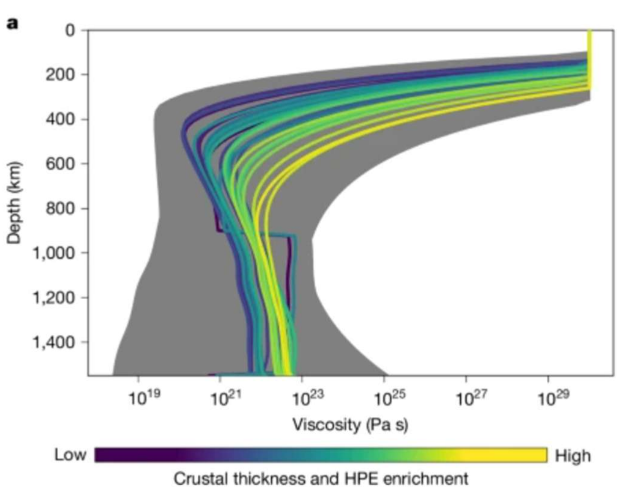
Reviewer 1 – Bernhard Steinberger

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).

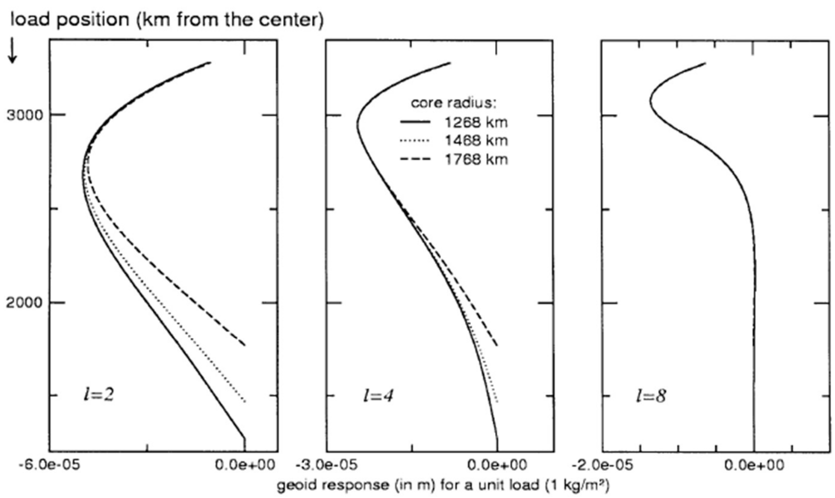


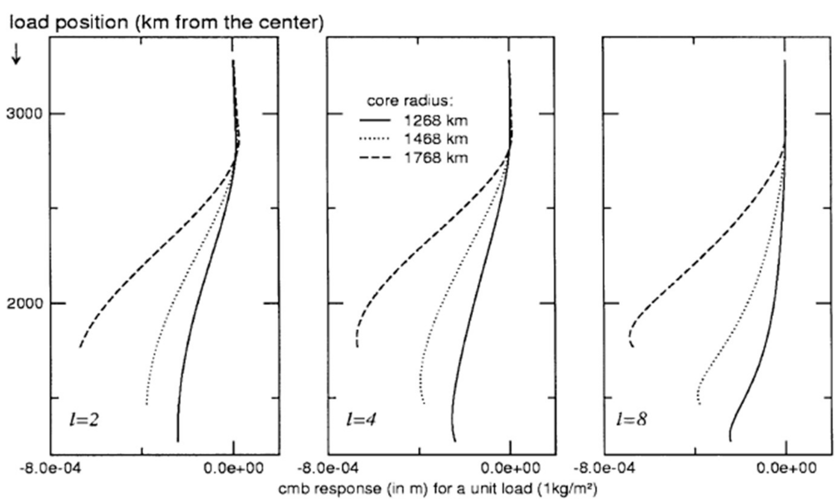
[1] Broquet, A., Plesa, A.-C., Klemann, V., et al. (2025). *Glacial isostatic adjustment reveals Mars's interior viscosity structure.* **Nature, 639**, 109–113.

Defraigne et al. (2001) explicitly investigated the sensitivity of the Green's functions to Martian mantle viscosity by testing several alternative profiles, including:

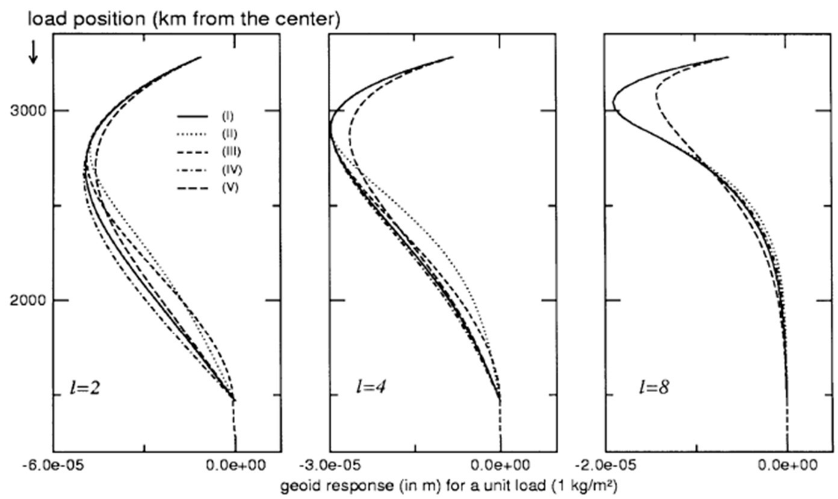
- a constant-viscosity mantle,
- a viscosity increasing linearly with depth (factor of 10 from lithosphere base to CMB),
- a model including a viscosity jump (factor of 10) near the spinel–post-spinel transition,
- and a model with viscosity decreasing linearly with depth.

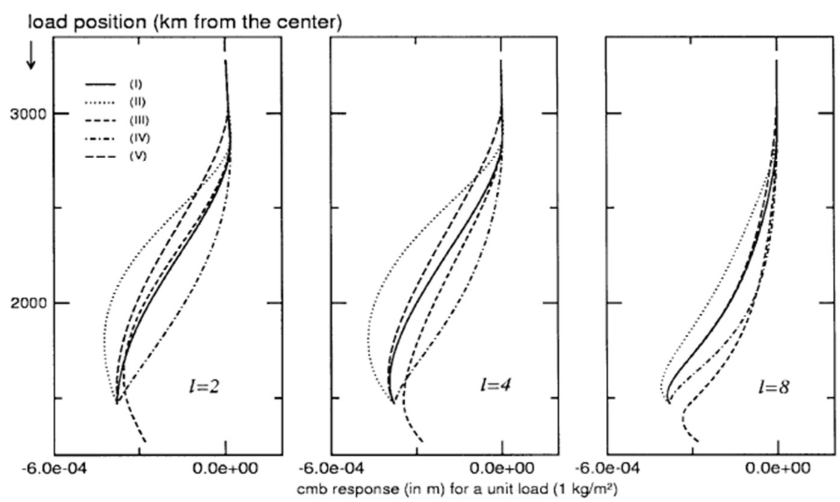
Their results show that the inferred CMB topography amplitude is indeed sensitive to the core radius (see figure just below), and to the viscosity structure (see figure further down), as the reviewer pointed out.





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





(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 $\sigma > 1$; whereas prograde long-period nutation in space appear at frequencies $\sigma < 1$."

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 the 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.

Reviewer 2

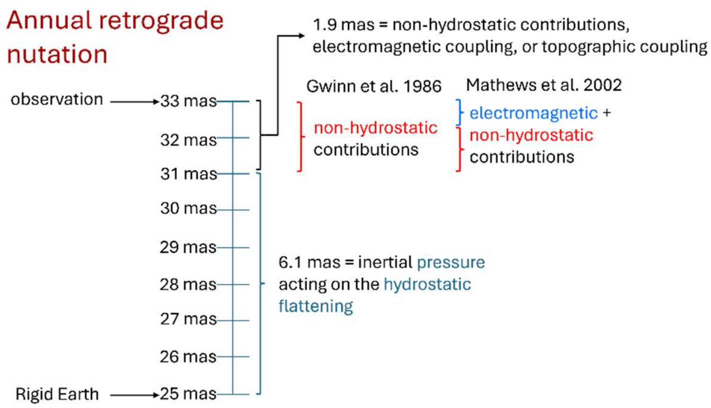
This paper develops a theory for how topography on the core-mantle boundary (CMB) can induce nutations and changes to the length of day (LOD) on Mars. The authors develop a theory that predicts the frequencies of inertial waves in the liquid core, and then anticipates that these resonances can excite torques that excite nutations and changes in the length of day. The theory predicts these excitations at specific frequencies for certain spherical harmonic wavelengths. However, significant nutations and changes in LOD are not observed at these frequencies. This leads to the authors to conclude that CMB topography is not exciting nutations or LOD variations – presumably because the CMB topography is not large enough to excite the associated inertial waves.

This would be much more exciting if the result of this exercise would be to explain an observed nutation or LOD change. Thus, this paper thus presents a somewhat negative result, although it is still potentially important to publish this type of study to save others the time to look for these CMB resonances. Still, the negative result leaves the reader to wonder whether the CMB topography is too small, or if something might be missing from the theory? Given this, it would greatly help the paper to have a sort of "proof of concept" – to show that the theory indeed does predict resonances that produce nutations (or LOD variations) on another planet where the nutations are well understood, such as Earth. It seems to me that this would be relatively easy to do, so I am a bit surprised that the authors did not do this. Even if the result was negative (no nutations at the predicted frequencies) then there is enough known about the Earth that it might be possible to explain why they see a negative result based on knowledge about mantle structure (and associated CMB topography). Thus, I would be much more comfortable with this analysis if it were also applied to the Earth, where much more is known, before applying it to Mars.

We thank the reviewer for this thoughtful and constructive comment. We agree that demonstrating the validity of the approach on Earth provides an important proof of concept before applying it to Mars. In fact, our investigation of Mars was preceded by an extensive study of the same mechanisms for Earth.

The possibility that topography at the core–mantle boundary (CMB) may excite inertial waves in the liquid core was originally motivated by the need to identify additional core–mantle coupling mechanisms for Earth. Nutation observations and variations in the length of day (LOD) provide strong constraints on the deep interior and, in particular, on CMB coupling processes.

Here below the example of the annual retrograde nutation for Earth.



Recent re-evaluations of the coupling constants at the CMB and inner core boundary (Cheng et al., 2026), following an approach similar to Koot et al. (2010), indicate that the required CMB coupling strength is larger than previously inferred. This leaves room for mechanisms beyond viscous and electromagnetic coupling.

In this context, we applied the same analytical framework as used here for Mars to the Earth. We demonstrated that inertial waves can indeed be excited within the terrestrial core by CMB topography. However, for these waves to significantly affect nutations, their eigenfrequencies must be close to nutation forcing frequencies. When evaluating the resonance conditions for Earth, we found that no significant resonance occurs near the dominant nutation frequencies. As a result, the contribution of topographic coupling to Earth nutations was negligible within the linear framework adopted.

For LOD variations, the analytical formulation is simpler. In that case, we identified resonant features that can produce signals at the level of a few tenths of a millisecond.

These results are presented in Puica et al. (2023) and Dehant et al. (2025) and are summarized in the Introduction of the present manuscript.

It is important to note that, in our formulation, the topographic torque scales with the square of the topography amplitude (except for the flattening contribution). This strongly limits its effect unless a resonance condition is satisfied. The absence of significant resonances for Earth nutations therefore naturally leads to a small contribution, consistent with observations.

The remaining discrepancy between observed coupling constants and the contributions from viscous, electromagnetic, and linear topographic torques may instead be explained by nonlinear effects such as form-drag. This possibility is currently being investigated (Rekier et al., 2025), and we have now added a sentence in the Introduction and in the conclusions to clarify this point.

In summary, the application to Earth serves as a proof of concept: the theory does predict inertial-wave resonances and associated torques when the frequency conditions are satisfied. The absence of strong nutation resonances for Earth—and similarly for Mars—is therefore a physically meaningful result rather than a failure of the theory. We have clarified this motivation and the Earth results in the revised manuscript to make this point more explicit.

Furthermore, the authors compare predicted resonant frequencies to the frequencies of nutations with known causes (annual and its sub-harmonics, see line 33). To me, this doesn't make sense because even if these nutations are observed it would be difficult to attribute them to the CMB topography. They are essentially looking for resonant frequencies that are "hiding" behind sub-harmonics of sun-driven nutations. Why not look for nutations in frequency ranges that are not already filled?

We thank the reviewer for this important remark. The key point is that detecting a signal in the orientation of Mars requires an excitation mechanism. Variations in nutation are primarily forced by external gravitational torques, mainly from the Sun and, to a lesser extent, from Phobos and Deimos. Without such forcing, a rotational mode can only be observed if it is excited by the atmosphere or by an internal process of sufficient amplitude.

Free rotational modes (such as the Free Core Nutation, FCN) may in principle be observable if they are excited. For example, on Earth, the FCN is clearly detected in Earth orientation data and is generally thought to be excited by processes occurring in the ocean and atmosphere or within the fluid core, possibly related to geomagnetic jerks. In contrast, no free FCN signal has been detected in the InSight orientation data for Mars. Moreover, Mars lacks a present-day global magnetic field, making internal excitation mechanisms analogous to those proposed for Earth unlikely.

In this context, the most plausible way to detect inertial modes is through resonance with forced nutations. If the frequency of an inertial mode lies close to that of a forced nutation, the resonant response may amplify the signal and make it observable in the orientation data. This motivates our comparison between the predicted inertial-mode frequencies and the known forced nutation frequencies.

We do not attempt to attribute already-identified annual or sub-harmonic nutations to CMB topographic pressure torque explicitely. Instead, we investigate whether any forced nutation frequency lies close to an inertial-mode resonance that could produce an observable amplification. Our analysis includes all possible inertial modes, including the rotational normal mode associated with the FCN. We find that, except for the FCN, none of these modes lie sufficiently close to a forced nutation frequency to produce a detectable resonant enhancement.

We have clarified this reasoning throughout the Introduction to make our strategy and its limitations more explicit. We also indicated the absence of a detected free mode in the InSight data (around line 45).

Some additional discussion of previous approaches to link the CMB with nutations or LOD changes would be helpful. For example, Koot et al. (2010) linked nutations on Earth with CMB coupling (viscous or topographic), but this previous analysis is not cited or discussed. Rekier et al. (2025) recently submitted a paper relating CMB topography to nutations (also on Earth), but it this is also not discussed. Similarly, LOD variations have been suggested to be caused by CMB topography on Earth (e.g., Yoshida and Hamano, 1995, Greff-Lefftz, 2011, and many other papers). Putting this work in the context of this previous work is important to include here.

We thank the reviewer for this important suggestion. The connections between CMB coupling, nutations, and LOD variations have indeed been extensively discussed in our previous studies focused on Earth (Puica et al., 2024; Dehant et al., 2025). However, we agree that this context was not sufficiently summarized in the original version of the present manuscript.

In the revised version, we have expanded the Introduction (around line 80) to provide a clearer overview of previous work linking CMB processes to nutations and LOD variations. We now explicitly discuss the analyses of Koot et al. (2010), which constrained CMB coupling mechanisms from Earth nutation data, as well as earlier studies addressing the role of CMB topography in LOD variations.

In addition, we have added the following sentence to acknowledge recent developments: "Similarly, Rekier et al. (2025) investigated the form-drag effect, which may provide a viable explanation for the observed CMB coupling constant (Koot et al., 2010; Cheng et al., 2026)." Also in the conclusion, we have added "Similarly, Rekier et al. (2025) investigated the form-drag effect, which may provide a viable explanation for the observed CMB coupling mechanisms.".

The revised manuscript now better situates the present study within the broader framework of Earth-based investigations of CMB coupling (see around line 80). Our earlier papers developed the analytical framework and evaluated its implications for Earth; the present work extends this approach to Mars and examines whether similar resonance mechanisms could produce observable rotational signatures.

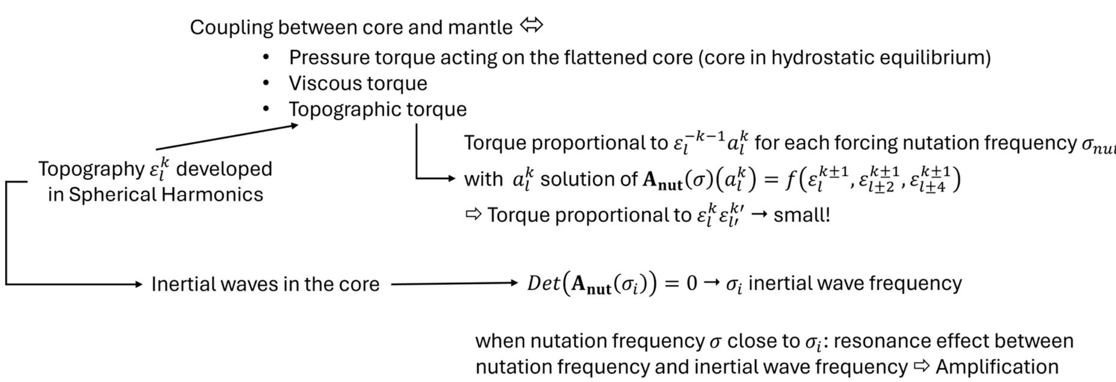
The text of the paper is reasonably well written, but it relies too heavily on equations and tables to make their points. In that sense, the paper is not particularly well presented, and it would help to show figures beyond two small and black and white figures to show the main results. The paper actually incorporates a lot of other information such the power spectra of the Mars's geoid field and the CMB topography kernels (together used to estimate the power spectra of the CMB topography on Mars). I feel that a paper with better use of figures would be more impactful.

We thank the reviewer for this helpful suggestion. We agree that clear and informative figures can significantly improve the presentation and impact of a paper.

In the present study, however, our analysis focuses on only a limited number of spherical harmonic degrees (primarily the highest degrees relevant for the resonance conditions). In practice, we considered all the nutations identified in Baland et al. (2020); however, their periods are much longer than that of the annual nutation, and no resonance appears in the inertial-wave frequency range. Because of this restricted spectral range, additional plots of power spectra or kernels would not substantially enhance the clarity of the main results, as the conclusions are based on discrete frequencies and resonance conditions rather than on broad spectral behavior.

Nevertheless, we recognize that the methodological procedure may benefit from clearer visualization. To address this point, we have added a schematic figure illustrating the overall approach. We believe this schematic improves the readability of the manuscript and makes the logical flow of the analysis more transparent.

We hope that this addition satisfactorily addresses the reviewer's concern regarding presentation.



I give more specific comments below, but given these concerns I am recommending major revisions. I think that a better testing of the theory is necessary before confirming a negative result for Mars. I also think that the authors can make better use of visual aids (figures) when presenting their work – I give some suggestions below.

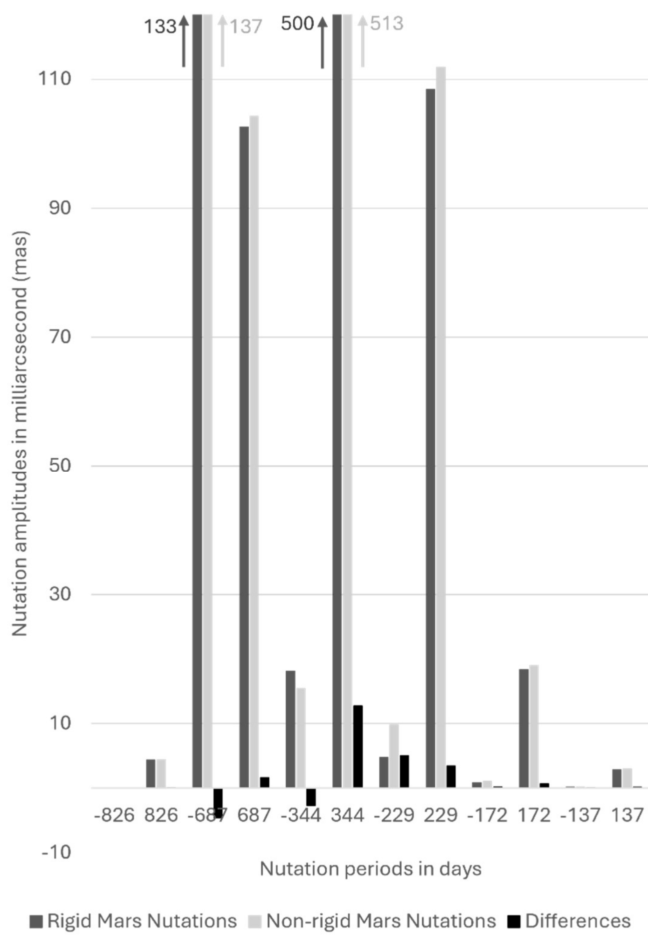
Specific Comments

Line 24 – the authors start the paper by discussing the new observations of nutations on Mars. It would help to show these in a figure (amplitude vs. frequency) – then it could shown which nutations are explained and which (if any) are not. The authors could also show the frequency range where they expect to detect CMB-associated nutation.

We thank the reviewer for this helpful suggestion. In response, we have added a new figure showing the nutation amplitudes (for a rigid and non-rigid planet) as a function of frequency. This figure highlights the nutation components that require the presence of a liquid core and core–mantle boundary coupling to be explained.

We also indicate in the figure the periods at which resonances associated with CMB topography would be expected to occur.

We believe this addition improves the clarity of the paper and helps place our analysis in the appropriate observational context.



Line 52 – the authors state the Mars is the only planet exhibiting a bi-phase terrain constrast. I think this is not true – what about the Moon? Earth has this too, but not distributed in hemispheres (although during supercontinent periods it did have this).

We thank the reviewer for pointing this out. We agree that other planetary bodies, such as the Moon and Earth (at times of supercontinent assembly), also exhibit terrain contrasts, though these are generally less pronounced or not organized into hemispheres as on Mars. To address this, we have revised the sentence on line 59-60 to more accurately reflect the situation:

"This large-scale hemispheric asymmetry is clearly visible on Mars, which exhibits relatively young northern lowlands and heavily cratered, older southern highlands."

This wording emphasizes the hemispheric organization and scale of the contrast on Mars, without implying that such contrasts are entirely unique to Mars.

Line 71 – the authors mention the idea that there is a molten silicate layer at the base of the Martian mantle, and also suggest that this is interesting to consider for nutations. Yet they do not come back to this idea later in the paper – could the presence of this molten layer help to explain why the CMB topography does not induce nutations (because the topography is instead on the lower part of the solid mantle, and separated from the CMB by the molten layer?)? Can topography on the solid-liquid interface (within the mantle) generate nutations? It seems to me that they would be different than at the CMB, since the density contrast is different and they occur at a different radius.

We thank the reviewer for this insightful comment. Indeed, the presence of a molten silicate layer at the base of the mantle could be interesting from a planetary interior perspective. However, with respect to resonance with the Free Core Nutation (FCN), we don't know yet how this layer would change the result.

To clarify, we did not suggest that this molten layer directly affects nutation; we only noted that it may exist. It is correct that the layer does not perturb the position of the liquid–solid boundary relevant for rotational resonance. We have now added a clarifying sentence in the manuscript:

"Adding further interest to the liquid–solid boundary, Khan et al. (2023) and Samuel et al. (2024) recently showed that InSight data require the presence of a fully molten, ~150-km-thick silicate layer overlying the liquid iron core. While this explains seismic observations, it might complicate the determination of the FCN from the geodetic data. This issue is still under study."

Line 74 – here the authors refer to epsilon_l^k and epsilon_l'^k', but none of these terms are defined (epsilon, l, k, l', k'). It would be better not to either define these terms or save this mathematical expression for later (when it can be defined more clearly).

We thank the reviewer for this comment. We agree that the notation was introduced without sufficient explanation. We have now clarified the text by adding (line 95): "when the topography is expressed in spherical harmonics and normalized by the core radius". This provides the necessary context for the terms ε_l^k and $\varepsilon_{l'}^{k'}$ without introducing undefined symbols prematurely.

Line 76 – Here "small" and "higher" are relative terms and their use here is vague and undefined.

We thank the reviewer for pointing this out. We agree that terms such as "small" and "higher" are relative and were not well defined. We have revised the sentence for clarity and precision:

Original: "The pressure torque can therefore be expected to be small, in particular for higher spherical harmonic degrees."

Revised: "The pressure torque can therefore be expected to be much smaller than the pressure torque associated with the hydrostatic flattening of the core ($(\varepsilon_2^0)_{hydrostatic}$), in particular for high spherical harmonic degrees l, as the topography amplitude decreases with increasing degree."

This revision quantifies the comparison and explicitly specifies the dependence on spherical harmonic degree.

Line 81 – The spherical harmonic representation of topography is mentioned – it would help to show this (in a figure) since it is used later when estimating the cmb topography.

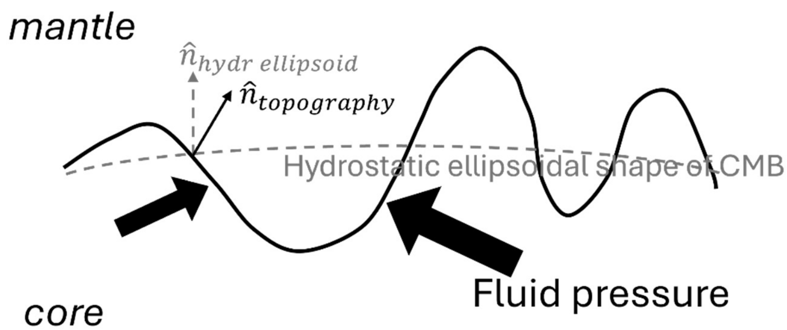
We thank the reviewer for this suggestion. While it is not strictly necessary to show the decay of the spherical harmonic coefficients of the topography (as these are small and have been reported for Earth), we agree that providing a reference for comparison can be helpful. All important contributions from the topography are already reported in Table 2 in meters. In addition, we have now included a hydrostatic reference value to illustrate the scale of the topography:

"Note that, for comparison, if Mars were in hydrostatic equilibrium, the difference between the equatorial and polar radius of the core would be on the order of 6 km."

This addition provides a clear context for interpreting the magnitude of the non-hydrostatic CMB topography.

Line 109 – personally, I think it would help to depict the pressure torque on CMB topography in a figure. (See Fig. 2 of Rekier et al., 2025 for an example).

We thank the reviewer for this helpful suggestion. Following this comment, we have added a schematic figure in the Introduction illustrating the pressure torque acting on the CMB topography. This sketch is intended to clarify the physical mechanism and to complement the mathematical description provided in the text.



Line 200 – this is one of several statements like "Only the frequency of XXXX cycles/day … could affect the nutations". It is not explained why the authors have eliminated nearly all of the resonances identified. This is part of why it would help to show the observed nutations as a function of frequency – so it is more clear why the authors can eliminate most of these frequences. (presumably the others are outside of the relevant range for nutations?)

We thank the reviewer for this comment. Indeed, the other resonant frequencies were excluded because they lie outside the frequency band relevant for nutations. Nutations appear at diurnal frequencies in a frame tied to the planet, and only resonances falling within this specific frequency range can interact with the forced nutation signal. To clarify this point, we have revised the text by adding the following statement at the beginning of the relevant sentence (see line 241): "Because nutations appear at diurnal frequencies in a frame tied to the planet (and following our convention, Eq. (13)), …"

Line 220 – here section 2.1.3 and Table 1 presents other degrees at higher orders, but it is not mentioned if any of these might be interesting for nutations.

We thank the reviewer for pointing this out. We agree that this point was not clearly expressed in the original manuscript. We have therefore reformulated the description of

Table 1 to clarify the role of the higher degrees and orders and their potential relevance for nutations.

The revised text now reads (see line 262): "Also in the general case, the solutions for $a_l^{\pm k}$ exhibit resonances, determined by the roots of the corresponding $[\ldots]_l^{\pm k}$. Table 1 presents the inertial wave period (column 5), along with the degree l and order k of the spherical harmonics component of the topography generating them (column 4), as well as the potential nearby nutation period (columns 1, 2, and 3). The table also provides the difference, in days, between the nutation period and the inertial wave period (column 6)."

Line 239 – The authors make an argument that they can relate geoid anomalies at Mars's surface to the CMB topography, based on the kernel that links them. In general, this only works for a simplified mantle flow, without lateral viscosity variations or compositional variations. On Earth, where there may be both types of heterogeneity present near the CMB, there can be (potentially large) topography on the CMB associated with isostatic topography (of the LLSVP regions), or the flow near them. See Heyn et al., 2020 for a more in-depth discussion of this topic for Earth. For Mars, much less is known so it is hard to know what to assume– still, it would help to discuss this, to give context for what the type of complexity that could be relevant for Mars.

We thank the reviewer for this important remark. We agree that the relationship between surface geoid anomalies and CMB topography relies on assumptions regarding mantle structure and dynamics, in particular the absence of strong viscosity variations or compositional heterogeneities.

As correctly noted, on Earth such complexities—especially near the CMB—can significantly affect the inferred topography, for example in association with LLSVP regions or mantle flow patterns (e.g., Heyn et al., 2020). For Mars, the internal structure is less well constrained, and similar complexities cannot be excluded.

In response to this comment (and a related remark from another reviewer), we have expanded the discussion in the manuscript to clarify the assumptions underlying the use of the geoid–CMB topography kernel and to acknowledge the potential limitations associated with unknown mantle heterogeneity. This additional discussion (see line 287) provides context regarding the mantle viscosity and the types of structural complexity that could influence the inferred CMB topography on Mars.

Line 284 – Finally the first figure. This figure shows the frequencies of the nutations. However, the figure is presented poorly – it is small, and the symbols are very difficult to distinguish. It would be better to make use of color here. Furthermore, only the zero-crossings are discussed - does the vertical amplitude have any meaning? The y-axis is only labelled “y” – what does this mean and what are the units? No units are given for the x-axis either, only the symbol “sigma”. I think the authors could be more imaginative about how to present the predicted resonances, instead of simply showing the determinant. Finally, the “caption” is referred to in the caption – I think they mean “legend” here.

We thank the reviewer for these helpful remarks, which are consistent with comments raised by the other reviewer.

We have revised Figure 1 (which became Figure 4) accordingly. The figure is now provided in vector format, allowing the editor to adjust its dimensions to ensure optimal readability in the final layout. We have also improved the clarity of the symbols and labeling, as well as the figure caption.

The axes have been clarified in the revised version. In particular, the x-axis (σ) is now explicitly defined in the caption and text, and units are provided. We also clarify that the relevant frequencies for nutation correspond to diurnal frequencies in a frame tied to the planet, whereas LOD variations are treated differently. This distinction is now clearly explained in the text to avoid confusion (see line 338).

As the reviewer notes, the zero-crossings are the relevant quantities for identifying resonances; the vertical amplitude itself does not carry direct physical significance in this context, and this is now clearly explained. We have added a sentence mentioning that in the text (see line 336).

Finally, we have corrected the terminology in the caption: “caption” has been replaced with “legend.”

We believe these revisions improve the clarity and presentation of the figures.

Line 302 – I think the authors mean “microarcsecond” instead of “microsecond” here.

We thank the reviewer for pointing this out. This was indeed a typographical error (also noted by the other reviewer). “Microsecond” has now been corrected to “microarcsecond” in the revised manuscript (see lines 352 and 355).

Line 322 – A list of tidal frequencies is given – are these the same as the six points shown in Figure 2? It would help to label these so the tidal frequencies could be connected to these points (not so easy to relate Martian days to whatever units are used for sigma on the x-axis).

We thank the reviewer for this comment. Yes, the tidal frequencies listed in the text correspond to the same six tidal contributions shown in Figure 5 (formerly Figure 2), which are identified by the different symbols in the legend.

In the figure, the curve represents the determinant of the matrix A_{LOD}. Resonances occur when this curve crosses the zero line on the x-axis, which defines the frequencies of the inertial waves. The proximity of these zero-crossings to the tidal frequencies (indicated by the symbols in the legend) determines the potential strength of the resonance: the closer the crossing to a given tidal frequency, the stronger the possible resonance effect.

To clarify this, we have revised the text and expanded the figure caption. The manuscript now states: "**Error! Reference source not found.** shows the determinant of the matrix A_{LOD}. As in **Error! Reference source not found.** (nutation-case), the presence of resonances in LOD is indicated when the curve crosses the zero line on the x-axis. The closer these crossings are to the tidal periods involved in LOD (and identified by the symbols in the legend), the stronger the potential resonance effect."

We believe this clarification makes the relationship between the listed tidal frequencies and the plotted points explicit.

References

Greff-Lefftz, M., Length of day variations due to mantle dynamics at geological timescale, Geophysical Journal International, Volume 187, Issue 2, November 2011, Pages 595–612, https://doi.org/10.1111/j.1365-246X.2011.05169.x → was seen in the frame of the paper Puica et al. (2023), but is more related to long-term behavior than present-day LOD variations.

Heyn, B. H., Conrad, C. P., and Trønnes, R. G., 2020, Core-mantle boundary topography and its relation to the viscosity structure of the lowermost mantle: Earth and Planetary Science Letters, v. 543, p. 116358. → very interesting but not added as we discuss here more Mars than Earth.

Koot, L., M. Dumberry, A. Rivoldini, O. De Viron, V. Dehant, Constraints on the coupling at the core–mantle and inner core boundaries inferred from nutation observations, Geophysical Journal International, Volume 182, Issue 3, September 2010, Pages 1279–1294, https://doi.org/10.1111/j.1365-246X.2010.04711.x → added

Rekier et al., Constraints on Earth' Core-Mantle boundary from nutation (arXiv: https://arxiv.org/html/2507.01671, 2025). → added

Yoshida, Y., and Y. Hamano (1995), Geomagnetic decadal variations caused by length-of-day variation, Physics of the Earth and Planetary Interiors, 91, 117-129, https://doi.org/10.1016/0031-9201(95)03038-X. → was in Puica et al. (2023)

Thank you very much. We knew some of these references and some of them were in our previous Earth-related paper. However, we did not know the paper of Heyn et al. (2020), which is very interesting.

We would like to express our sincere gratitude to this anonymous reviewer for his/her careful and constructive review.