LATE ADDENDUM

During the review time, we have found another clue of lightning in an image taken by the navigation camera onboard the same Juice spacecraft (NavCam). Even if it is a facility instrument, not suitable for quantitative retrievals, we interpret the fact that we see a lightning flash on exactly the same cloud as MAJIS (although a bit separated in both space and time) as a confirmation of the MAJIS observation, and we think the manuscript can benefit for a small addiction.

Therefore, we added in the revised manuscript version one figure (Fig.14) and the relative paragraph, located at the end of Sect 4.5 (“Search for independent lightning detection”).

We list below our point-by-point answers (in red) to reviewers’ comments (in grey).

Response to Reviewer # 1

We thank the referee for his meaningful review, which highlighted some minor points to be more clearly explained, and let us know some more bibliographic materials.

Not mentioned are the effects of lightning in the radio spectrum, prominently VLF whistlers. These have been observed historically and numerously, on the ground and also in space. JUICE has also the RPW (radio and plasma waves), this could enable coordinated optical and radio waves analysis and studies of lightning. However, this can be left for the future. The manuscript is already quite rich. I recommend to publish the work after only minor comments.

Both historical and more recent radio and microwave measurements at Jupiter (and other planets) are recalled at the beginning of Sect.4.6. Synergies with RPWI on this topic are of course expected to be very valuable for Juice observations, but we agree with the reviewer that deeper discussion is out of purpose of this manuscript, which only includes a preliminary view of the main signatures of Jovian lightning expected in MAJIS data. It is also worth noting that the probability of a coincident detection, e.g. by MAJIS and RPWI, of the same lightning event is reduced by the strict constraints on both spacecraft position and MAJIS pointing/timing. Conversely, general synergy in global statistical analyses could be more easily envisaged. A comment on that is added at the very end of the conclusion (Sect.5).

Minor comments:

Lines 153-156: "It has been obtained by rotating the line of sight by about 4° (2° of rotation of the internal mirror) in steps for a total time of seconds. At every step (i.e. every 200 ms), a 128-pixels spectral frame encompassing 1016 wavelengths has been acquired, with an integration time of 22 ms." This is a bit confusing for me: Does it mean, that there are gaps in time, in a 200 ms long step the integration happens for 22 ms, i.e. about 11% of the time. At the other time (~89%) no integration of light is going on? I understand that the motion of the mirror is adjusted to the distance to the planet/moon (11500 km at EGA) and spacecraft rotation such that there spatial gaps are avoided: pixel width on Earth ~1.7 km (2 km with motional smearing). The duration of lightning flashes measured on the ground varies, a median duration of 0.52 s is given in Kákona et al., 2023, so the 200 ms step duration would be no problem, However, a flash typically consists of much shorter "strokea". This temporal characteristics of lightning flashes is discussed in section 3.5, but the possible effects of the integration in time by MAIJIs with gaps (if I understood correctly) and of relatively slow sampling (200 ms) could perhaps be elaborated on a bit more.

We confirm that the MAJIS time sequence contains long gaps among acquisitions: obs[22ms],gap[178ms],obs[22ms],gap[178ms],etc... and that this was done in order to avoid spatial gaps on the surface, given the high flyby speed. The temporal issue of the observations arises from the fact that we cannot know what was the true temporal variability of the flash sequences, what number of strokes they were made of, their lifetime and exact timing. This is why we could only derive the integrated energy as a lower limit (only that registered during acquisitions, not gaps) and we have large discrepancies in temperature retrievals. We added some more details on this in Sect.3.5 to make this point clearer.

Line 301: "kelvin" --> "Kelvin"

No problem in changing it, even if, in the usage as measurement unit, the lowercase should be more appropriate (see e.g. https://chec.engineering.cornell.edu/units-of-measurement/). We leave it to the editor's choice.

Section 3.3 Oxygen lines
A recent publication with temperature estimates in lightning based on atomic oxygen lines (observed at the ground) is by Wemhoner et al. (2026). I'm not sure how relevant this would be for the discussion in this manuscript, and suggest to the authors to have a look at this paper.

We thank the reviewer for this suggestion. Wemhoner et al. (2026) retrieve 17600 K as average peak temperatures for CG flashes, in line with our large retrieved range. A citation has been in Section 4.3.3.

Section 3.5. Temporal resolution
Kákona et al. (2023), already mentioned above, might be a fresh reference with some relevance for this discussion.

About flashes' time evolution we already cite the work of Peterson&Rudlosky(2019) which is not outdated. In order to have a second reference we find Lopez et al.(2017, https://doi.org/10.1016/j.atmosres.2017.06.030) more appropriate and we add it to the manuscript. After deeper investigation, we find that Kàkona et al. (2023) results do not differ much in terms of mean values, but the very high variability they reported could raise some methodological doubts we prefer not to address here.

Response to Reviewer #2

The authors are grateful to this referee as well for his very accurate review, including interesting comments useful for improving both clarity and coherence of the manuscript.

Specific comments

l. 52: Can the authors clarify what they mean by “the first detection of its kind for any planetary atmosphere”? Lightning in planetary atmospheres has already been observed, for instance from the ISS (see caption of Fig. 4).

This is apparently the first time that lightning flash spectroscopy is achieved from space. Other published lightning observations from space are always consisting of images in either broad filters (like e.g. detection on Jupiter by spacecraft cameras) or narrow filters (i.e. the lightning-dedicated satellite platforms on Earth). At our knowledge, the measurement closest to the MAJIS one has been obtained by Juno UV spectrometer on Jupiter (we acknowledge it at the end of Section 4.1, Giles+2020,doi:10.1029/2020JE006659), which measured light emission spectra across H_2 Lyman band (their fig.5). Anyway, besides being in a much narrower spectral range, the similarity to auroral emission spectra comes out in favor of transient events other than intracloud lightning. On the other hand, as somehow detailed in Section 4.1, Earth lightning spectroscopy is extensively studied but only by ground-based observations.

We rephrase the sentence a bit to make this clearer.

l. 63–64: "Temperature estimates, more sensitive to observational biases, yield a broad range of values (…)" – this sentence could be clearer. The temperature of what is being referred to? Could the authors also provide examples of observational biases?

"temperature of the lightning channel", added.

"Observing biases" referred to the spectral and temporal resolutions mentioned at the beginning of the paragraph. Rephrased to make it clearer.

l. 100: "very low flyby": can the authors specify the corresponding altitude range?

Done.

l. 129: Can the authors define the acronym FWHM upon first use?

Done.

l. 134–137: The authors cite seven papers describing the instrument. This seems excessive for an overview. Are all of these references equally relevant?

The listed papers are focused on different aspects of instruments features, calibrations, performances and operations. Filacchione et al. (2024) deals with spatial calibration; Haffoud et al. (2024) with spectral calibration; Langevin et al. (2024) deals with radiometric calibration; Rodriguez et al. (2024) deals with calibration validation; Vincendon et al. (2024) deals with ground calibration; Stefani et al.(2025) deals with inflight calibration, while Poulet et al. (2024a) introduces a summary of performances. They are therefore deemed equally relevant for readers who want deeper these aspects.

l. 167: Can the authors explain what f represents?

f is defined right here. Rephrased for clarity.

l. 181 and 186: Can the authors define NESR?

There is already the definition as Noise Equivalent Spectral Radiance at line 181.

Fig. 2: Can the authors label the four panels as (a–d) and refer to them accordingly in the text?

Done.

Fig. 2: Can the authors double-check the units in the lower right panel? Is 0.08% indeed the maximum value?

Thanks for noticing that, y axis now fixed.

Fig. 2 (caption): Is the lower-left spectrum shown after correction? If so, could this be specified in the caption?

Thanks for noticing this omission: the spectrum refers to the data before correction, actually it is the background average removed from the cube. The green spectrum is the associated standard deviation, which is not affected by additive operations as background subtraction. Caption updated.

Table 1: Can the authors explain what the last column (flash length) represents and how it is derived?

It is simply the projected length of the portion of the MAJIS slit where lightning emissions are seen, which we assume in the following to represent the diameters of the entire flashes. Table's caption updated.

l. 249: Can the authors confirm whether the wavelength is 4610 nm and not 3979 nm, as indicated in Fig. 3? More generally, the use of different wavelengths is somewhat confusing. What difference does it make to use 3979 or 4610 nm to derive brightness temperature? A brief clarification would be helpful.

Thanks for the note, the mention of 4610 nm is a leftover of previous versions. There is little difference (negligible for our purpose) coming from using these two wavelengths since both are poorly affected by gas absorption, but the 3979 nm has been effectively used throughout the manuscript. Text updated here and in Fig.4's caption.

l. 274: Can the authors remind the reader of the value of f? Is it still 10% as stated in l. 167?

Yes, f is constant, its value is not made explicit again here, being implicit in the formula 1/(1+2f)=0.83

l. 286: Can the authors ensure consistent notation for the peak at 744.5 nm? Both 744 and 745 nm are used.

The only mention of 744 was in the label in Fig.5c, now changed. We adopt a 745 nm label for that line throughout the text.

l. 290: Can the authors comment on the uncertainty in the line assignments? The use of "possible" introduces ambiguity.

The line assignment implicitly relies on assumptions derived from our knowledge of atmospheric composition and previous lightning spectroscopy observations. We can demonstrate that MAJIS measurements are compatible with the emission lines listed in Table 2, owing to the most common air molecules, but, given the non-optimal resolutions which cannot fully constrain the models, and we cannot exclude in principle the presence of other minor emission lines somehow altering the spectral shapes. We can only state that no other species/lines are needed to explain these observations. This statement has been added in the manuscript.

l. 305: Can the authors remind the reader of the spectral resolution at this point?

Done. We also replace the generic "degradation" with a convolution over MAJIS instrumental response's line shape.

l. 306–308: Can the authors elaborate on why this Balmer line is considered "interesting"? Is it related to lightning detection?

Yes, H-alpha emission is well known in association with lightning hence its detection in MAJIS spectra could be significant for identification and discharge characterization. E.g. H-alpha is typically formed by water dissociation, i.e. is increased in wetter conditions, and the relative emission intensities could be in principle traced back to the dynamic mechanism of lightning discharge. Unfortunately our detection can only be tentative given the low SNR. The detection of other lines of the Balmer series (e.g. H-beta at 486 nm), even at low SNR, could have conclusively demonstrated the presence of atomic H, but none of them are covered by the MAJIS spectral range.

The sentence has been rephrased making these considerations more explicit.

This point is also now better explained in Section 4.4, where the recent publication by Yingying(2025) is now cited.

Tables 3 and 4 vs Table 2: These tables partly contain the same lines but not completely. Could the authors clarify in the captions and/or text how they differ and which one supersedes the others?

Tab.2 lists all the emission lines found in MAJIS spectra above noise. We propose here the species possibly contributing to them, but the list is independent from any modeling. Tab.3 and 4 only list the lines above noise assigned to Oxygen and Nitrogen respectively, identifying the atomic transitions that maximize the spectral overlap with the response of the given MAJIS spectral band (i.e. largest filling factor, listed in the last column). Captions updated accordingly.

l. 447 and 452: The lines at 630.03 and 631 nm are both reported as undetected. Are these the same line? Please harmonize the notation.

Yes, fixed to 631 nm.

l. 527: Could the authors clarify the meaning of the first sentence?

We are describing the time scales known to be involved in the rise and decay of lightning pulses, in order to give a framework against which to compare the MAJIS timescale. We rephrased the sentence a bit.

l. 641: Could the authors remind the reader which figure shows flash D?

Added citation of Fig.5 showing peak spectral radiances.

Table 5: Could the authors explain how the last row is obtained? The values do not appear to correspond to a simple “B+C+D” sum.

Being B,C,D consecutive detections, in this last row of the table we give an estimation of the total energy emitted in the case the spectra B, C, D represent three samples of a unique flash sequence. This is obtained by assuming that the average energy density observed in B,C,D (1.72 kJ/km2 for O I) is actually emitted in all three cases over the largest area (the D’s one, 257 km^2), yielding for OI: 1.72 x 257 x 3 ~ 1330 kJ.

It is worth noting that, since MAJIS acquires for 22 ms every 200 ms, we have 178 ms blind gaps between consecutive frames. Then, the B+C+D detections may suggest a sequence longer than 422 ms (22x3+178x2) but shorter than 600 ms (200x3). Anyway, we actually cannot know which portions of the sequence are sampled by B,C,D acquisitions and which ones fall in the gaps, hence we only count the energy directly measured, without making hypotheses on emissions during gaps.

We improved this description in Tab.5’s caption and in the text.

Eq. 15: Could the authors provide a reference (textbook or paper) detailing each term?

All the terms in eq.15 are already used and defined in previous sections (see eq.1 and extensive discussions of filling factors in sect.3). The eq.15 is directly derived from eq.1 and eq.13. Context references are already cited just below the equation (Li et al., 2016; Kieu et al., 2021; Boggs et al., 2021).

A further mention of eq.13 has been added to make its derivation clearer.

l. 712: Could the authors provide a reference for multiplet parameter uncertainties and maybe an estimation too?

The literature on the subject is of course huge, but references for all multiplet parameters used in this work can be found within the NIST database. We can provide as a generic reference Kramida,2024 (doi: 10.1140/epjd/s10053-024-00820-y) encompassing a comprehensive review of uncertainties. About the specific lines used in temperature retrieval, we only searched for order of magnitude errors (we deemed them sufficient to our purposes, given the measurement's characteristics), finding values of about 0.2 - 1 % for the transition probabilities (A_{ji} coefficients) (see e.g. Hibbert+1991, doi:10.1088/0953-4075/24/18/010), while errors on other parameters (central wavelengths and state energies) are usually much smaller (~10^{-6} %, see e.g. Civis+2018, 10.3847/1538-4365/aae5f8).

We added the citation of Kramida,2024 to the text.

Fig. 9 (x-axis): The label “ratio I_b + I_q” is unclear. Is this a sum or a ratio? Please rephrase.

It is a ratio. Updated.

l. 774: Please add a reference for the values of χ and D.

Added citations of (Cox, 2002), (Frost&McDowell,1956), (Wang+2026), (Darwent,1970).

l. 811: “different distance from the noise level” is unclear; please rephrase.

Removed. This “line strength temporal bias” is already described above so here we only keep the reference to sect.3.5.

l. 850: Please define LNO_x molecules.

They are defined here. We can add “so-called” LNOx for clarity, a reference for them is already given.

Technical corrections

l. 129: Define FWHM upon first use.

Done.

l. 261: Check the font for μ in μm (units should not be italicized).

Change to nm.

Fig. 4 (inset): Improve arrow color contrast.

Done.

l. 326 (Table 2 caption): Use “positions” instead of “locations”.

Changed.

Table 2: Revise column titles for clarity. For instance change the title of the columns for “Observed positions (nm) & Most likely assignments & Additional contributions\\\”.

Done.

Tables 3 and 4: Can the authors revise the titles of the column as “Observed line positions & Electronic transitions & Spectral filling factor\\\” and “(nm) & (nm) & Line assignment & $\delta_{k,b}$\\\”, for instance?

Done.

Tables 3 and 4: Check font in the last column (appears struck through).

Corrected.

l. 416: Use “MAJIS spectral band”.

Done.

l. 445: “a decrease in temperature”
Corrected.

l. 480: Replace “are seen peaking” with “are centered”.
Corrected.

l. 487: Check spacing (“related to other”).
Corrected.

l. 491: Check spacing (“in Figure 8b”).
Done.

Fig. 8: Increase thickness of dashed orange curves.
Corrected. Citation of “orange curves” was a leftover of a previous version, now removed.

l. 624–625: Check parentheses around references.
Corrected.

Table 5: Improve layout (duplicate “$\sum f$”, units formatting).
Corrected.

Fig. 9: Increase label font size.
Done.

l. 755: Add “-s” to “ratio”.
Corrected.

l. 806: I smiled when I read the sentence: “It is evident that the two methods investigated do not fully agree with each other (…)”. Can the authors remove the word “fully”?
Done. “Ironic” emphasis removed :)

l. 862: Check parentheses.
Corrected.

l. 923: Use superscript in cm^{-1}.
Corrected.

l. 944: Check parentheses and confirm Fletcher et al. (2025) status.
Done.

l. 974: Prefer “neither … nor”.
Done.

l. 1070, 1098, 1193: Check references.
Done.

l. 1240, 1259: Add DOIs if available.
Done.