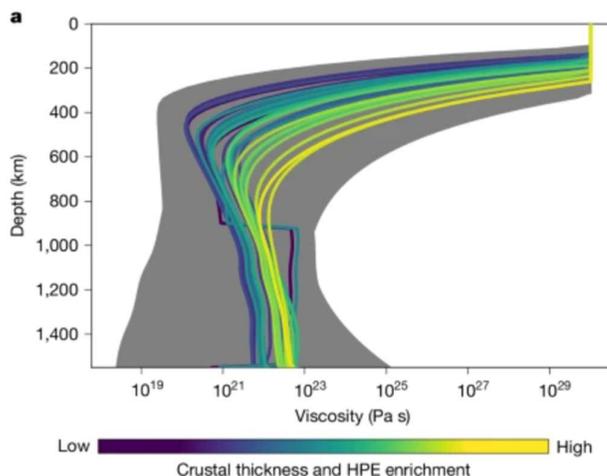**Reviewer 1 – Bernhard Steinberger**

Given my area of expertise, my ability to review that paper is limited to anything that relates to mantle dynamics and than is not much, unfortunately. The one part I am familiar with is on the Green's functions. On the whole, in the absence of further information, I think the approach taken - converting geoid coefficients to CMB topography coefficients assuming amplitudes of mantle loads are independent of depth - is reasonable, but of course the sensitivity kernels and hence the conversion depend on viscosity structure, and I would have preferred if that was a bit discussed, perhaps elaborating on uncertainties depending on viscosity structure, or variability for the range of possible viscosity structures.

We thank the reviewer for this thoughtful comment. We agree that the conversion from geoid coefficients to CMB topography depends on the assumed mantle viscosity structure through the associated Green's functions, and that this sensitivity deserves clarification.

In this study, we adopted the viscosity profiles originally proposed by Sohl and Spohn (1997) and later used by Defraigne et al. (2001) to compute Green's functions for CMB deformations. Mars' mantle is extremely viscous compared with Earth, consistent with slow convection and stagnant-lid tectonics. In the reference model, viscosity decreases by about six orders of magnitude (10^{20}–10^{26} Pa s) beneath the crust down to the base of the lithosphere and even deeper, primarily due to the strong temperature dependence of rheology under a steep conductive gradient (lithosphere viscosity: 10^{20}–10^{26} Pa s; convecting mantle viscosity: 10^{21}–10^{22} Pa s). Within the underlying adiabatic deep mantle, viscosity (10^{22}–$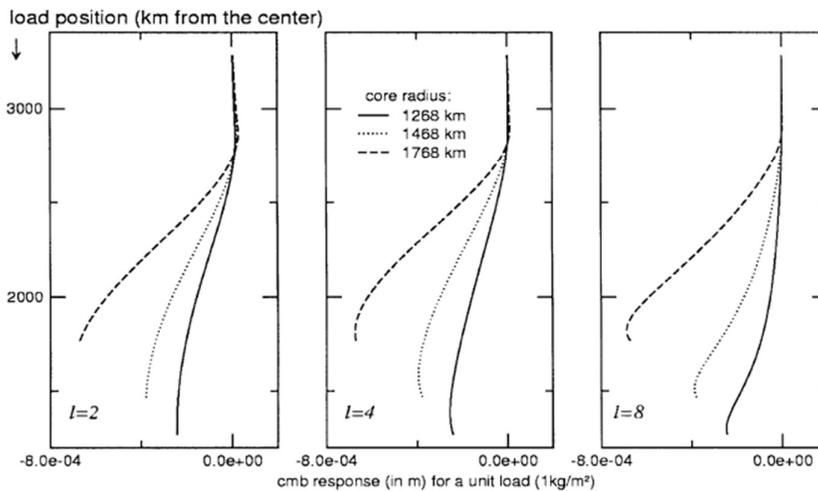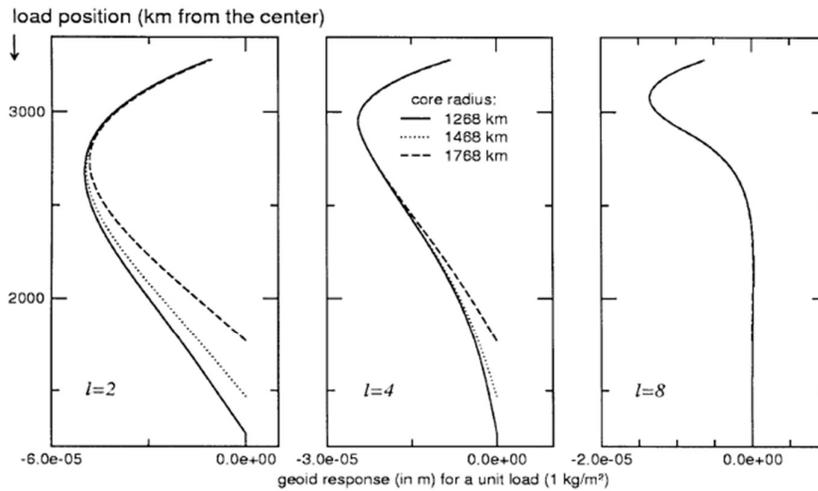10^{23}$ Pa s) varies more moderately (about one order of magnitude), as the temperature effect slightly overcompensates the pressure dependence. The most recently provided values were those of Broquet et al. (2025)[1], in which mantle viscosity for depths <500 km is 10^{19}–10^{27} Pa s, 500 km is 10^{21}–10^{23} Pa s, very close to the values used in Defraigne et al. (2001). The figure on the left reproduces the viscosity profiles inferred by Broquet et al. (2025).

[1] Broquet, A., Plesa, A.-C., Klemann, V., et al. (2025). *Glacial isostatic adjustment reveals Mars's interior viscosity structure.* **Nature, 639**, 109–113.
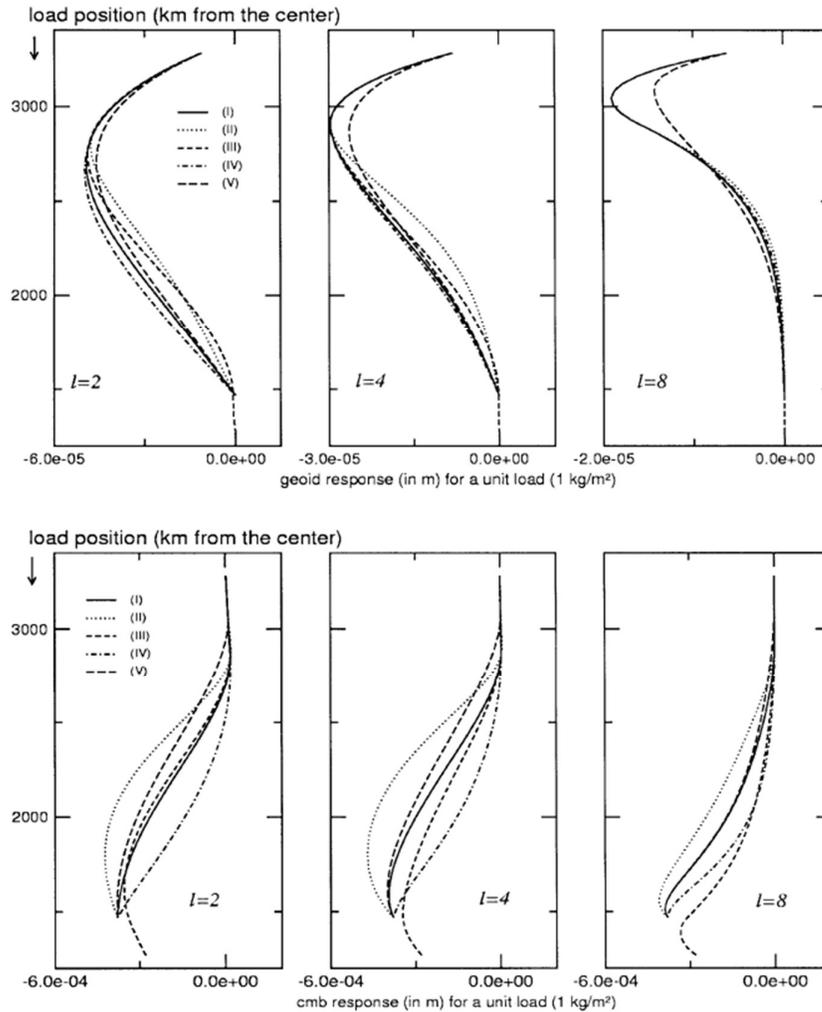
Defraigne et al. (2001) explicitly investigated the sensitivity of the Green's functions to Martian mantle viscosity by testing several alternative profiles, including:

- a constant-viscosity mantle,
- a viscosity increasing linearly with depth (factor of 10 from lithosphere base to CMB),
- a model including a viscosity jump (factor of 10) near the spinel–post-spinel transition,
- and a model with viscosity decreasing linearly with depth.

Their results show that the inferred CMB topography amplitude is indeed sensitive to the core radius (see figure just below), and to the viscosity structure (see figure further down), as the reviewer pointed out.

(Defraigne et al., 2001, where we noted that the CMB is in km and not in m)

load position (km from the center)

3000

2000

(I)
(II)
(III)
(IV)
(V)

l=2

-6.0e-05   0.0e+00   -3.0e-05   0.0e+00   -2.0e-05   0.0e+00

l=4

l=8

geoid response (in m) for a unit load (1 kg/m²)

load position (km from the center)

3000

2000

(I)
(II)
(III)
(IV)
(V)

l=2

l=4

l=8

-6.0e-04   0.0e+00   -6.0e-04   0.0e+00   -6.0e-04   0.0e+00

cmb response (in m) for a unit load (1 kg/m²)

(Defraigne et al., 2001, where we noted that the CMB is in km and not in m)

Current observation constraints do not allow robust discrimination among these models as also shown by Broquet et al. (2025).

In our analysis, we restrict ourselves to models without a bridgmanite layer immediately above the CMB and assume a relatively large core, consistent with recent constraints (Le Maistre et al., 2023). Under these assumptions, variations in plausible mantle viscosity profiles modify the inferred CMB topography amplitude by no more than about 25%, which does not alter the order-of-magnitude conclusions of this study.

To clarify this point, we have added the following sentence to the manuscript:

"The viscosity profiles used in these computations are those proposed by Sohl and Spohn (1997) and adopted by Defraigne et al. (2001). Recent viscosity estimates from Broquet et al. (2025) are in very good agreement with these values. Although Defraigne et al. (2001) examined several alternative models, including cases with a bridgmanite layer at the base of the mantle, we restrict our analysis to models without such a layer

and assume a large core, consistent with Le Maistre et al. (2023). In this framework, uncertainties in the viscosity profile affect the inferred CMB topography amplitude by no more than about 25%."

We believe this addition better places our assumptions and associated uncertainties in context.

Line 53: on degree-1 gravity: In center of mass coordinates, this is required to be zero, as also pointed out by Wieczorek et al. (2019). Also, that would mean that the approach of inferring CMB topography from gravity mentioned in the above point would not work, so it should be clarified how you get degree-1 CMB topography. Relate it to degree-1 surface topography (since that can also be computed from sensitivity kernels) or infer it from Kaula's law?

We thank the reviewer for this important clarification. We fully agree that the degree-1 gravity coefficient must vanish in a center-of-mass reference frame, as emphasized by Wieczorek et al. (2019). Accordingly, we did not use the degree-1 geoid coefficient to infer degree-1 CMB topography.

Instead, the degree-1 CMB contribution is inferred following the approach of Wieczorek et al. (2019), who showed that spherical harmonic degree-1 mass anomalies within the lithosphere can generate corresponding degree-1 displacements of internal density interfaces, including the core–mantle boundary. In their model, this results in an offset of the Martian core relative to the center of mass of approximately 90 m.

To clarify this point, we have added the following sentence to the manuscript (around line 255):

"For that case, we use the computations of Wieczorek et al. (2019), who considered gravity anomalies within the lithosphere that perturb the shapes of the underlying hydrostatic density interfaces, including the core–mantle boundary."

This makes it explicit that the degree-1 CMB topography is not derived from degree-1 geoid coefficients, but from the lithospheric mass anomaly framework described in Wieczorek et al. (2019).

Line 66/67: "greater than in cases without a post-spinel phase transition" - I don't understand, I think the case with a larger core is the case without a post-spinel phase transition.

We thank the reviewer for pointing this out. You are correct — this was a typographical error.

From the Green's functions presented by Defraigne et al. (2001), models that include a ringwoodite–bridgmanite (post-spinel) phase transition predict a smaller CMB topography amplitude than models without such a phase transition. In the case of a relatively large core, mantle pressures are insufficient to trigger the ringwoodite–bridgmanite phase transition in the lowermost mantle.

Consequently, the predicted CMB topography for a large core (on the order of 1–5 km) is larger than in models that include a post-spinel phase transition.

The sentence has now been corrected accordingly ("with" instead of "without").

Line 67: What is non-hydrostatic equilibrium? Do you mean deviation from hydrostatic equilibrium?

We thank the reviewer for pointing this out. Yes, we meant a deviation from hydrostatic equilibrium. The wording has been corrected accordingly in the revised manuscript.

Line 104: You start here with 1, but the introduction is not numbered. I find this a bit confusing.

We agree that the numbering was inconsistent and potentially confusing. We have now revised the manuscript so that section numbering begins with the Introduction and is consistent throughout the paper.

Figures 1 and 2: These symbols are hard to recognize. It would be helpful to plot that figures bigger. For the Plus-signs on the axis, the horizontal line cannot be seen, so they are especially hard to be recognized. Symbols for prograde and retrograde appear to be identical, so they cannot be distinguished. What is the unit for sigma (frequency)? In Figure 1 sigma goes from 0.99 to 1.01 whereas in Figure 2 it goes from 0 to 0.09.

We thank the reviewer for these helpful comments.

The figures have been provided in vector format and enlarged in the revised manuscript, allowing the editor to adjust their final dimensions to ensure optimal readability.

The frequency variable σ is expressed in a frame tied to the planet. This explains the different frequency ranges shown in the two figures. Nutations appear in the diurnal frequency band in the body-fixed frame, which is why Figure 1 spans values close to 1. Retrograde and prograde nutation are clearly indicated in the figure caption now. Indeed, to clarify this point, we have added the following explanation to both the main text and the figure captions:

"Nutations appear in the diurnal frequency band in a frame tied to the planet; retrograde long-period nutation in space appear thus at frequencies σ >1; whereas prograde long-period nutation in space appear at frequencies σ <1."

In contrast, long-period LOD variations appear at much lower frequencies, as shown in Figure 2 (this figure became Figure 5 in the meantime.) We have added "The x-axis represents the frequencies in cycle/day." in the figure caption.

We believe these modifications improve both clarity and readability.

Line 290-302: You write microsecond level in lines 293, 296 and 302, and microarcsecond level in line 300. Do you mean microarcsecond level in all cases? I am not sure what microsecond level would mean. Microarcsecond is a small distance, so it seems to me a measure for the size of nutations.

We thank the reviewer for pointing out this inconsistency. You are absolutely correct: we mean microarcseconds in all cases, not microseconds.

The quantities discussed in these lines refer to angular amplitudes of nutations and should therefore be expressed in angular units (microarcseconds). Units such as microseconds or milliseconds would instead apply to variations in the length of day (LOD), which are measured in time.

All occurrences in the nutation case have now been corrected to "microarcsecond" in the revised manuscript.

Figure A1: There are supposedly symbols for Earth and Mars but they appear identical, so couldn't be distinguished. Also, on each graph, there is only one symbol of each kind, it seems, whereas it should be two, if it is for Earth and Mars. Also, same comments as in Figure 1 and 2; it would be helpful to plot the figure bigger.

We thank the reviewer for these comments. The caption of Figure A1 has been revised to clarify the meaning of the symbols and to clearly distinguish between the Earth and Mars cases. The apparent ambiguity in the previous version has now been removed.

In addition, the figure has been provided in vector format and enlarged in the revised manuscript to improve readability and allow appropriate scaling in the final layout.

We believe these modifications address the concerns raised.

Minor comments:

line 21: "from <u>the</u> degree 2-order 2 component" done

Line 65: Not "In a core" but rather "With a core" done

Line 169/170: "properties of spherical harmonic ~~properties~~" done

We would like to express our sincere gratitude to Bernhard Steinberger for his careful and constructive review.