite element study as a single-subject, model-based sensitivity analysis rather than a population-level inferential study;**
- **replacing inferential statistical language in the finite element analysis with descriptive sensitivity-analysis terminology;**
- **improving the terminology concerning gestational weight gain and added gestational mass;**
- **defining joint laxity and explaining the interaction between mechanical loading, tissue behavior, and foot geometry;**
- **revising the interpretation of IAA from direct "muscle contribution" to model-estimated muscle-force-induced joint angular acceleration;**
- **clarifying the role of segmental inertia in the musculoskeletal model;**
- **acknowledging walking speed as a potential source of bias in both the gait and footwear studies;**
- **emphasizing joint moments, joint work, force timing, and**

biomechanical meaning rather than only curve differences;

- **reorganizing the discussion according to the three-study structure of the thesis;**
- **making the footwear conclusion more cautious and avoiding direct claims of fall-risk reduction.**

Thank you again for your careful and constructive review. Your comments helped me improve the scientific accuracy, methodological transparency, and interpretive clarity of the dissertation.

Sincerely,
Li Xin

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