“MAJIS performances in the infrared during the JUICE 2024 Earth fly-by: comparisons with IASI measurements and sensitivity to trace species”

Response to comments from both referees

General comments:

We thank both referee for their thorough review of our manuscript.

We reply to all their comments below (answers in red) and provide a revised version of the manuscript.

We have significantly changed section 5 (IASI-MAJIS comparison) with more forward-modelling studies, and added a short section 6 to highlight a bias between left-edge and right-edge pixels of the MAJIS detector array. For simplicity, in the “track changes” version, we overridden the latexdiff automatic process and have considered that all section 5 was “new”, given the significant changes.

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Referee #1
Comments in detail:

I would suggest a slight rephrasing of the title: "MAJIS infrared performance during the JUICE 2024 Earth flyby: comparison with IASI measurements and sensitivity to atmospheric trace species".

To address also another comment by referee#2 (regarding the choice of word “sensitivity”), we changed the title to “MAJIS infrared performances during the JUICE 2024 Earth fly-by: comparisons with IASI measurements and detection of atmospheric trace species”

page 4: "We did not perform any retrievals in this paper" - a few lines below the authors state "For CO, CH4 and N2O, we .... scaled these profiles in order to obtain a best fit". These are somewhat contradictory statements.

We did not do any retrievals in the sense that no inverse retrieval algorithm was used to minimize a cost function ; only direct calculations were done for different multiplicative factors to trace species profiles and we stopped the analysis when the residuals were at the order of the noise level. Our goal was to get a set of atmospheric profiles that match the data well in order to have a good baseline for our future sensitivity analysis, without seeking optimization (that is outside the scope of this paper). We agree that this could be confusing, especially where we employed the term “best fit”. For clarity, we removed the term “best fit” and that part of the sentence that described what we did not do (remove “We did not perform any retrievals in this paper; rather”) and changed a bit the text to:

“Other trace species were set to typical vertical profiles found in the tropics; however, some IASI-4A/OP mismatches were found in the spectral ranges corresponding to the signatures of CO, CH4 and N2O. We thus also scaled these trace species profiles until we obtained a good match of these spectral features (with residuals of the order of the noise level), resulting in the spectrum shown in Fig. 1.”

page 5, cloud filtering: The MAJIS cloud filtering via a 4.64 um brightness T threshold appears not to be the optimal choice. Would the addition of a VIS/NIR brightness value further improve the recognition of clouds? (See Fig 3, where in the right panel more filigrane cloud structure tend to be lost.)

We argue on our side that the cloud filter works well enough: even if there seems to be a few filament clouds not captured by the BT flag, the correspondence seen in Fig. 3 is satisfactory enough to be comparable to the “less than 5% cloudy” at the scale of the wide IASI field of view. In addition, in Fig. 5, we see that all IASI data that were independently flagged as “cloudless” from a high resolution onboard camera (<5% cloud contamination) also have a BT at 4.64µm > 290K. Of course it does not mean that all data that have a BT > 290K are cloud free, but that is already half of a validation.

The reason for not choosing a MAJIS VIS radiance threshold value for the cloud filter is that the IASI/MAJIS comparison exploits the MAJIS NIR channel. We found it simpler to base our cloud filter on the NIR channel itself, while checking the validity of our filter with this visual inspection of clouds in the VIS channel, as reported in the manuscript.

page 13, line 193ff: if the discrepancy is induced by differences in H2O as claimed by the authors, I would expect that the quite pronounced spectral patterns shown in Fig 10 at wavelengths beyond 4.6 um correspond to H2O spectral signatures. Adding some further simulations using 4A/OP should allow to verify this claim more explicitely.

We did make these calculations for different water vapor profiles as retrieved by IASI (all other variables such as surface and atmospheric temperatures held constant), as stated lines 199-201 of the submitted version. To limit the number of figures we had not shown it. From these spectra, we found a positive correlation between the position of water lines and IASI-MAJIS differences. Our conclusion at submission time was that the IASI-MAJIS mismatch could be explained by a combination of a ~5% radiometric bias in the MAJIS calibration (as far as co-locations from swath “C15” are concerned) and of some changes in water vapor between the scenes.

However, since submission, we have done more sensitivity studies with 4A/OP to changes in surface temperature, emissivity and atmospheric temperature, and our conclusion slightly changed. Indeed, a +1K in atmospheric temperature results in a similar signal as a 10% decrease in atmospheric water vapor, so that we cannot disentangle the two contributions. Such changes are plausible over a 2-hour local time gap between MAJIS and IASI, according to ERA-5 reanalysis, and produce changes in radiance within +/- 5%, commensurate with the overall IASI—MAJIS statistics. This highlights the need for closely co-located data for such inter-instrument calibration exercise. Base on swath C15 (with only a 20 min local time difference with IASI), we have led a

more thorough analysis and now characterize the radiometric MAJIS bias to be of the order of -8% (instead of -5%) for these left-edge pixels.

Small differences in water vapor and/or atmospheric temperature remain part of the story in explaining the IASI-MAJIS mismatch, especially for the MAJIS swath taken ~2h away from IASI. They are better documented in the revised version.

page 15, line 239: "potential variations in meteorological conditions". This seems a persisting problem for the whole comparison, especially when it comes to the H2O profile. I wonder whether the ERA5 hourly data combined with 4A/OP runs for estimating the resulting effects on radiances would help to further substantiate the conclusions and perhaps narrow down the residual uncertainty of the MAJIS radiance calibration.

Following the referee's comment, we extracted ERA5 hourly data (at 7AM, 8AM, 9AM, 10AM) to characterize the expected atmospheric variability. In terms of temperature, over 3 hours, very small changes are expected in the troposphere (0.5K) and grow to 3K in the stratosphere. Water vapor shows a temporal variability of the order of 10–15%.

Conclusions: Here, a coincidence interval of 2 hours is mentioned, while the reader would expect (based on section 4.3 and table 1) 2.5 hours?

Indeed our criterion was 2.5 hours (in order to get co-located data for C10 and C9); we have corrected this text.

Further down, the conclusion discusses in length (lines 286 to end) items which have not at all been treated in the paper. Either this part should be removed or an additional section needs to be added which makes the claims of the conclusions explicit.

Agreed; we actually removed a large part of this text and moved a few sentences to the end of the discussion before the conclusion.

Data availability: I find an embargo period of about 5 years after data collection of the flyby data (this would imply data accessibility three years after publication of this study) inacceptable, roughly as distant from any contemporary FAIR principles as Jupiter is from the sun. It would appear to me that the minimum requirement from the viewpoint of a study publication would be a release of at least the subset of MAJIS spectra actually used in the form of an electronic supplement.

That was not our choice but the policy (from ESA) at the time of submission, but since a workaround solution has been agreed between the JUICE project and the PI instruments to make available the MAJIS calibrated data used for the LEGA papers. We will thus add the following data availability statement:

MAJIS calibrated data acquired during the JUICE Moon–Earth flyby in August 2024 are available through the following Guest Storage Facility: European Space Agency, 2026, ESA-JUICE_LEGA-Special-Issue_v1.0, https://doi.org/10.57780/esa-2bb387d.

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Referee #2

General comments: This is an interesting cross-comparison for the first in-flight characterisation of the MAJIS IR channel against a reference sounder (IASI), applied through an established methodology (4A/OP). The data is well studied and the study merits publication after revision. My main concern is that the authors tends to state the central calibration results more firmly than the actual data support: The comparison uses limited data subset rather than statistics over the full data set, The differences are attributed mainly to water vapor when other effects such as those from calibration bias, pixel-position effects and a high-angle emissivity assumption are omitted. I recommend publication after revision, contingent on the four major comments below.

We thank the referee for his review that motivated us to refine our analysis and sensitivity studies.

However we tend to disagree with that sentence: " ...when other effects such as those from calibration bias, pixel-position effects and a high-angle emissivity assumption are omitted"

as calibration bias are at the center of our work (this is precisely what we want to bracket); as pixel-position effects were also mentioned (that is why we did not do a global statistical analysis but separated case studies for the middle, left or right edge of the MAJIS array); and high-angle emissivity are not a concern here (the largest value, among the co-located data, is 50°).

Statistics over the full dataset were initially done but given the range of different observing conditions (such as viewing angles or local times gaps between IASI and MAJIS) and different parts of the MAJIS array for different swaths, we preferred to take a closer look at co-locations from different MAJIS swaths. Not showing a global statistical analysis was thus motivated for scientific reasons. In the revised version, we have re-arranged our results to first show the global statistics, then focused on separate swaths (with detailed figures for C11 and C15) as they reveal different biases.

Major comments

1. The calibration conclusions come from on one example (C15), + some limited C10/C11 yet there are 36 co-locations, each with tens–hundreds of MAJIS spectra. Why do authors do not want to show the mean and scatter of full data set; (MAJIS−IASI)/IASI as a function of wavelength across all 36 co-locations, with error bars? Without this, "better than 10%" remains unsupported. When the authors show the difference, they can add a %-difference plot or curve secondary axis to Fig. 10b.

It is not that we did not want to show it; we did this plot early in the analysis and then turned to more focused comparisons, swath by swath. The reason behind it was to try to disentangle potential biases that would not be global, but dependent on the position of the pixel on the array; or depending on the varying "quality" of the co-location criteria.

We now include this plot over the 39 (slight update) co-located data, with the following text:

“[…], the median of the relative difference between IASI and MAJIS is found to be less than 5\% over most of the spectral range (reaching 10% locally), centred near zero, with a 1-σ standard deviation ranging between 5 and 10% depending on wavelength (see Fig. 11). The exception is a ``spike" seen near 4.2 µm corresponding to a minimum radiance in the spectra; the high relative difference found here is thus explained by the division by very small values. Note also that without the spectral shift correction, the IASI-MAJIS residuals are locally up to 20 times larger beyond 4 µm (not shown). “

We then discuss separately the IASI-MAJIS residuals for different swath and for that we now show both the relative (in %) and absolute differences, as the referee suggests. We hope this will convince the referee that based on comparison with IASI, we find that MAJIS radiometric calibration is indeed within 10%, over most of the considered spectral range.

2. The “best case” is C15 (L176), itself flagged as glint-affected and corrected; C15 is then reused in Section 5 for the SNR and trace-species analysis despite being among the poorer matches. Using a contaminated swath as the radiometric reference, and again as the trace-species testbed, should be justified or replaced with cleaner swaths.

While that swath is largely glint-contaminated, the upper part of the array is not. We used a very stringent filter for glint contamination on purpose and we are confident (based also on an analysis on the VIS channel where glint is also obvious) that the remaining MAJIS spectra that pass our filter are not contaminated. There is no correction applied (we do not know to which correction the referee is referring to). In the submitted version, we chose these spectra as our benchmark test case because they are the closest in space and time to IASI spectra (acquired only 20 minutes earlier than MAJIS, versus ~1h30 for the C10 and C11 cases and 2h for C09); hence we found it the best case to perform a sensitivity analysis on trace species as we could start from a reference atmospheric state (temperature and composition) taken from IASI retrievals. The SNR and trace species analysis from other swaths yield extremely similar results anyway; hence we argue to keep our test case in the manuscript.

3. The fact that C10/C11 agree well without correction while C15 needs +5% does not confirm the influence of the water-vapour story.The water-vapour attribution, and the +5% bias, should be presented as cpossible hypotheses , not as established. The abstract and conclusions currently overstate this.

We agree; this is why we presented two (combined) hypothesis: a radiometric bias that depends on the detector’s pixel position, and an effect of different meteorological conditions between IASI and MAJIS times of acquisitions (as there was a ~2h gap in local time for swath C10 and C11, but only 20 min gap for swath C15). We precisely presented these in those terms (line 223-229 of the submitted manuscript) “One hypothesis is that…The other hypothesis is that…”, while our conclusions was also cautious (“we suggest that...”) so that we find the referee’s comment somewhat a bit unfair.

We have anyway re-written the whole section in the revised manuscript, based on more extended studies of the IASI-MAJIS comparison, including sensitivity studies with 41/OP (see revised

manuscript). We feel that we now have more elements pointing towards a pixel-dependent radiometric bias (left versus right edge of the detector) and that small differences in water vapor and/or atmospheric temperature are also a part of the story in explaining the IASI-MAJIS mismatch, especially for the MAJIS swath taken ~2h away from IASI.

4. The flyby geometry is ~50° emission angle, yet 4A/OP uses a fixed emissivity of 0.98 across the whole range. At ~50° the sea-surface MWIR emissivity departs measurably from 0.98 and becomes angle- and wavelength-dependent, and the reflected term grows, wavelength-varying effect of the same magnitude attributed to water vapour. An angle-dependent emissivity should be tested before the residual is assigned to water vapour.

The referee is right that emissivity will vary with emission angle and wavelength (and windspeeds), however we argue that these changes are very small.

Firstly, according to Masuda et al., 1988 (Remote Sensing of Environment), the emissivity of sea water is expected to lie in the range 0.963 – 0.977 at a wavelength of 4 µm for emission angles between 0 and 50°, with amplitudes of wavelength-dependencies of 0.015 at maximum between 3.5 and 4.1 µm. For instance, at a 50° emission angle the emissivity of sea water varies between 0.9559 at 3.5 µm to 0.9637 at 4.1 µm. [We checked our program and actually used a constant value of 0.97 in the results presented in the manuscript, not 0.98 that was used for preliminary tests; this error has been corrected in the revised manuscript].

Secondly, while we note from Masuda et al. that emissivity values depart significantly from 0.97 at high emission angles (~0.94 at 60°, 0.86 at 70°, and so on), the highest emissivity value in the whole IASI data set is 53°, and 50° in the MAJIS case, so that we should not be concerned by any significant variations.

Thirdly, it is important to note that while changing the emissivity will have a small impact on the measured spectra in the range 3.5 - 4.1 µm, it does not significantly impact the outgoing thermal emission in the range 4.1 to 5.5 µm, dominated by atmospheric (gaseous) absorption and not surface emission. Hence, in the spectroscopic range of water vapor, changing the emissivity has no noticeable effect on the spectra, and we disagree with the referee remark that the emissivity could have a " wavelength-varying effect of the same magnitude attributed to water vapour" and that "An angle-dependent emissivity should be tested before the residual is assigned to water vapour.".

That being said, we do now include a sensitivity test on the influence of emissivity in our revised analysis (see Figure 12) and a sentence about expected variations in the ocean's emissivity, with references to Masuda 1988 and Masuda 2006. We find that small emissivity variations in the range +/- 0.01 or 0.02 could help fill the gap between IASI and MAJIS at wavelength shorter than ~4µm for some swaths (not all), and confirm that it has no significant effect in the range 4 -5.6 µm. However, IASI and MAJIS being taken at very similar emission angles, we argue that a change in emissivity of ~0.01 is not realistic, even when taking into account wind variations.

Minor comments

Abstract

- Abstract can be slighly misleading since it may imply IASI and MAJIS share similar local times, locations and footprints, whereas all differ and effectively only three IASI spectra are truly comparable. Please state this clearly.

We have rephrased with “nearly co-located”. The introduction also makes these points clearer.

- The “Excellent” is contradictory with “better than 10%” Considering it is an instrument designed for other objectives yes the comparison is indeed impressive but either name the benchmark or soften the sentence.

  We have removed the term “excellent” that was indeed subjective.

  In Abstract “~15%” beyond 5.25 µm is menstioned here, however in the text and conclusions “~15–20%” — is stated.

These parts have been edited according to the updated analysis.

- For the trace species the authors use uniform capability. The detection significances show each gas is detectable above the noise. The retrieval precisions (Table 2) are a different, theoretical quantity: the minimum column uncertainty achievable if noise were the only error. Usefulness depends on comparing that precision against each gas's natural variability: CH_4 is unusable, CO marginal. The abstract and Section 5 should therefore not present a uniform "sensitivity to trace species.

We agree and we did include a comparison with each gas natural variability already in the submitted manuscript (see Table 2) and already emphasized in Section 5 that CH4 is useless (line 264: “Hence, no useful information on the CH4 spatial variations can be derived from MAJIS spectra”). We had also already written in the submitted manuscript that these were minimum uncertainty values, if noise was the sole source of error (line 270: “In addition, these estimates are “lower bound” values as the actual uncertainty would account for the propagation of several sources of errors (eg. on the temperature profile, ground emissivity,...) and for correlation of errors between trace species.”). Hence we already fully align with the referee. We slightly changed the abstract (and conclusion) to make sure there is no overstatement but feel the referee’s rebuttal was a bit unfair.

- Minor: “carbone monoxide” → “carbon monoxide.”

Changed.

Introduction

- State briefly here the spectral resolutions of both MAJIS and IASI (currently mentioned only later), and note the observations are close but not exactly co-located in local time.

Goo idea; we have added a mention of the differences in spectral (and spatial) resolutions of the two instruments in the introduction. We have also edited this sentence: “at similar -- **although not identical** -- locations, local times and viewing angles.”

- For description o Majis observations the authors move too fast and rely heavily on forward references to companion papers (Poulet 2025, Langevin 2026, Oliva 2026); some data and figures must stand on its own.

We have removed from the introduction that sentence referring the reader too quickly to companion papers for a detailed description of MAJIS and changed section 3 to “Characteristics of IASI and

MAJIS observations". More details are now given on MAJIS instrument and EGA dataset in that section (while still keeping references to other papers when it is suitable).

Section 3 — IASI characteristics

- Drifts into describing MAJIS co-located measurements; comaprison of the footprint, local-hour mismatch and comparability should be addressed may be in other sections.?.

We solved this issue by changing the section to "Characteristics of IASI and MAJIS observations", so that we now describe the two instruments in parallel, including a comparison of their footprint sizes etc.

Section 4 — Timing & co-location

- Provide a table of the time and local time of each observation actually used (currently deferred to other papers).

Rather than repeating here a general table for all MAJIS observations (which can be found in other papers), we have added this table below. It focuses on characteristics of co-located MAJIS and IASI datasets, showing the ranges in local times and emission angles of both datasets, and latitude/longitude ranges to help locate them on the map.

Swath number	Number of co-located IASI obs.	Longitude range (MAJIS and IASI together)	Latitude range (MAJIS and IASI together)	Local time range (MAJIS)	Local time range (IASI)	Emission angle range (MAJIS)	Emission angle range (IASI)
C08 right edge	1	140.5-140.55	20.6 – 20.8	6h57	9h23	19.3	37.3
C09 left edge	8	140.7 – 141.9	17.7 – 18.8	6h59 - 7h03	9h24 – 9h29	17.0° – 18.8°	25.5° – 33.3°
C10 middle	13	146.1 – 147.8	22.9 – 25.2	7h20 - 7h28	9h43 - 9h50	24.1° - 25.4°	4.9° – 10°
C11 right edge	14	153.2 – 154.6	25.6 – 27.4	7h50 - 7h58	9h18 - 9h23	32.4° - 33.7°	24.0° – 31.7°
C15 left edge	3	174.4 – 175.0	28.8 – 29.2	9h20 - 9h22	9h01 - 9h03	49.7° - 50.5°	52.2° – 53.8°

- The 06–10 h MAJIS scenes; are they within 3 h of IASI, or 2.5 h as in Table 1? Reconcile, and re-examine the loosened criterion.

It is 2.5h as reported in the Table; we did not find a reference to "3 h".

- ±2.5 h at these local times is a large geophysical gap; quantify its expected radiance impact (linked to Major comments).

Over the tropical ocean, where the surface and atmospheric temperature varies little at diurnal scale (of the order of 0.5K only in the troposhere according to ERA-5 data), this is not "that" large a gap.

That being said, for specific wavelengths, especially in the strong 4.3 µm CO2 band (probing the upper troposphere), we clearly start to see some departures between IASI and MAJIS observations that can be attributed to different atmospheric temperatures for the swath with a 1h30 gap, which increases for swaths with gaps of 2h or more.

Ideally we would work with a smaller gap but unfortunately, restricting this criterion to only one hour leaves only the 3 IASI co-located observations in C15 (as said in the submitted version around line 165). Our strongest conclusion in the submitted paper was based on swath C15, precisely for this reason (small local time gap).
As this number of co-located data was very low, we chose to loosened this criterion to get a larger number of co-located data and have more material for discussion and statistics.

In the revised manuscript, we quantify further the impact of potential changes in meteorological conditions, supported by calculations of synthetic spectra with 4A/OP (see revised text and Fig 12).

Another Earth fly-by is planned for September, 2026; hopefully this will bring complementary information (we have added this point in the conclusion).

Section 4.1 — Cloud filtering

- Excluding clouds is the good choice (cloud variability would affect the signal); the paper need only state this rationale in one sentence early also (also Table 1).

Good idea; we now mention the challenges of inter-comparison of two satellites datasets in the introduction, including the effects of cloud and glint contamination.

- For clouds VIS+IR is used on the MAJIS side (0.7 + 4.64 µm); IASI has limited range. Does it have a visible channel ? Figure 5 mentions an onboard camera. how is it used? Authors can discuss further?

Indeed, as IASI spectral range starts near 3.6µm, complementary information on clouds are obtained from a camera called the Advanced Very High Resolution Radiometer (AVHRR), mounted onboard METOP and NOAA satellites. It operates at 5 different channels simultaneously in the visible and infrared bands, at central wavelengths 0.63µm, 0.865µm, 1.61µm (only daytime), 3.7µm (only nighttime), 10.8µm and 12µm. It has an instantaneous square field of view of about 1.1 km near nadir and is used operationally by EUMETSAT to provide cloud coverage (among other data products). Cloud contamination within IASI, in % of field of view, comes as an operational IASI L1C data product.
We have added some details about the camera, along with a reference to a recent paper estimating different cloud coverage methods Whitburn, S. et al: A CO2 -independent cloud mask from infrared Atmospheric Sounding Interferometer (IASI) radiances for climate applications, AMT 15, 6653–6668, https://doi.org/10.5194/amt-15-6653-2022, 2022.

- The <5% IASI cloud fraction is not a radiometric floor; The authors can quantify its actual radiometric impact and show it is negligible against the 10% claim.

The precise radiometric impact would depend not only on cloud fraction but also on the cloud type (low and thick cloud versus high and thin cirrus, for instance), but a 5% threshold was found by previous studies to be a good compromise between accuracy and number of co-locations, and is used as a standard in CAL/CAL IASI activities. Note that while a 5% threshold was used, most IASI data that we retain had a “0%” cloud contamination (according to this flag).

To convince the referee, we selected a large set of IASI data, all acquired at similar viewing angles over the ocean near 9:30 AM, either flagged as “cloud-free” or 5% cloudy. The bias between the

two ensemble of datasets is small : the “5% cloudy” data have a small positive +1.1% bias below 4µm and a small negative -0.4% bias between 4 and 5.5µm, compared to the cloud-free cases, with a standard deviation of 1.3%. The order-of-magnitude error due to possible different cloud contaminations between IASI and MAJIS is thus of typically ~1 %.

Section 4.2 — Glint

- Justify the 3.9 µm glint proxy and the >301 K threshold; this explanation arrives very late (L155) and should move forward, next to Fig. 6 (which is vey helpful).

We have re-arranged the text to improve the clarity of this section and now justify the use of the 3.9 µm glint proxy right after mention of Fig. 6.

- Glint is itself an additional uncertainty Authors shall make it explicit.

We agree, but this is the very reason why we have excluded glint-contaminated pixels from our analysis and . However, we agree that a filter based on a unique wavelength for all swaths (with different viewing geometries) is not perfect. In some cases this threshold may be too strict and can filter out data not contaminated by glint; in other cases it might be still slightly too high and retain a low amount of glint signal. After further investigation, we find that a residual glint contamination of the order of 2-3 % of excess radiance at 4µm is still possible with our filter, depending on observing geometries and wind orientation, and have added a word of caution in the revised version.

- 11 retained observations are all daylight (L141)....are not they?

Yes they are all daylight.

- Fig. 7 / L146: can clouds be separated so the glint pattern is clearer?

Unfortunately not, as glint is strong also above clouds (high reflectivity), like for instance in swath C13. Removing cloud-contaminated pixels would remove most of the data from this swath, and the glint pattern would then be even less clear. We tried with figures at other wavelengths but due to saturation artefacts for swath C12 -C13 -C14 at most wavelengths in the visible, we kept the choice of lambda = 2.35µm for data visualisation of glint effect.

- Explain the dark slit/line along the edge of some swaths (C11, C14, C15).

The MAJIS nominal array is 400 pixels-wide but during the Earth fly-by, data was acquired in windowing mode, where only 64 (or 128) pixels-wide scans were recorded. For calibration and test purposes, data was acquired either for central, left or right edge pixels of the MAJIS detector array. We did explain this in the revised version, but quite late, and now describe it in section 2. Furthermore, for left and right edge acquisition, observations were slightly off-centered on purpose to see the effect of straylight on the detector. The dark lines on the edge of some swath correspond to 15 out-of-field of view pixels that did not receive any direct signal from the target. We have added this missing piece of information, also in section 2. Note that straylight was found significant only in the visible range and is documented in Langevin et al. companion paper.

Section 4 — Figure 2

- It can be confusing for reader to see data is over exposed and saturated, nevertheless still have lower radiance values, and he cloud signal in addition the glint. Plesea explain how a a swath can be glint-bright and saturation-dark at once , mechanism and how saturation is distinguished from clouds?

Saturation is defined when raw Digital Numbers (uncalibrated radiances) are over a threshold of 11 000 in the infrared channel. It occurred for swath C12, C13 and C14 that had a longer acquisition

time. Saturation occurs in the interferometric space ; and although it is not intuitive this can result in both negative and positive artefacts in terms of radiance. Furthermore, non-linearity effects could also produce too low and spectrally flat radiance, as seems to be the case here.

Section 4.4 Comparing IASI and MAJIS spectra

- Explain why the IASI footprint is an ellipse rather than a circle (in figure we see a circle, off-nadir projection foreshortening...

The IASI footprint is an ellipse, as the circular FOV will be elongated in the cross-track direction (it is a circle only for pure nadir). In the figure you do see that our co-locations encompass an ellipse (eg fig. 9, blue points) and for simplicity in the submitted version we did not represent graphically the full IASI footprint ellipse but just circled the central (longitude, latitude) of a given IASI data. We have revised our co-location figure to now graphically display an ellipse for consistency, and explain it in the text.

- The 2–5% emission-angle correction (ratio of two 4A/OP spectra, L180) is another error source can be considered it in the error budget.

We see it from a different perspective: as we account for differences in emission angles in our analysis (using synthetic spectra from 4A/OP), we do not consider it as a source of error, as this effect is very well characterized.

- Clarify whether the “raw comparison” (L183) is before or after these corrections; the wording is confusing.

  Indeed this was confusing: the IASI-MAJIS comparisons discussed and shown in the submitted manuscript were done with a correction factor applied to IASI spectra based on 4A/OP calculations at different emission angles, so that this was not a “raw comparison”. Our updated analysis makes this point clearer: we now either show the raw comparison (and the effect of emission angle change as estimated from 4A/OP overplotted), or comparisons with synthetic spectra computed with the actual emission angles of each instrument.

Section 4 — Spectral shift & residuals

- L172: the 4 nm shift beyond 4 µm is asserted but not shown — provide a demonstrating figure, or state clearly it is documented in Langevin (2026) and confirm the spectra used here already include the correction.

It is demonstrated and documented at length in Langevin paper through cross-correlation studies. We already stated that in our paper we only present results with MAJIS spectra already corrected for this shift (see line 172-174 in submitted version). However it is true that we mentioned the shift in the abstract, so that we understand the referee’s confusion. We have thus added a figure highlighting the shift (see next page and new Fig. 10), but refer to Langevin et al 2026 for details about the correction.

- L183: where does the 0.15 W/m²/sr/µm difference apply (below 4.57 µm?), and what are the peaks above 4.5 µm — water vapour? Explain.

This difference applies beyond 4.55µm. Water vapour lines dominate the spectra beyond ~4.9µm. This text in no longer in the revised version.

- L210: why 5.25 µm and not 5 or 4.75 µm? Why not anchor on specific water-vapour lines? The “<10% above 5–5.25 µm” claim is a bit arbitrary oplease revise these numbers carefully (and see Major 1 for the % plot).

We now show plots both in absolute and relative units in the revised version. We droped the reference to “5.25µm” in the revised version.

- Can the water-vapour effect be modelled out with 4A/OP rather than invoked qualitatively?

Yes this is what we did already in the submitted version (see lines 199-201 and reply to referee #1) and is shown more clearly in the new figure 12 (see revised version). Given the results of the new analysis, we do not retain the water vapor variability as a main source of discrepancy anymore, as small atmospheric temperature changes can produce rather similar changes. We now conclude that the main source of discrepancy is a small calibration bias for swath C15, but that small differences in atmospheric water vapor OR temperature are also responsible for the IASI-MAJIS mismatch taken ~1h30-2h apart.

Section 5 — Synthetic spectra & trace species

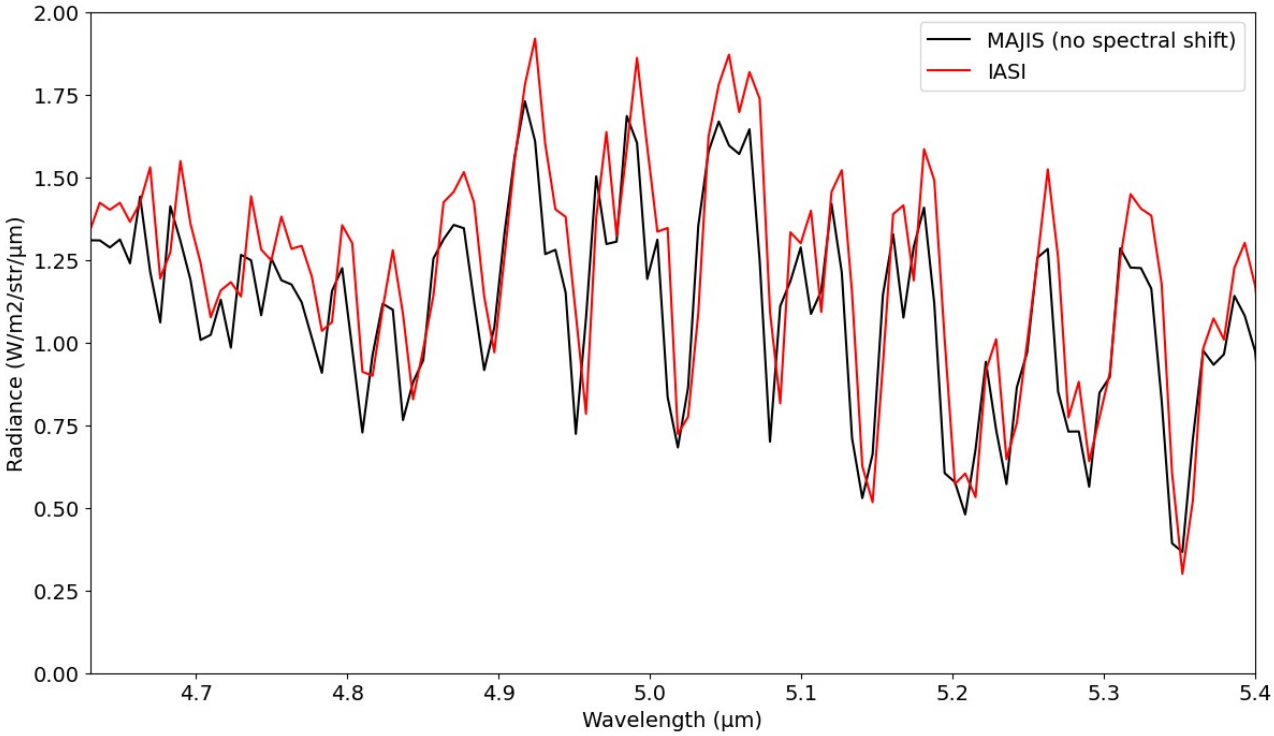
- The SNR (200–300) is derived from the variance of (MAJIS−4A/OP), which contains model and measurements error and it is not an instrument-noise metric. The authors can extend the discussion or estimate noise from a cleaner source (e.g. repeated-pixel variance).

SNR values were estimated from an instrumental point of view from the read-out noise in Poulet et al. 2026 (same issue); as already quoted in the manuscript. The estimate we gave in our paper did not pretend to be an instrument-noise metric value, but has the merit of being an independent evaluation and shows good consistency with Poulet et al. study.

Conclusions

- The 4 nm shift is not demonstrated in the paper.

Indeed but we never pretended to demonstrate it in our paper; it is done in a companion paper focused on spectral calibration (Langevin 2026); as said in the manuscript. However we are happy to add a figure below to illustrate this shift, through the comparison of a IASI and a MAJIS spectrum without any correction. The shift actually varies depending on the pixel position on the array. For the quantitative analysis, based on cross-correlating MAJIS spectra with 4A/OP and IASI ones, the referee is invited to read Langevin et al., 2026.



- The water-vapour explanation is a hypothesis, not established (see Major 3)

That was presented as an hypothesis, based on calculations done with 4A/OP for different amount of water vapor, as said in the submitted manuscript. In this the revised version, the effect of other sources of variability such as atmospheric temperature or changes in emissivity have been added.

- The % error figures and band-by-band claims should be revised (see Major 1).

We are not sure what the referee means by “band-by-band claims”.
We have revised our approach to quantify in more details the % error due to different factors (different emission angle, surface emissivity, meteorological conditions between IASI and MAJIS). We also show in new figures, in addition to MAJIS-IASI (convolved at MAJIS resolution) comparisons, another type of comparison: $(\text{observed} - \text{4A/OP})_{\text{IASI}} - (\text{observed} - \text{4A/OP})_{\text{MAJIS}}$, where the 4A/OP spectra were computed for the same emission angle as the corresponding observation, and using the native spectral resolution of each instrument. Indeed, this way the synthetic spectra with 4A/OP are computed with each instrument lineshape (ILS) function, and we avoid biases linked with convolving IASI (that has its own ILS, including an apodization) to MAJIS ILS. We believe the results are more convincing, and point to a bias of ~-7.5% for MAJIS data from C15. We also added a paragraph that estimates radiometric biases between the left and right edges of the detector array.

- Trace gases: retain the detection result but temper the retrieval-precision claims (

We have changed the conclusion but we feel like these precision values were already well tempered : they are not even mentioned in the abstract; they were already compared with natural variability of these gases (with a mention of non usefulness of CH4 for instance); and were clearly presented as lower bound uncertainty values (noise-limited) and were not described as retrieval-precision values in the text.