

Reviewer#2

General comments:

The manuscript presents a valuable analysis of High Accuracy Accelerometer measurements acquired during the JUICE lunar flyby. The various signals, whether expected or unexpected, are carefully examined, and the authors provide well-supported explanations for them. However, the estimation of the sublimated water mass should be strengthened and better justified. Below, I provide several comments and suggestions aimed at improving the clarity, consistency and overall quality of the manuscript.

We thank the reviewer for their careful reading of the manuscript and for their constructive comments, which helped us improve the quality and clarity of the paper. Here below the authors addressed all the issues.

Specific comments:

The Earth flyby immediately following the lunar flyby is not discussed in the manuscript. Did the HAA remain off, or was the closest approach too far for a relevant HAA analysis?

As the reviewer correctly pointed out, the Earth flyby took place approximately 24 hours after the lunar flyby. However, HAA was switched off about one hour after JUICE's closest approach to the Moon and remained OFF during the subsequent Earth flyby. This operational strategy was adopted because the predicted Earth flyby altitude was considered too high (6840 km above the surface) for HAA to measure any significant acceleration signal.

Page 6, line 296: Please provide a few details about the calibration of HAA data, or at least a reference.

The HAA data calibration consisted of applying a low-pass filter with a cutoff frequency of $f_c = 10^{-1} \text{ Hz}$ to consider only frequency inside the instrument bandwidth, followed by a parabolic detrend and a correction for thermal effects induced by temperature variations. The thermal compensation step was required because the instrument's thermal control system was not activated during the measurements. This correction was performed using the thermometer readings, which were converted into equivalent acceleration signals and subsequently removed from the data. The conversion factor was determined to minimize the residuals of the calibrated acceleration after subtraction of the expected acceleration signal.

These details have been added in the text.

Page 13, lines 361-362: The authors should clarify whether HAA data was used to fit the radiometric data. Was the thermal snap modeled as discussed in section 4.2.2?

We thank the reviewer for this request for clarification. HAA data were not included in the orbital determination process, because the only available radiometric measurements were obtained in X/X band, whose accuracy provided only limited sensitivity to the spacecraft outgassing signature.

Consequently, the inclusion of HAA data in the orbit determination fit was not expected to provide any appreciable improvement in the estimation of the outgassing signal.

However, a clarification about this has been added in the manuscript.

The thermal snap has not modelled because is an event with null average, with no detectable effect on the orbit.

Page 13, lines 375-376. This sentence should also clarify what is meant by being "consistent with [...] null ΔV_i ."

Due to the limited quality of the X/X radiometric data, the estimated central value of the delta velocity due to the outgassing event was within the related formal uncertainty. In this sense the estimated ΔV has been stated as "consistent with null value".

Page 13, line 383-384. The authors mention that the data collected by Acc1 and Acc2 are noisier than Acc0. Nevertheless, is it possible to provide an upper bound to ΔV_x and ΔV_y ? Lines 405-406 indicate that they are significantly smaller. Providing bounds would help the reader.

As the authors wrote in the paper, the Acc1 and Acc2 channel are strongly affected by thermal effects degradation, which are very difficult to calibrate and disentangle from the faint outgassing signal. Due to this, is difficult to provide any quantitative information about the ΔV_x and ΔV_y using the accelerometer data.

However, because the outgassing signature is expected to occur at frequencies different from those associated with thermal distortion effects, it was possible to verify that no compatible outgassing signatures were detected in the X and Y channels that

could be superimposed on the thermal signal. An upper bound for the ΔV_x and ΔV_y can be defined from the formal uncertainty of the orbit determination considering 3σ :

- $|\Delta V_x| \leq 0.3 \text{ mm/s}$
- $|\Delta V_y| \leq 5.4 \text{ mm/s}$

Note that the lower value observed along the X axis is a consequence of the spacecraft line-of-sight direction being aligned with the spacecraft X axis.

The text has been updated accordingly.

Page 13-14, lines 385-403: The approach followed by the authors to estimate the mass of the sublimated water appears similar to that adopted by Sandford et al. (2020) for the Osiris-REx Sample Return Capsule,. This should be stated explicitly.

The text has been updated accordingly.

Sandford et al. (2020) report a typical value for $V_{lim} = 1000 \text{ m/s}$ (the same given by the authors), associated with a range of modelled capsule temperatures. The authors of this manuscript should clarify why they chose $T=300\text{K}$. Given the probable location of the outgassing event, is there a more reliable estimate or measurement of the local surface temperature?

In addition, the authors should provide a reference for Eq. (7). With $T=300\text{K}$ and $R=461.5\text{J/kg/K}$, I obtain $V_{lim} = 1056 \text{ m/s}$, rather than 1000m/s . The authors should also clarify why the choice $V_{sub} = V_{lim} / 2$ (line 398).

Sandford et al. (2020) state that the velocity of H_2O outgassing can be up to half the theoretical V_{lim} . Therefore, the inferred sublimated mass should be given as a range. This is currently only addressed indirectly. Please revise this section accordingly. The conclusion on line 424 should also be revised, since "around 8g" is an upper bound.

We agreed with the review that local surface temperature would be a more reliable value for the temperature at zero velocity. However, it's difficult to assess the outgassing location and even more the associated temperature.

We also want to highlight to the review that what we want to assess in the paper is an estimation of order of magnitude for the outgassed mass, for this reason we provided a sort of range for it.

It has clarified better in the manuscript.

The authors added a reference for Eq. (7). The value of $V_{\text{sub}} = V_{\text{lim}} / 2$ comes from indications given by the paper in Sandford et al. (2020), as stated by the reviewer. This value has been used to give an upper limit of the amount of mass outgassed. It has been clarified in the manuscript.

Conclusion: The potential interference cause by resonance modes of different spacecraft substructure should be mentioned in the conclusion. The authors should also state that the impact of this effect on the science results deserves further investigation, possibly in future work.

The text has been updated accordingly.

Technical corrections:

Please check the consistency of the vector notation in figures, equations, and text.

Page 1, lines 17-18: "JUICE will perform several flybys, including the first-ever combined Lunar-Earth Gravity Assist (LEGA), which occurred in August 2024.". The sentence mixes future and past: "will perform" suggests that all flybys are still upcoming, whereas the LEGA already occurred in August 2024.

The text has been updated accordingly.

Page 1, line 21: "Submilimeter" -> "Submilimetre".

The text has been updated accordingly.

Page 2, line 56: Since it is not clear whether "This phase" refers specifically to the orbital phase around Ganymede, the statement "unprecedented insights into the moons' geology" may be ambiguous. Do the authors mean Ganymede specifically, or the three outer Galilean moons more generally? Please clarify.

The text has been updated accordingly.

Page 2, line 77: “deals about” → “deals with”.

The text has been updated accordingly.

Page 4, Table 1: There is a typo in the unit of the intrinsic noise floor.

The text has been updated accordingly.

Page 4, line 160: “till” → “until”.

The text has been updated accordingly.

Page 7, Fig 4: Please add to the caption that the modelled gravity gradient is superimposed on the HAA calibrated data, so that the caption is self-contained.

The text has been updated accordingly.

Page 8, line 235: These displacements are inversely proportional to the thickness h .

The text has been updated accordingly.

Page 8, Equation (5): Please define $\Phi_{1\text{AU}}$ and D_s .

The text has been updated accordingly.

Page 9, line 263: “Here, $C_{p,f}$ and $C_{p,f}$ are” → “Here, $C_{p,f}$ and $C_{p,b}$ are”

The text has been updated accordingly.

Page 9, Table 2: Unit notations “J/kgK” and “W/mk” are ambiguous. Following the current style of the paper (e.g., Table 1), I recommend “J/kg/K” and “W/m/k”.

The text has been updated accordingly.

Page 9, line 289: “deg” → “°”.

The text has been updated accordingly.

Page 9, line 294: Please refer to the red area in Fig. 4 for clarity.

The text has been updated accordingly.

Page 10, Fig. 9: Please add to the caption that the vertical dashed lines indicate the proper modes of the different components. The MAGboom #1 and #2 should also be indicated more clearly.

The text has been updated accordingly.

Page 10, line 304: "a HAA" → "an HAA".

The text has been updated accordingly.

Page 11, Fig. 10: Is the blue area intended to highlight the time interval during which outgassing is suspected? The figure would benefit from clarifying that, e.g., in the caption. The caption should also indicate that the red circles correspond to SWI operations.

The text has been updated accordingly.

Page 11, line 317: "Fig. above" → "Fig. 10".

The text has been updated accordingly.

Page 11, line 318: "Submillimeter" → "Submillimetre"

The text has been updated accordingly.

Page 11, line 324: "mode" → "modes". Additionally, the resonance modes should be formatted as "(0.44, 0.46, and 2.6 Hz)" and "(0.13, 0.21, and 0.89 Hz)".

The text has been updated accordingly.

Page 12, Fig. 12: Please define RW in the caption: "reaction wheels (RW)".

The text has been updated accordingly.

Page 13, line 370-371. Please specify which version of GODOT was used.

The text has been updated accordingly.

Page 13, line 373: "radio metric" → "radiometric".

The text has been updated accordingly.

Page 13, line 374. "too high covariance matrix" is imprecise. The authors should refer to specific quantities, e.g., large covariances.

The text has been updated accordingly.

Page 13, line 378: Please introduce FD as Flight Dynamics earlier in the paragraph.

The text has been updated accordingly.

Page 13, lines 381, 397. Why is DV used instead of ΔV , especially since ΔV_x and ΔV_y are used elsewhere?

The text has been updated accordingly.

Page 14, line 395. Please specify the value of the specific gas constant R.

The text has been updated accordingly.

Page 14, line 412: "sensible masses" → "proof masses".

The text has been updated accordingly.