Response to Reviewer #1 – Second round

Review for "The JUICE 2024 close flyby of the Moon: Thermal assessment from MAJIS":

The authors have thoroughly revised the manuscript and addressed my suggestions and concerns from the review. The manuscript has improved significantly, and I am satisfied with most responses. I have a few minor suggestions that should be addressed before acceptance.

Minor comments

1) The manuscript does not address the covariance between emissivity and temperature in the retrieval framework. It appears these cross-covariances have been omitted or consid- ered negligible. Please clarify this in the manuscript.

Response: We thank the Reviewer for this useful comment. We clarify that, in the present implementation, no a priori cross-covariances between temperature and emissivity are prescribed. The observation error covariance matrix S_e is assumed diagonal, with wavelength-dependent variances given by the squared in-flight NESR, and the a priori covariance matrix S_a is also taken as diagonal, with independent prior variances assigned to temperature and emissivity. Thus, any temperature–emissivity coupling is not imposed in the prior, but arises through the forward model and is reflected in the posterior covariance. We have revised Sect. 2.1 accordingly to make this explicit.

2) The following statement in the response to my comments:

"The coupling between temperature and emissivity is naturally represented through the Jacobian and the posterior covariance, which captures their correlation in the retrieved state."

This statement is not technically accurate. The Jacobian matrix shows how measurements respond to changes in state variables, but it does not directly represent the relationship between temperature and emissivity. The posterior covariance matrix is not part of the retrieval process itself; it quantifies the uncertainties and correlations among the retrieved parameters.

A clearer explanation is that when the Jacobian sensitivities for temperature and emissivity are similar, the inversion process becomes more ambiguous. The posterior covariance matrix reflects the resulting uncertainties and correlations.

Including a plot of the diagonal elements of the averaging kernel matrix would be helpful. These values indicate the contributions of observations and prior to the retrieved state, making it easier to assess retrieval sensitivity and information content.

Response: We thank the Reviewer for this important clarification. We agree that our previous wording was not technically precise. In the revised manuscript, we now state more clearly that the Jacobian matrix describes the sensitivity of the measured radiance to perturbations in temperature and emissivity, and that ambiguity in the inversion arises when these sensitivities become similar in the crossover regime. The posterior covariance matrix is then used to quantify the resulting uncertainties and correlations in the retrieved state; it is not itself part of the prior constraint applied during the retrieval. We have revised the text accordingly.

Regarding the Reviewer's suggestion to include averaging-kernel diagnostics, we agree that such quantities are in principle useful to assess information content. However, in the present implementation the state vector contains one temperature plus N spectral emissivity parameters per pixel, so the full averaging kernel is high-dimensional and would be cumbersome to present in a concise and interpretable form for the scope of this paper. We therefore chose not to add a dedicated averaging-kernel figure in the revised manuscript. Instead, we clarify more explicitly how the prior covariance regularizes the inversion and how the posterior covariance should be interpreted.

3) In Fig. A1(a) and A1(c), the observed radiance and NESR appear to have nearly the same magnitude. Please recheck these plots and clarify the corresponding SNR values. If the observed radiance is comparable to the NESR, the resulting SNR would be close to unity, indicating very poor measurement quality.

In their response, the authors state:

"However, for the selected best-case pixel, its amplitude is much smaller than the measured/modeled radiance in the 4.5-5.5 µm interval, and therefore it appears nearly overlapped by the other curves at this plotting scale."

This explanation is difficult to reconcile with the figures. If the NESR amplitude is much smaller than the observed radiance, the two curves should not overlap closely unless the plotting scale compresses the differences. Please clarify the plotting scale and provide quantitative SNR estimates for the displayed spectra.

Response: We thank the Reviewer for this helpful remark. We agree that, in the previous version of Fig. A1(a,c), the plotting style could give the misleading impression that the NESR was comparable to the observed radiance. In the revised figure, we now clarify that the blue curves represent the measured radiance ± NESR, rather than the NESR itself. We also report quantitative SNR diagnostics over the 4.5–5.5 µm interval for the two example pixels. For the best-case pixel, the median SNR is ~2541, with a 5–95 % SNR range of ~294–8061. For the suboptimal pixel, the median SNR is is ~126, with a 5–95 % SNR range of ~30–907. These values confirm that, even for the noisier example, the observed radiance remains well above the instrumental noise level, and that the apparent visual proximity of the curves in the previous version was primarily a consequence of the plotting style rather than an indication of SNR ≈ 1. The figure and caption have been revised accordingly.

Response to Reviewer #2 – Second round

Review for "The JUICE 2024 close flyby of the Moon: Thermal assessment from MAJIS":

The manuscript has been significantly improved, and the authors have thoroughly addressed my prior points. I recommend publication following these minor technical corrections:

We thank the Reviewer for this positive overall assessment and for recognizing the substantial improvements made to the manuscript. We also appreciate the constructive nature of the remaining comments, which we address below as minor technical revisions.

Line 582-585: Reword to: "This outcome is consistent with results from previous studies." (Replace "expectations" with "results").
Please also refine the discussion of previous studies to account for varying viewing geometries. Studies using nadir configurations (e.g., Wohlfarth et al., 2023; Bandfield et al., 2015) yield lower values (~20° for Diviner, 20–25° for GF-4), whereas off-nadir approaches generally yield 30–35°. Explicitly stating that the MAJIS setup closely matches the nadir geometry will better contextualize your findings against the literature.

Response: We thank the Reviewer for this helpful suggestion. We agree that the comparison with previous studies should better account for differences in viewing geometry, and we have revised the text accordingly. In particular, we replaced "expectations" with "results" and clarified that published roughness estimates depend on both observing geometry and retrieval approach. We also specify that the roughness values discussed here are derived from the C4 dataset (Table 2), which was acquired under distinctly off-nadir viewing geometry, whereas only C1 and, to a lesser extent, C2 are closer to near-nadir conditions. The discussion has therefore been revised more cautiously, placing our retrieved values in the broader context of geometry-dependent roughness estimates rather than directly associating them with strictly nadir observations.

Lines 565,636,641,646,655: The authors use the term 'macroscopic roughness' to describe roughness at meter scales (e.g., at the resolution of LRO/LOLA). While technically accurate, this terminology may introduce ambiguity. Because 'macroscopic roughness' can refer to anything from a few millimeters up to several meters, it is highly context-dependent. In the literature, one commonly finds these overlapping definitions:

1) Microscopic roughness: Roughness at the spatial scale of individual mineral grains (micrometers up to ~millimeters).
2) Macroscopic roughness: Broadly, anything larger than microscopic roughness. In planetary photometry, this often describes any unresolved variation larger than individual grains.
3) Unresolved/Photometric roughness: Any topographic variation that is not explicitly resolved by the sensor.
4) Thermal roughness: The scale of roughness that dominates anisotropic thermal emission and is typically addressed by thermal models (generally in the mm to cm range).

Given these varying definitions across sub-fields, I recommend that the authors avoid the blanket term 'macroscopic roughness' and instead use explicitly quantified terms. For example, use 'mm-cm scale roughness' to describe the scales that dominate thermal behavior, and 'meter-scale roughness' to refer to the larger topography captured by LOLA.

Response: We thank the Reviewer for this useful suggestion. We agree that the term "macroscopic roughness" may be interpreted differently across sub-fields and could be ambiguous in the present context. We have therefore revised the manuscript to use more explicit scale-based terminology throughout this section. In particular, we now refer to the retrieved thermal roughness as mm–cm scale roughness (or sub-pixel mm–cm scale roughness) and to the LOLA-derived quantity as meter-scale roughness. The corresponding wording has been updated in the main text and in the captions of Figures 8, 11 and A2 to avoid ambiguity.

Lines 899–904: The argumentation here needs clarification. The text states that there is no scene-wide absorption at 3.5 µm, but this appears to contradict the provided plots, where an absorption feature is clearly visible and it says the specta have been averaged over the scene. Please clarify.

Response: We thank the Reviewer for this remark. The apparent contradiction arises because two different quantities are being discussed. Figure 15 shows retrieved spectral emissivity, whereas the diagnostic check was performed on the input spectral radiance cube. Our statement refers specifically to the absence of a scene-wide absorption centred at ~3.5 µm in the median radiance spectrum of C4. This does not imply that a local or terrain-dependent structure cannot appear in the retrieved emissivity, especially within the 3.0–4.7 µm crossover region, where the separation between reflected and emitted components is ill-conditioned and small residuals in calibration or reflectance modelling can be amplified by the inversion. We revised the text to make this distinction more explicit.

Lines 904–919: This is a highly valuable comparison, but its main takeaways are currently missing from the conclusion. Given its importance to the paper, I recommend summarizing these comparative findings in the Conclusions section.

Response: We thank the Reviewer for this useful suggestion. We agree that the comparison discussed in Sect. 2.5.2—particularly the terrain-averaged spectral emissivity profiles shown in Fig. 15 and their qualitative comparison with Apollo soil spectra—deserves a more explicit summary in the Conclusions. In the revised manuscript, we have therefore added a short passage clarifying that Fig. 15 primarily supports a reproducible relative emissivity offset among the selected terrains within the thermally dominated part of the MAJIS range, while the 3.0–4.7 µm crossover interval remains more sensitive to modelling assumptions and calibration residuals. This addition makes the main takeaway of the comparison more explicit in the concluding section.

Lines 985–992: This is a particularly interesting and robust finding. (Note: It is always good practice to leave a positive comment as is; it shows you are reading thoroughly and acknowledging their good work!)

Response: We thank the Reviewer for this positive comment.

Line 994: The phrase "precise formal uncertainties" seems to contradict the earlier discussion of a severe, untraceable artifact around 3.5 µm. Please clarify how the uncertainties can be considered "precise" given the presence of this artifact, or consider softening the phrasing to better reflect the limitations of the data.

Response: We thank the Reviewer for this important remark. We agree that the phrase "precise formal uncertainties" could be interpreted too broadly in light of the limitations discussed for the crossover region, including the apparent ~3.5 µm structure. In our intent, "formal uncertainties" refers specifically to the posterior uncertainties derived from the inversion under the adopted noise and prior assumptions; it does not include systematic effects related to calibration residuals, reflectance modelling, or reflected/thermal separation. To avoid overstatement, we have softened the wording in the Conclusions and clarified that these formal uncertainties apply under favourable conditions, while additional modelling and calibration limitations remain, especially in the crossover interval.

Lines 994–1008: This final section reads a bit too colloquially and borders on promotional language. Please revise to be more concise, ensuring the text maintains an objective, scientific tone throughout the conclusion. Please also double-check that abstract and conclusions are consistent.

Response: We thank the Reviewer for this useful comment. We agree that the final paragraph of the Conclusions could be made more concise and more strictly objective in tone. In the revised manuscript, we have therefore softened the wording, removed expressions that could sound overly promotional, and streamlined the final section to emphasize the main methodological and scientific results without overstatement. We also re-checked the consistency between the Abstract and the Conclusions and revised the wording accordingly.