Author's response to the reviews and relevant changes to the manuscript egusphere-2024-1093

This document provides a detailed point-by-point response to all referee comments and the relevant changes that have been made in the manuscript. Referee comments are shown in *italics*. Author's responses are shown in **bold**.

Anonymous Referee #1:

The authors have adequately addressed my remarks from the initial review. Thank you very much. Please address the following technical points before proceeding with final publication:

1. Issue has been addressed.

2. Issue has been addressed.

3. The issue has been mostly addressed. A generalized coordinate ζ has been introduced (see Eq. 3). Please make sure that all symbols provided in the paper are adequately defined.

All instances of the symbol ζ have already been replaced with the thickness parameter ϑ^3 in the previous revision of the manuscript. The thickness parameter ϑ^3 is now explicitly mentioned on page 4 of the revised manuscript. We have checked to the best of our ability that all symbols are defined adequately and available in Appendix A: List of symbols.

4. Thank you very much for the explanation. In the view of the additional information, I agree with the authors that straight two-dimensional beams can be thought of a one-dimensional simplification of the standard plate theory. The authors mentioned that this section is crucial for establishing a relation to the commonly used onedimensional model for flexural isostasy. Under these circumstances, I would encourage the authors to re-introduce this paragraph and make sure they adequately explain that the one-dimensional reduction does not introduce any other three-dimensional effects.

The paragraph has been re-introduced with an explanation on the absence of additional rotational effects for the beam model.

5. Thanks for the explanation, however this point is still confusing. In the revised form of the paper you write "Using the more general shell equations, it is possible to perform simulations of the lithosphere on the whole surface of the Earth." (page 10 in the revision). It seems that you spent considerable amount of space in the paper to present the equations for the plate, but you did not spend any effort in explaining the equilibrium equations for the shell. From a mathematical standpoint, the equilibrium equations for the shell are virtually an embedding of the plate equilibrium equations to a curvilinear space. However, this introduces significant mechanical effects and load carrying behaviors, that are not found in plates. It would be great if you could think of ways to unify your presentation in terms of plates and shells, because currently there is a deep presentation for the plate and almost no word spent for the shell.

The equilibrium equations for thin elastic shells are discussed in Section 2.1.1. They are reduced to the case of a plate with an initially flat configuration in Section 2.1.2. Topographic loading and buoyancy are introduced for the plate model in Section 2.2 and extended to the spherical shell model in Section 2.4.

6. Thanks a lot for the clarification.

7. Understood, thank you very much for the explanation.

8. Thanks for the explanation. I would use an italicized font when writing symbols attributed to functional spaces, such as Sobolev spaces.

Thank you for the suggestion. We would like to keep the upright font style for function spaces in our manuscript.

9. Thanks a lot for the explanation. If modeling the entire lithosphere using a single shell is an enormous simplication, which is clear, would it still be possible to use multipatch geometries to capture the physical reality in a more accurate manner? It might be worthwhile adding a note into your paper.

Multi-patch parametrizations are still useful for modeling domains with geometries that are more complex than quadrilaterals, e.g., the multi-patch example for Central Java. Simulations on a curved domain can provide results that are more accurate if the initial midsurface configuration of the lithosphere plays a significant role, which may be the case for certain locations or in large scales.

10. Issue has been addressed.

11. Issue has been addressed.

12. Issue has been addressed.

13. Issue has been addressed.

14. Issue has been addressed.

15. Issue has been addressed.

16.–17. Thanks a lot for the clarifications. Please consider adding this argumentation directly into your manuscript to make sure that ambiguity is removed.

Section 3.2.3 has already been modified in the previous revision of the paper to address most of the issues. A sentence has been rearranged and we have added the remark "Note that contiguous patches are assumed to share the same interpolatory control points at the interfaces to enforce the C^0 continuity in our implementation."

18. Thanks a lot for the clarifications.

19. I think this enhanced my misunderstanding regarding the use of plates and shells in this work. I know understand, that on page 16 it is discussed yet plates in regard to the strong C^1 continuity, but in Section 5.3 it is used a shell for the example. Is then my understanding correct, that Section 3.2.3 discusses the C^1 (strong) multipatch coupling only in the setting of plates? I understand that for the shell (see example in Section 5.3) you are not using any multipatch coupling of shells, could you please clarify that somewhere in your text to remove ambiguity?

Section 3.2.3 discusses the strong multi-patch C^1 coupling for both plates and shells, as indicated by the sentence "This has been extended from the case of planar multi-patch domains to multi-patch surfaces in Farahat et al. (2023a, b)." The numerical implementation used in our work is the one in Section 8.4 of Collin et al. (2016), which works for surfaces as well (see Section 6 therein) and has been used for the example discussed in Section 5.3 of our paper.

20. Thanks for clarifying. Please consider adding this information into your manuscript.

The phrase "when the data required for the simulation are only available on certain parts of Earth's surface" has been added to a relevant sentence on page 19 of the revision.

21. Thanks a lot for the clarification.

22. Thanks a lot for addressing the bug. This will make reproducibility of the results easier.

Thank you very much for testing our software and for all the other feedbacks.

Referee #2 (Tony Lowry):

With the changes, the paper is worthy of publication. For future work on this topic though, I would recommend that the authors read carefully and understand the concepts referred to earlier in papers by Forsyth (JGR 1985) and the textbook by Kirby (2022). Note particularly that estimation of subsurface loading is not as simple as inverting gravity, because both topography and gravity are functions of both flexure (hence the choice of flexural rigidity) and the loading distributions... Which places Te entirely within a null-space that can only be overcome by imposing statistical constraints on the problem.

Having spent a couple of decades now trying to advance the frontiers of seismic data analysis to improve upon estimates of the subsurface density structure (with a partial goal to improve estimates of Te), I can affirm with some confidence that the seismology community is still a very long distance from having sufficient confidence in seismically-constrained density structure to no longer need statistical (i.e., power spectral) approaches in estimating Te. So, when taking the next steps with this new approach, I hope you will also be prepared to incorporate power spectral estimation into the analyses!

Best regards,
Tony

Thank you very much for the helpful feedback and for suggesting acceptance of our paper for publication at Geoscientific Model Development. We will keep your remarks in mind and consider power spectral estimation in the analyses for future work on this topic.

Further changes to the manuscript:

Equations without numbering and equations that are made smaller to make space in the two-column version of the manuscript have been given numbers and returned to the default size in the one-column version, respectively. This should be fine-tuned in the final two-column version of the paper.

Missing gradient operators, denoted by the nabla symbol $\boldsymbol{\nabla}$, in the definition of the $\mathbf{C}^{\mathbf{1}}$ constraint matrix (Equation 40 of Section 3.2.3 in the revised version) have been added. Some index placements (superscripts instead of subscripts) for the parameter ϑ in Section 3 have been fixed to distinguish coordinates from knot values. Some spacings have been added between the declaration and the definition of mappings in display-style math mode. A citation has been fixed: Hinze et al. (2009).